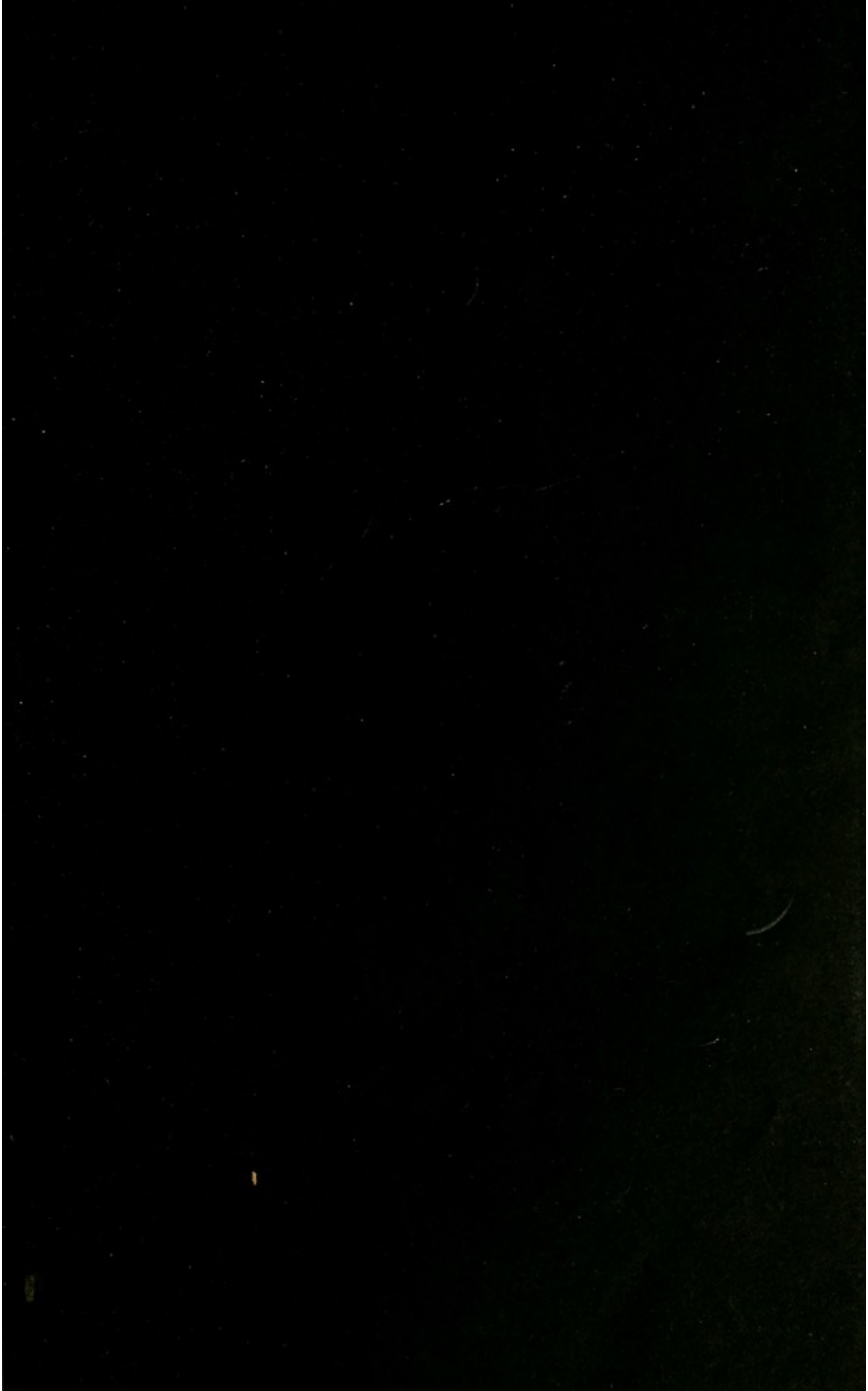
The prevention of consumption : a mode of prevention founded on a new theory of the nature of the tubercle-bacillus / by C. Candler.



THE PREVENTION OF CONSUMPTION

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A MODE OF PREVENTION FOUNDED ON A NEW THEORY OF THE NATURE OF THE TUBERCLE-BACILLUS.

BY

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PREFACE.

It is acknowledged on all hands that Koch's discovery of the tubercle-bacillus has been practically useless hitherto, save in the matter of diagnosis; and it is patent that no material addition to the etiology of phthisis has been made since the discovery was given to the world. So far as I can learn, no one has looked at the question from the point of view here taken up. The conception I have formed as to the botanical position of the bacillus I believe to be new. If it shall prove to be true, it will at once revolutionize the treatment of phthisis, and if it do not ultimately bring about the disappearance of the disease from civilized nations, it will inevitably lead to the immediate reduction of a large amount of tuberculosis amongst certain classes of their subjects. Given the truth of the main propositions herein contended for, it admits of easy demonstration that the prevention of ordinary pulmonary consumption, at all events, is well within the domain of practical hygiene.

As, therefore, the central view in this speculative inquiry is worth nothing, or is of extreme value, the argument has been set down without much regard to order or arrangement, inasmuch as elaboration would be waste of time.

The sooner the questions raised are disposed of one way or the other the better, and I take it to be of greater importance to submit them for consideration forthwith, in their present crude state, than to delay over the task of putting them into a more presentable shape. If there is nothing in the argument, its greater finish would be thrown away; if there is any pith in it, it will, though turned out roughly, hit the mark.

C. CANDLER.

London, 1887.

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INTRODUCTORY.

THE question of the *Prevention* of *Consumption* is taken up from the period of the brilliant discovery of the *Bacillus tuberculosis* by Koch, or rather from the period when the bacillus was admitted by the highest and most competent authorities to be the specific cause of phthisis. For it is thus possible to shut out a mass of historical and descriptive matter which, though of large interest, is not essential to the argument.

The present position of inquirers into this question is one of doubt and expectancy. No one can foretell how long we shall have to wait for the full fruition of the knowledge we have gained. All the bright promises held out that tuberculosis generally, and phthisis especially, would be brought under control have, so far, been followed up by somewhat meagre performances. The enormous stride taken towards prevention by the distinguished German micro-pathologist has left us yet a long way from the goal; that is, judging by practical results. The bulk of the faculty in Great Britain and her dependencies, as well as in America and other English-speaking communities, would seem to be in accord with regard to this matter. It is

palpable, indeed, that this large section of the profession fails to perceive that much substantial good has come, hitherto, of the determination of the infective agent in phthisis, and that it is unable to see that any further good can come of it until more is learnt of the precise nature and source of the infective agent, and of the ordinary means by which it invades the lungs efficiently, or so as to cause consumption. In point of fact, many of the leading English and Continental physicians, whilst gracefully acknowledging that Koch has vastly enriched our store of knowledge and furnished a new method of diagnosis in doubtful cases of consumption, have expressly stated that, in all that relates to treatment and prophylaxis, we are as yet no further advanced. Professor Jaccoud, however, in a lecture delivered at the Pitié, after intimating that the measures now employed in phthisis were already in use by himself and others prior to Koch's discovery, said: "Neither the pharmaceutical, the climatic, nor the hygienic treatment of phthisis has been influenced by the discovery of the bacillus. Its import is merely nosogenic. It simply adds another means of diagnosis, and that is the only service it has rendered, for, as regards the etiology, symptomatology, and treatment, it has done nothing and can do nothing" (*Medical Times*, May 17, 1884). There is something bitter here—a foreign element—a touch of feeling imported into an abstract question. The spirit which moved Professor Jaccoud to pronounce that the discovery of the bacillus *has done nothing and can do nothing* had manifestly upset his philosophy and clouded his judgment, for he can hardly have been blind to the fact that the bacillus must always play the leading part in the etiology of phthisis, and must therefore lead, eventually, to a more effective prophylaxis.

When Koch delivered his celebrated Address on Tuberculosis to the Physiological Society of Berlin, he wound up

by suggesting certain inferences that were to be drawn from his discovery. He pointed out some of their bearings upon sanitation, and foreshadowed the system for preventing the occurrence and spread of tuberculosis which has since been carried out in Berlin and Vienna, if not in some other capitals of Europe—though not, perhaps, in so thorough a fashion as in Vienna, where the "Doctoren-Collegium" would appear to have "bettered the instruction" of Koch.* That Continental physicians, however, concur in hygiene of this nature, or regard it with complacency, may easily be deduced from their acquiescence or silence. Professor Jaccoud, indeed, gives us positive evidence of his belief in thoroughness; for in the lecture at the Pitié already referred to, after insisting on the fact that he and others had employed such prophylactic measures as "ventilation, disinfection, separation of the phthisical from the healthy," etc., before Koch's discovery, he started a point in sanitation which was missed seemingly by the Viennese Doctoren-Collegium. I cannot give his own words, but in the epitome of his lecture in the *Medical Times* will be found the following:—"Prophylaxis of phthisis in families must commence from the very cradle; and so strongly does

* "The hygienic section of the Viennese Medical 'Doctoren-Collegium' has lately issued an elaborate series of regulations, framed with the view of stamping out the infectious disease generally known as tubercular phthisis, and familiarly termed in the Austrian capital, from its local ravages, 'the disease of Vienna.' The measures proposed in this document are indeed drastic, including as they do the *isolation from their relatives* of all those in whom pulmonary cavities or intestinal ulcers have become developed; the *prohibition of marriage* under certain circumstances, and a timely caution against the use of milk derived from cows which may be suffering from bovine tuberculosis! Precautions in the way of disinfection are detailed with great minuteness, but of all these prophylactic proceedings, one, more than all the rest, seems worthy of serious application, viz. the disinfection of tubercular sputa, the rationale of which really rests upon ascertained facts, and not upon unproven theory. We shall await with interest the result of these regulations in checking the occurrence of Vienna's arch-enemy, the tubercle bacillus."—*Medical Times*, May 17, 1884.

Professor Jaccoud believe this that he interdicts maternal suckling even when the mother is healthy and the father only the subject of phthisis, in the fear that the wife may contract the disease from the husband and contaminate the milk." On the whole, therefore, consumptives in some parts of Europe would seem to have been subjected of late to at least as vigorous measures as they were in former days.

In this country, the profession generally have looked askance at this hideous prophylaxis, though some physicians have shown a disposition to adopt it. The *Lancet*, however, though it had an acute perception of the rare worth of Koch's work, and led the van in handsomely acknowledging his grand discovery, suggested caution in drawing conclusions (*Lancet*, April 29, 1882); and the faculty here have remained so cautious that up to this time, whatever may be the views of some physicians, it has not been seriously contemplated to move the State to pass enactments to stamp out tuberculosis on the Viennese or any other Continental lines. Thanks, perhaps, to certain wary qualities and certain social considerations, the British schools have not been driven into accepting all the "logical consequences" of the proof of the bacillary cause of phthisis. Here and there, no doubt, one may come across an enthusiastic contagionist of strong convictions, who, if he had his way, would soon have coercive laws to eradicate phthisis and tuberculosis that should not disgrace the Doctoren-Collegium. However, the general conclusion that there must be something defective in the theory which has been the outcome of Koch's discovery—the theory that phthisis is invariably, or principally, caused by a transmission of the bacillus from person to person—has saved British communities hitherto. For the faculty as a whole have shrewdly elected to await further developments before

moving in the direction of State Hygiene for the prevention of phthisis; and, as the etiology of the disease has not been advanced a step since Koch announced the discovery of the bacillus, the question of prevention remains, in Great Britain and her dependencies, practically where it was before the discovery.

THE VIEW OF THE PURE PARASITISM OF THE BACILLUS TUBERCULOSIS.

THERE is a way by which the etiology of phthisis may be set in a new light, and by which the prevention of the malady to a large extent may possibly be ensured. Exact knowledge of the cause of the disease must obviously precede prevention. Fortunately, we now have that knowledge. Whatever diversity of opinion may exist in this matter beyond a certain point, it is established that the *Bacillus tuberculosis* is either the actual or the potential cause of the formation of tubercle. This has been determined on sufficient evidence, and I do not propose to go behind the evidence, or to challenge the soundness of the judgment. Koch's work up to this point has stood the test. It has been demonstrated that the bacillus is the infective agent in phthisis.

But, in giving to the world the results of his research, the great micro-pathologist also gave an inference from the facts he had brought out which, I venture to submit, was not warranted by them. I conceive that this inference was reached on insufficient grounds, and that it is an untenable inference. It is strange, perhaps, that the error, as I regard it, has not been detected and seized upon; but nevertheless I take it to be a cardinal error which has considerably reduced the value of Koch's investigation by delaying the perfect fruition of his discovery. The infer-

ence to which I attribute such unfortunate, and possibly to many thousands such disastrous, consequences is the one which Koch draws as to the botanical position of the bacillus. This conclusion, I shall endeavour to show, has obscured all etiological questions relating to phthisis, and thwarted every effort for its prevention.

As the argument mainly turns upon this question of the pure parasitism of the bacillus, we will give such extracts from Koch's celebrated Address as will be touched upon. They are taken from an excellent translation by Dr. W. D. Oakley, which appeared in the *Canada Medical and Surgical Journal,* and will serve for convenient reference. Towards the end of the Address, Koch says—

"The parasitic nature of the tubercular virus having thus been rendered certain, in order to complete our knowledge of this disease two questions must be answered—Where do the parasites originate; and how do they gain entrance into the body?

"With reference to the first question, it is necessary to decide whether the infectious material develops only under such conditions as are provided by the animal body, or whether, as can the splenic fever bacillus, it can pass through its development-process in any place free in nature.

"The results of several experiments showed that the tubercle bacillus can grow only at the temperature of 30° to 41° C. Under 30°, and at 42°, not the slightest growth took place during three weeks' time; while, on the other hand, the *Bacillus anthracis* grows actively between 20° and 42° or 43° C. On the ground of this one fact, the first question can be answered. In temperate zones, at no time does there occur at least two weeks of a continuous temperature of over 30°. It follows, therefore, that the tubercle bacillus in its process of development is limited to the animal body,

and is, moreover, not an accidental, but a pure parasite, and can only originate in an animal organism.

"The second question—How do the parasites enter the body?—must now be considered. The great majority of all cases of tuberculosis begin in the respiratory tract, and the infectious material first becomes apparent in the lungs or bronchial gland. It is therefore very probable that the bacilli are inhaled clinging to particles of dust. The way and manner in which they become mixed with the air is no longer mysterious, when we consider the quantity of bacilli in pulmonary cavities which, mixed with other contents, are expectorated and everywhere disseminated.

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"Moreover, such sputum after drying does not lose its virulence. . . . It can therefore be inferred that sputum which has been dried upon the ground, on the clothes, etc., retains its virulence for a long time, and, should it enter the lungs as dust, is capable of causing tuberculosis. It is probable that the tenacity of this virulence depends upon the spores of the bacilli, and it may here be stated that the spore-development of this bacillus takes place within the animal body, and not outside it, as in the case of the *Bacillus anthracis*.

* * * * *

"Let us now ask ourselves of what further importance are the results of these investigations? In the first place, it may be regarded as a triumph of science that it is now possible to produce complete proof of the parasitic nature of a human infectious disease; of the disease, indeed, of all others the most important.

* * * * *

"Clear rules for the warding off of phthisis have not been possible heretofore. But in the future struggle against

the dreadful plague of the human race, one will no longer have to contend with an indefinite something, but with an actual parasite, whose life-conditions are for the most part known, or can easily be still further investigated. That this parasitic organism only finds conditions suitable for its existence in the animal body, but cannot, as the *Bacillus anthracis*, outside of it, exist under ordinary natural conditions, bids us hope for results in battling against tuberculosis. Above all, so far as lies in man's power, must be closed those sources whence come the materials of infection, and of these sources certainly is the sputum of the already phthisical. Sufficient care has not heretofore been taken in the collection and removal in an injurious condition of such sputum, and there are no great difficulties to interfere with its proper disinfection, by which a great part of the infectious matter is rendered innocuous. Certainly great care must be bestowed upon the clothes, beds, etc., which have been used in the tuberculous.

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". . . When the conviction that tuberculosis is an exquisitely infectious disease has taken root amongst physicians, then will be fully elaborated, by means of discussion, the most efficient means of warfare under which to contend with our common enemy."

It is probably the air of absolute certainty with which Koch lays down the proposition that the bacillus is a pure parasite, that has deterred inquirers from examining into this botanical question. The inference, as set forth, seems to have all the force of a scientific law. Koch evidently considered it to be the only possible deduction from the facts. Hence the confident way in which he gives it, and then proceeds to point out the logical consequences as regards prevention. There is no doubt whatever implied in

his words—nothing to indicate that there was a bare chance even that the bacillus might be an accidental parasite. Its pure parasitism is spoken of as though it were matter of positive proof. This tone of decision and the general state of nescience as regards the pathophytes explain, perhaps, the omission of the etiological world to subject the matter to searching inquiry; for that it was an omission, and a serious one, will be seen on reflection.

I demur to the somewhat authoritatively pronounced dictum that it follows that the bacillus is a pure parasite from the one fact that it could not be got to grow outside the organism, except at blood-heat. The inference is by no means an inevitable sequence from the fact, for the fact does not take in enough to found such a weighty conclusion upon. It simply amounts to this—that the development-process of the bacillus did not occur at ordinary temperatures during the highly artificial cultivations required in Koch's investigation. And one has to suppose, from the inference drawn from it, that this fact was regarded as equivalent to proof that the bacillus cannot be cultivated at ordinary temperatures under any conceivable artificial conditions. It would seem to have been assumed that because it did not grow in one series of experiments, it was a certainty that it would not grow in any other series. But, as all possible combinations of soils and conditions were not exhausted in Koch's research, who shall say that some other investigator, or Koch himself, may not yet successfully cultivate this bacillus at ordinary temperatures? Again, even if it be granted that it shall be found impossible within the next ten or twenty years to cultivate the bacillus below blood-heat, it would not follow of necessity that there might not be an undiscovered means of growing it, as any one who knows the history of the common yeast-form will readily apprehend. Moreover, if this bacillus

should defy the efforts of man during all time to cultivate it below blood-heat, he may nevertheless have been contributing, undesignedly, to its growth, and it may have been growing luxuriantly all round him, in the flora of temperate countries.

Wherefore I submit that Koch's premiss in this matter does not sustain the conclusion, and that the whole question of the botanical position of the tubercle bacillus is an open one.

Most pathologists have so long regarded the pathophytes as autonymous plants that they have come, perhaps, to overlook, or to disregard, the fact that the question of the autonymy of any one of them is still undetermined, inasmuch as botanists have declined, or omitted, hitherto, to enter upon the systematic study of these forms. From the first, botanists as a body have contented themselves with sneering at the efforts of pathologists to connect pathophytic forms with pathological changes. It was to no purpose that men of repute showed conclusively, after careful investigation, that a certain specific form of vegetation was a constant in certain parts of the body in a certain disease, and that, inferentially, it was either the contagium, or the contagium-carrier. Instead of going to their assistance, mycologists were humorous and satirical on what they considered to be their apocryphal discoveries. They could not overlook or forget the many ridiculous mistakes of inexperienced inquirers, and could not, or would not, seriously entertain the view that the obscure forms met with in disease, forms of which they knew nothing and of the existence even of which they expressed strong doubts, caused the disease. They would have it that a large section of the medical world was possessed with a fanciful and an irrational theory as to the injurious effects on the human race of vegetable forms which were probably imaginary

forms, or, if really met with as described, were accidentally present and were perfectly harmless; at all events, there was no sufficient proof of their noxiousness. Language very like this is to be found in their works and periodicals long after the investigations of Pasteur, Obermeier, and Koch; and though their tone of contempt and incredulity is modified to some extent, yet even recently one of the greatest of mycologists, in a work of profound research in some branches of mycology, could not resist a fling at the imperfect biological and botanical work of the micro-pathologist. In short, botanists strenuously resisted the views of pathologists as long as they decently could, and then gave them a reluctant half-hearted recognition, and from first to last have left them to be worked out without special aid on their parts. At all events, we have not seen that any botanist has studied the life-cycles of Pasteur's microbes, of the *Spirillum Obermeieri*, of the *Bacillus anthracis*, of the *B. tuberculosis*, or of any other human pathophyte, in order to ascertain whether all or any of these forms are pure parasites or not; and if not, to connect them with their parent-forms, and to trace them to their ordinary matrices in nature.

It is both extraordinary and unfortunate that botanists, of all men, should not have had the penetration to perceive that, underlying the mass of botanical and biological rubbish heaped up on this subject, were potential germs of truth. For there was nothing in the view that vegetable parasites cause zymotic disease that was repugnant to what was known of parasitism. It had long been shown that insects of many kinds were attacked and killed by vegetable parasites, and that man himself was attacked by unmistakable forms of vegetation—notably in his skin. What, therefore, was there so preposterous in the notion that vegetation should invade the animal organism and cause

internal disease? The very confidence and pertinacity with which the pathologist went on with his inquiry might have awakened botanists to the suspicion that, after all, there might be something in it. A widespread belief, or a rooted conviction, which has taken hold of the minds of even a less educated class than pathologists, is not always to be despised by philosophers; and there could not be a more striking illustration of this than one furnished by botany itself. It was the persistence of farmers in maintaining that the presence of the barberry had something to do with rust in corn—an opinion which botanists had always scouted—that led De Bary to investigate the matter; and the investigation ended in the well-known brilliant discovery that the *Œcidium* forms were in organic continuity with the *Puccinia graminis*—as well as in showing that the farmers were right. So that, all things considered, it must always be matter for regret that botanists failed to take in the whole position of the pathologist, and that, seeing how Botany and Medicine overlapped, they did not hold out a willing and a helping hand to their fellow-workers. Could they have done this, the etiology of Infective Disease and Preventive Medicine might possibly have been advanced a quarter of a century.

It will be seen that pathologists who found specific phytic forms could find no orthodox system to place them in. Hallier's great Micrococcal Theory was utterly discredited. His unhappy discovery of the Cholera fungus on rice; his strange, and at that time startling, views of the morphoses of the fungi; his faulty microscopical observations; and his tendency to generalize on insufficient data, enabled botanists to tear his theory to shreds—although they have had to hark back somewhat of late.

The only systematic arrangement of pathophytes extant that pathologists could turn to was Cohn's, though most

mycologists looked upon his family of Schizophytes, with its division into tribes of Sphærobacteria, etc., and its subdivisions into genera and species, as a mere classification, without botanical interest or value, inasmuch as the forms are grouped without sufficient regard to their reproductive, as well as vegetative, characters. It was objected, with much force, that Cohn did not show (and it has not yet been shown) that the several pathophytes are each perfectly distinct, independent, or good species; and that, therefore, his arrangement of them, based as it is upon assumption, is purely arbitrary, and must be regarded simply as a provisional scheme which may serve, perhaps, as a convenient register until a systematic arrangement shall be possible—an arrangement resting on a knowledge of their morphoses and of their normal terminations of development in true, or sexually produced, spores. The life-cycles of the forms must be made out before they can legitimately be admitted into, and assigned their proper place in, a botanical system.

In the meanwhile, as pathologists could neither undertake this gigantic work nor await its completion by the botanists, but yet had to place their specific forms somewhere, they mostly placed them in the Schizophytæ. It was impossible, however, for micro-pathologists to continue their researches and cultivations long without getting involved in purely botanical questions. The question whether a particular form was only a *specific form*, or whether it was a *distinct species*, was one of these; and in Koch's work on *Traumatic Infective Disease* (New Sydenham Society, 1880) he discusses the subject of distinct species of bacteria, and protests against the opinions of Nageli and Brefeld, though, as he admits, from the pathological side of the question. He tries to shake off mycological trammels, and, discarding all morphological considerations, insists on regarding bacteria, etc., as *distinct species*, provisionally,

when they cause specific lesions in the body. And the exigencies of the case may perhaps have justified this mode of determining a species by its pathogenic effects in the animal organism, though it is no great wonder that botanists were aghast at the creation of a distinct species out of a specific form without knowing whether it was merely a morphosis-form or not, and at the creation of it on pathological grounds alone.

Still, no solid advance has been made to this day. Flügge and Zopf and Klein have of late refined on Cohn, and the subdivisions of micro-organisms are becoming a serious tax on the memory, and singularly irksome, because the whole terminology must be unlearnt when a true botanical system shall supersede the existing arrangement. There is no help for it, however, and micro-pathologists must be content, for the sake of precision, to burden themselves with all the nomenclature connected with series after series of fictitious genera and species.

We now come to the question whether the *Bacillus tuberculosis* is or is not a distinct species; that is to say, whether this bacillus is a stable form into which the spores invariably develop under all conditions, or whether, whilst in the animal organism, it is a morphosis-form, genetically connected with other forms either in or out of the organism. Koch, as we have seen in his Address, has disposed of this knotty question in words which imply absolute certainty. He has authoritatively determined that the bacillus is a pure parasite, and, by implication, that it is a distinct species. But when we find that the determination rests solely on the ground of the one fact—namely, that the tubercle bacillus did not grow, in his cultivations, on certain artificial soils under certain (not fully specified) conditions, below a certain degree of temperature, we have to ask ourselves whether this fact is able to bear the strain put upon

it; whether it warranted the conclusion upon which has depended such grave consequences. For this conclusion is not merely interesting scientifically. It entailed the adoption of the practical methods for the prevention of phthisis which Koch suggested on the strength of it; and thus it has influenced for good or for evil the sanitary and prophylactic measures, *re* consumption, of nearly the whole of Continental Europe. And on the soundness or unsoundness of the conclusion yet hang innumerable unsettled questions of vital import to all English-speaking communities. It is difficult, indeed, to realize the far-reaching results of the fact that Koch failed to cultivate the bacillus below the temperature of 30° C.

That the soundness of Koch's inference that this bacillus is a pure parasite has not been submitted to all known tests—the more especially seeing that it involved abstruse questions in botany, and was reached from one fact ascertained in an investigation specially directed to pathology—is remarkable. That it should have passed current as sterling coin without ringing, and should have been accepted with passive and universal acquiescence, is almost incomprehensible. For Koch has been criticized sharply enough in many things. He has not been spared on occasion; but on this point, a most vital one, and the key of his position in regard to prevention, he has not been touched. Judgment, however, must not go by default. The whole case relating to the parasitism of the bacillus has yet to be heard. And there must be an appeal *in foro scientiæ*. The case is that the one fact brought forward by Koch (viz. that the bacillus did not grow in his cultivation-processes except at blood-heat) is insufficient either to have warranted then, or to uphold now, the inference he reached from it; and that the opposite conclusion to the one he arrived at from this datum—namely,

that the bacillus is an accidental parasite—may be drawn from a large body of facts. It has to be observed that in the *Lancet* of March 15, 1884, is an abstract of Koch's paper on the Etiology of Tuberculosis in the Reports of the Imperial Board of Health (Berlin), 1884, in which Koch repeats his view of the pure parasitism of the bacillus —two years after the first announcement of it.

Some of the objections to Koch's inference I will state, briefly, at once; others will be seen further on. (1) After Koch succeeded in growing the bacillus on the previously heated, sterilized, jelly-like serum from beeves' or sheep's blood, placed in a constant temperature of 37° or 38° C., he did not, as it would appear, make any further experiments for the express purpose of ascertaining whether the bacillus would grow at lower temperatures on the same material under different conditions, or on other substances under varied conditions. He certainly says, "Other substances having similar properties can be used as well as stiffened serum to nourish the tubercle bacilli. For example, a sort of jelly prepared with agar-agar, which remains hard at breeding temperatures, and to which some meat infusion and peptone has been added. On this, however, they do not grow in such characteristic forms as on blood serum." But there is nothing to indicate that Koch tried to grow the bacillus at lower temperatures on every conceivable soil under all conceivable combinations of conditions. Indeed, it is apparent that all the cultures in this research had for their primary object the demonstration that the bacillus is the infective agent in tuberculosis; and that the pure parasitism of the bacillus was of secondary consideration—if not an after-thought which it was not deemed necessary to verify by further experiment. (2) Mention is not made of the degree of moisture in the air of the flasks. (3) The conditions as regards light

are not referred to. (4) The vegetative and reproductive life of the bacillus was not investigated specially, but was merely observed incidentally. (5) The fact that there were many generations of the bacillus on such a substance as the stiffened jelly used in Koch's laboratory suggests greater tenacity of life than would be looked for in a pure parasite detached for months from its host. (6) The creation of a species out of the specific bacillus-form is at least premature. (7) It is an inference from a large body of facts that the bacillus in the lungs generally dies out in a certain time—within a shorter period than that during which it has been found to retain its vitality and its infective power on stiffened serum—or else becomes encapsuled. (8) The explanation of the occurrence of tuberculosis generally, and of phthisis specially, by the agency of the bacillus-spore in dried sputum, is altogether insufficient. (9) Many phenomena connected with the occurrence of phthisis furnish strong presumptive proof that the bacillus (or its parent-form) "can pass through its development-process in" some places "free in nature," and exists "under ordinary natural conditions;" or, in other words, that it is an "accidental," and not a "pure," parasite, and can "originate" out of "an animal organism."

The objections to Koch's inference need not be discussed here at length. It is to be regretted that, in electing to step out of his own realm of pathology, he should not have taken his method and caution with him. When he propounded to himself the two questions, "Where do the parasites originate; and how do they gain entrance into the body?" he might have tested what appeared to him to be a sequence by every means known to mycologists, and by such others as suggested themselves to him, and then have given results; or, if time was pressing, he might legitimately have put as an inference of his own that which he laid down as a forced

conclusion or a certainty. However, passing over this now, it will be palpable to those who reflect upon the subject that, owing no doubt to the fact that Koch's cultivations were exclusively devoted to the pathological aspect of his research, they were singularly few in the matter of variety. So far as one may gather from his Address, the soils he employed were very unlike the soils, or soil, upon which it might be conceived the bacillus would grow—if it does grow—in nature at ordinary temperatures. That Koch failed to grow it, therefore, by his processes below blood-heat, though unprecedented in research, by no means shuts out the supposition that it might be got to grow below that temperature on a more congenial soil placed under favouring conditions. For the niceness of parasitic forms as to soil is notorious. It is seen in each one of them selecting or affecting certain, and generally different, tissues to colonize on. Koch's well-known instance of a seemingly capricious form which distinguishes between a house and a field mouse is a curious case in point. Though the explanation may lie, as he suggests, in the difference he found in the blood of the animals, yet who could have foreseen that so slight a variation in soil would have determined the life or death of the parasite? A micrococcus shall be found to flourish on ammonium lactate, and perish on ammonium butyrate; but we need not multiply instances of a vegetal form taking this food and rejecting that, when both foods are seemingly alike, or closely allied in character. Yet the cases in which this apparent fastidiousness, or daintiness, has been brought to light must be few indeed as compared with the number of unknown and unsuspected instances in which the lower forms are restricted or prohibited in their growth by the obscure and "erratic" laws of their nutrition.

The bacillus of tubercle has been observed to be as choice as regards soil as other parasitic forms. Dr. Watson

Cheyne, in his Report on the relation of micro-organisms to tuberculosis (*The Practitioner*, April, 1883), says :—"That certain organs and tissues are better soil than others for the growth of parasites is well known, and can easily be shown with regard to tuberculosis; the apices of the lungs are better soil than the bases, epithelioid cells are better soil for the bacillus than granulation cells or cheesy matter, and so on. Now, it does not necessarily follow that the quality of the soil is the same in man and animals. In cattle the bacilli prefer the surfaces of the serous membranes to an extent which they only very rarely do in man. In man, again, the pia mater is a common seat of tubercles in acute tuberculosis; in rabbits and guinea-pigs the presence of tubercles in the pia mater is, I believe, unknown—certainly they have not been found in my experience. Other facts might be mentioned, but what I have said is sufficient evidence of the difference between tuberculosis in man and rabbits, dependent partly on the mode of infection, but chiefly on difference in soil."

This account of the bacillus favours the view that if Koch had, by accident, hit off a soil more nearly allied to its (assumed) natural soil, he might have cultivated it successfully without keeping it in a temperature of blood-heat. One of the most favourable of soils for the growth of vegetable parasites in animal organisms would appear to be the excreta of man and other hosts, but no mention is made of such soils in Koch's research. Of course, I do not overlook the fact that for his purposes he required a pure cultivation of the bacillus. But this very necessity for isolating a form may be the means of depriving it of a large proportion, if not all, of that kind of nourishment which is essential to its higher sexual development; and it is not to be overlooked that, as there are no pure cultivations in nature, an admixture of forms may be necessary to the development

of one or more of those present in a fungus-field. Possibly the tubercle bacillus exacts commensalism as a condition of its perfect growth at ordinary temperatures. At all events, it is quite conceivable, if not a warrantable supposition, from what is known, that some forms which grow readily enough on congenial soils at ordinary temperatures would linger or die out on less congenial soils at ordinary temperatures, though they might grow on these latter soils with the stimulus of artificial heat, even if they failed to develop into all their pleomorphic forms.

Koch's graphic account of the mode of growth of the bacilli is highly suggestive of a starveling vegetation, just succeeding in the struggle for existence, and requiring constant transplantation as soon as the little nourishment it can extract from the surface of the stiffened serum is exhausted. He says that "the growth of these colonies" (of bacilli) "advances gradually during three or four weeks. They grow larger, from smooth masses generally somewhat smaller than a millet seed, which lie loosely on the surface of the jelly, but never penetrate into it independently, or cause it to become fluid. . . . The growth of the colonies, as mentioned above, ended after a few weeks, and further increase does not occur, apparently because the bacilli are deficient in all power of independent motion; they spread out on the jelly surface merely by the pressure of growth, and, consequently, these slow-growing cultures reach only slight dimensions. In order to keep such a culture progressing, some of the bacilli must, some time after planting —from ten to fourteen days—be conveyed to new jelly. . . . Then, as before, fresh scaly masses make their appearance. . . . A sort of jelly prepared with agar-agar," can be used. "On this, however, they do not grow in such characteristic forms as on blood serum."

Even on these closely allied artificial soils, therefore,

Koch found some differentiation in the development-processes of the bacillus forms—a modification in the character of the vegetation—an indication of a tendency to morphose; and, convenient and admirably designed for their special object as these cultures were, it is yet patent that they were singularly deficient from the botanical point of view. It may rationally be supposed that the bacilli wanted something they did not find on the sterilized and stiffened jelly, on which to have luxuriated and possibly to have developed into other forms; and a deal of conscientious work has to be done before it can be determined whether the bacillus can or can not grow in nature at ordinary temperatures.

There may be another explanation of the slow growth and spread of the bacilli on serum than the one given by Koch that they are "deficient in all power of independent motion," and spread out "merely by the pressure of growth." Fluid cultivation of the bacillus may not advance the question of pure or accidental parasitism much, but some observations made by Dr. Williams * are significant on the question of the independent motion of bacilli and as to the nice requirements of the forms, and are otherwise interesting biologically. In *syrup*, Dr. Williams found that the bacilli diminished in forty-eight hours, and gradually disappeared. In *hay infusion* they were present for twenty-four hours, and then disappeared entirely. In *distilled water* there was no growth or multiplication. In *Pasteur's solution* they diminished at first, then remained stationary for some days, but "they showed signs of increase on the eighth day." In *pork broth* their numbers did not increase, "but rather declined, though they did not totally disappear from the solution." In a strong *beef solution* they in-

* "Observations on the Influence of certain Culture Fluids and Medicinal Reagents in the Growth and Development of the *Bacillus tuberculosis*." By Dr. C. Theodore Williams. *Proceedings of the Royal Society*, January 24, 1884.

creased enormously, both in numbers and in length. "The bacilli were most abundant in the neighbourhood of the meat *débris*, round which they swarmed like ants on sugar. . . . About the seventh or eighth day they were seen increasing by division." (The *Bacterium termo* and other bacilli were also present). A weaker *beef solution* gave similar but less decided results. The marked preference of the bacilli for beef solution over both Pasteur's solution and pork broth affords another curious instance of the difference these low forms may find in the nutritive characters of very similar substances; whilst the increase in length and the swarming round the *débris* not only indicate such an indication to change as might lead the vegetation, if placed on the most suitable soils under the most favourable conditions, to develop into other than bacillus-forms, but also a decided "power of independent motion" in the bacilli.

The conditions of moisture and light in Koch's culture are not referred to. From the hard scaly growth of the bacilli on serum we may perhaps infer a rather dry heat in the flasks—a non-natural condition for the growth of a pure parasite. Yet, as a certain amount of moisture in the incubator may be assumed, the question of the intensity of the chemical light admitted to the breeding surfaces is of the greater importance—of the highest importance, indeed. Solar radiation having a given chemical power will be injurious or destructive to most of these low forms, whilst it may be necessary to the existence of a very few. It will be seen further on that I infer from numerous phenomena that the sun has an inimical effect upon the bacillus-vegetation. Here I would point out that Messrs. Downes and Blunt submitted infusions containing bacteria, etc., to the influence of sunlight, and gave the results in two most

interesting and suggestive papers to the Royal Society.* Professor Tyndall at first suggested objections to these observations, but afterwards substantially confirmed them in a Report to the British Association.† It would take us too far out of our way to refer to the research of Messrs. Downes and Blunt at length, but a few quotations will enable me to indicate the nature of that portion of their work which specially applies to our subject. The main result of their research was that all bacteria contained in meat infusions, etc., were destroyed by a certain amount of exposure to sunlight, and that the infusions were sterilized. They say, "It is important to note, however, that in Observation 3, the germs of a fungus had apparently survived an amount of insolation which was fatal to the development of *Bacteria*. . . . We have found that when *Bacteria* appear early and in large numbers in the solution we have used, the mycelium of Penicillium or other microscopic fungus is rarely seen, the Bacteria apparently preoccupying the ground; when, however, the development of the *Bacteria* is from some cause retarded or prevented, we have frequently found tufts of delicate mycelium submerged in our experimental solution, after they have been encased or removed into diffused light. . . . We infer, accordingly, that light may retard or altogether prevent the appearance of mycelial fungi, but that its influence in this respect is slower and less powerful than upon the *Schizomycetes*. . . . May we not expect that laws similar to those which have manifested themselves may be in operation throughout the vegetable, and perhaps also the animal,

* "Researches on the Effect of Light upon Bacteria and other Organisms." By Arthur Downes, M.D., and Thomas P. Blunt, M.A., Oxon. *Proceedings of the Royal Society*, 1877. "On the Influence of Light upon Protoplasm." 1878.

† "On the Arrestation of Infusorial Life." Professor Tyndall. *Nature*, September 15, 1881.

kingdom wherever light has direct access to protoplasm? . . . (1) Light is inimical to, and under favourable conditions may wholly prevent, the development of these organisms. . . . (2) The fitness of the cultivation-fluid is not impaired by insolation. . . . We showed in our former communication that by increasing the density of our cultivation-liquid the development of *Bacteria* could be proportionably delayed. In this way we have been able to accentuate the difference in the behaviour of the solution under varying conditions of light. . . . That the action is chiefly dependent on the blue and violet rays is shown, etc. . . . It happens that during portions of this time we had insolated tubes, containing ordinary Pasteur's solution, with the result that all bacterial development was prevented by a few days' exposure to the sun. . . . We conclude, therefore, both from analogy and from direct experiment, that the observed action on these organisms is not dependent on light *per se*, but that the presence of free oxygen is necessary; light and oxygen together accomplishing what neither can do alone: . . . we think that protoplasm will be found to possess varying degrees of tolerance of excess or deficiency of this element" (oxygen). "To some forms of life, if Pasteur be right, oxygen is injurious even when diluted as in ordinary air; to others it is hurtful only when oxidation is quickened by some adjuvant force, as, for example, by light. Finally, since light here acts as an oxidizer, it is conceivable that there may exist sluggish forms of protoplasm, whose oxidizing processes, and therefore general growth and development, may be *favourably* augmented by a modified degree of light. We are not, of our present knowledge, however, able to point to such. . . . "

These mutilated fragments of the valuable researches of Messrs. Downes and Blunt will serve to show the immense

importance of light in this connection. Although their observations were confined to liquids, the results of course apply, *mutatis mutandis,* to low forms of vegetation on solids. As we are not told whether Koch took solar light into account in cultivating tubercle bacilli, either on solids or in liquids, all that need be said here is that, if he did not, the chemical effects of the sun may have been one of the disturbing elements which precluded the growth of the vegetation in his flasks below blood-heat.

The tenacity of the vegetative life of the bacillus outside the animal organism—in the cultivation-processes of the micro-pathologist—points significantly to the inference that the form may also have a free existence somewhere in nature. Here we find a so-called pure parasite—an anærobiotic form—which, when removed from its host, survives under non-natural conditions (on stiffened serum), vegetates ærobiotically for months, and yields generation after generation without any sign of diminution of vegetative vitality (if provided with food by transplanting it), or of pathogenic power, so far as traumatic experiments are concerned. Dr. Watson Cheyne [*op. cit.*] says, "The serum in the tubes may be allowed to dry up, as occurred in one of my tubes, but still the bacilli grow when transferred to fresh serum, and produce tuberculosis when inoculated into an animal." The fact that a form of this kind, the spore-development of which, as Koch states, "*takes place within the animal body, and not outside it,*" clings to life in this marvellous way on dried-up jelly, and then starts again into active growth when transferred to fresh serum (without any spores to carry it on, be it observed, and solely by vegetative processes), shows that this parasite has, at all events, a great facility in adapting itself to decidedly foreign, and apparently adverse, conditions. And although little enough is known of this class of vegetation, this behaviour of the

bacillus would appear to stamp it as an accidental parasite. With regard to sporulation, by the way, Dr. Theodore Williams, in the paper before referred to, has some observations connected with the spore-development of the bacillus which seem to clash with Koch's statement that the formation of spores is confined to the animal body. For instance, in the presence of *boracic acid*, it appears there was an abundant increase of the bacillus. Dr. Williams says, "Rods in couples, arranged at an angle of 45°, were common. They were of fair length, and contained many spores. Multiplication by division was going on." Except the spores were contained in those bacilli only which were taken from the host, it is not easy to reconcile this experiment with Koch's results, unless it be assumed that, although he used culture-fluids repeatedly, he missed these instances in which the sporulation of the bacillus occurs outside the body.

Direct evidence is furnished on all sides that the parasitism of the bacillus in the animal organism is generally of the feeblest description (until or unless it gets into the blood or lymph streams); but strong presumptive evidence will be given further on that it perishes outright, naturally, in certain changed conditions of the host, in a few weeks, even when present in the lungs in considerable numbers; or that it becomes encysted, or encapsuled, in such wise as to be "quiescent," or practically non-parasitic for the time being. It need hardly be observed that if these terminations to the vegetative life of the bacillus always occur in the body, they furnish an argument of some force against the assumption of its pure parasitism.

As Koch's research connected with the vegetative life of the bacillus was merely ancillary to the working out of his great pathological problem, I submit that his observations were too restricted to be of material use in determining the large moot botanical question whether the bacillus is

a distinct species (in the mycological, not pathological, sense), or only a specific form. Koch, indeed, has waived, or set aside, all considerations arising out of pleomorphism and all doubts engendered by the recent upheavals and derangements of former botanical views and doctrines, and has determined that the bacillus is a distinct, good, autonymous species. As he is, apparently, a law unto himself, he has pronounced that the life-cycle of the bacillus ranges from spore to bacillus, and from bacillus to spore; and that it enters into no other phase and takes no other shape. This authoritative way of disposing of the point simplifies matters extremely, but it does not set them at rest.

The *Medical Times* (January 31, 1885), in an article on *Bacillary Phthisis*, observes: "Our efforts must, then, be directed to the study of the life-history of the bacillus itself. We must learn what it best and least thrives upon, how it is propagated, how it travels from place to place, from organ to organ, . . . indeed, everything that concerns its life-history;" and then announces that Professor Koch and a school of ardent admirers "are hard at work in Germany on these lines." I will not hazard anything as to the results of this investigation, which I sincerely trust has been entered upon. There can be no doubt about the wisdom of the step; and it is to be regretted that it could not have been taken long since—even before the important inference as to the parasitism of the bacillus was arrived at. Whatever may be the outcome of future research in this direction—whether it upsets or confirms Koch's inference—as the case now stands, I say without hesitation, that instead of its being a forced conclusion from his premiss that the bacillus is a pure parasite, the only thing that follows from the premiss is that the bacillus did not grow below blood-heat by his modes of cultivation.

The arguments which, perhaps, are the most cogent of

all that can be brought at the present time against the view of pure parasitism, are those to be found in the facts connected with the incidence of phthisis. Many of the phenomena, however, undoubtedly lend themselves to the theory of pure parasitism. They are seemingly in perfect accord with the view of an invariable transmission of the bacilli from one animal organism to another. In some instances, the chain of evidence by which it has been shown how the infection has gotten from the infector to the infected is apparently so strong that it looks impossible to break it; and there is no gainsaying that in these instances the theory advanced by Koch, which grafts easily and naturally on the old doctrine of contagion, affords, in the existing state of our knowledge, a rational, and, to some minds, the only, interpretation of the phenomena. On the other hand, many of the facts observed in connection with the occurrence of the disease are utterly irreconcileable with the theory. They cannot be made to fit in with it by any possible means. The explanation will not explain them. Although there is no question as to their bacillary origin, yet these modes of the occurrence of certain cases of consumption cannot be got to accommodate themselves to the view of pure parasitism. And it is this defective accommodation which has, no doubt, led a section of the British school to distrust the view: for the old canon still survives—that unless a theory will take in every fact, it is unsound. And large groups of facts are absolutely repugnant to the view of the transmission of phthisis from person to person by way of bacillus-spores. The incidence of the disease in certain instances is a positive bar to the supposition that contagion is concerned in these occurrences. In such cases there is no presumable mode of connecting the bacilli in the lungs of patients with bacilli derived from other patients. The transmission of

the forms from one consumptive to another cannot be made out so as to show that the forms from the one have been the efficient means of infecting the other. The facts and reasons for these statements, or assertions, will be given before the case against pure parasitism is completed.

THE VIEW OF THE ACCIDENTAL PARASITISM OF THE BACILLUS TUBERCULOSIS.

When Koch made known his grand discovery, and it became the common property of the etiological world, it was of course open to accept, or reject, his facts and his inferences, either wholly or in part, according to the nature of the evidence he produced, or according to the weight of the evidence in the estimation of the different schools. Indeed, at the present day, although most sections of the profession acknowledge with acclaim that he has determined the specific agency of the bacillus in tuberculosis, yet some sections differ widely from his views on many points connected with the dissemination of tuberculosis. And I have ventured, whilst accepting his facts with the highest admiration for his pathological research, to challenge his conclusion from his subordinate but yet important botanical work. I cannot accept such a large inference, even from one so distinguished, upon what would appear to be slight grounds; and, after giving the subject much thought, I have reached the opposite inference to his—the one he rejected—namely, that the bacillus is an accidental parasite. The issue will have to be tried sooner or later on its abstract merits.

If we pass from the botanical side of this question, where so little is really known, to the clinical and other aspects of it, we shall soon find definite and cogent reasons

for concluding that the bacillus-vegetation grows somewhere free in nature, or in or about the habitations of man. Koch, electing rather, it would seem, to trust to experiments designed by himself, and carried out in his own laboratory, has declined, or omitted, hitherto, to go out into the wider field as well and take a larger survey. But I conceive that the history of phthisis, carefully studied, enables the inquirer to lift the whole subject of the cause and prevention of the disease bodily out of the slough into which it is fast settling by being encumbered with the dead weight of pure parasitism. For there is no use in mincing matters, or shutting one's eyes: the glory of the bacillus is departing—for a space. The hopes at first raised by Koch's words were dashed long ago, and there are signs of a reaction which threatens to dim the lustre and diminish the value of his research. Etiologists are beginning to ask, not only whether a discovery which has led to nothing is worth much, but whether it is a discovery after all. One learns from the Paris correspondent of the *Lancet* (*Lancet*, December 18, 1886) that Professor Peter aired some curious views in his Inaugural Lecture at the Hôpital Necker. "M. Peter," the correspondent says, "then alluded to Virchow's magnificent conceptions in cellular pathology, . . . and the final overthrow of these views by the discovery of the bacillus. For a time Koch was triumphant, but the supreme reign of the bacillus had not been long. It has been shown that the extent of the disease does not coincide with the quantity of the bacilli, and the partisans of these micro-organisms now say, 'It is not the bacillus that makes the disease; it causes it only indirectly by the secretion of a ptomaine.' At the present time the whole of pathology is dominated by pathogeny, and this by bacteriology. Bacteria here, bacteria there, bacteria everywhere. Every day we learn with stupefaction that yet

another disease is parasitical." Light froth of this kind may sully, but cannot permanently affect, genuine work—such as the demonstration of the bacillus. Yet Professor Peter is the exponent of a certain class which is not altogether displeased either at Koch's discomfiture, or with the failure of his discovery in regard to protective measures; and of another larger class, which is beginning to have serious doubts about "bacteriology," seeing that it has not led to any appreciable diminution of infective disease. Both classes put together will not, either by wit or argument, stem the tide of inquiry, but we shall probably see a small wave of hostile opinion that may do some mischief by interruption and delay.

The contention here is that Koch is right as to the bacillus-vegetation being the specific infective agent in phthisis, but wrong as to the nature of the parasite; and, further, that by regarding the bacillus as an accidental, in place of a pure, parasite, the enormous importance of his discovery may then be seen: for a perfect scheme for the certain prevention of phthisis in civilized communities suggests itself. Perhaps the readiest way to convey my views will be by the aid of an assumption. Let it be assumed that the bacillus is an accidental parasite. Granted this postulate, we may, even with present materials, construct an hypothesis which shall not only explain all those phenomena in the occurrence of phthisis which have hitherto been inexplicable, but will show that those phenomena which have apparently been fully explained by contagion have another and, as I conceive, a more rational explanation. So far as I have been able to discover, there is not a fact recorded in the history of phthisis, however obscure or erratic, which is not reducible to plain natural laws by means of this hypothesis. If, indeed, there be found one established fact that can be shown to be at variance with

its fundamental principles—to be destructive to any of its essential parts—it will be as fatal to this hypothesis as many well-observed facts are to the view of pure parasitism.

Should the assumption be shown hereafter to have been correctly evolved, it will follow that the occurrence of this fell malady of consumption can be controlled, at will, in most States. It will be in the power of these States to reduce or regulate the sum of phthisis in their territories in exact proportion to the means they are disposed to employ; and those means do not include any one of the harsh and even cruel measures that have been taken in some countries, or any of those which it has been suggested should be carried out in our own country. The scientific affirmation of the assumption that the bacillus is an accidental parasite would at once relieve hygiene and prophylaxis from all uncertainty as to aim. Empiricism in this matter, now supreme, would no longer misrule; and the physician, with a clear, definite object before him, could act confidently and promptly, and without the haunting suspicion that his careful and conscientious efforts may, after all, have been misdirected. Whatever might be the result elsewhere, I have a firm conviction that, if it were established by recognized competent authorities to the satisfaction of the great bulk of the faculty that the tubercle bacillus grows outside the body at ordinary temperatures, the sum of mortality from consumption in this country would be reduced by one-half in five years; and that the disease could, and probably would, be rooted out in fifteen or twenty years. But all this is premature and purely hypothetical.

In its (assumed) free existence, the tubercle-plant either grows on its matrix or matrices as a bacillus, and enters the body as a bacillus, or as the spore of a bacillus, and its

whole cycle from spore to spore is limited to the bacillary form; or, as I infer, it has a higher or parent-form outside the body, the fruit of which, after entrance into and incarceration within the body, may germinate, and will then develop into bacilli; and when these bacilli escape, or are expelled, their spores may (under favouring conditions) revert to the parent-form, though the point—of great importance from some aspects—need not be discussed here. Nor is it material to the issue immediately before us which of the two views of the assumed free bacillus-vegetation presented is for the time being adopted. The specific questions to be dealt with now are those which Koch put: "*Where do the parasites originate; and how do they gain entrance into the body?*"

The etiologist who, having formed the conception that the bacillus-vegetation grows and propagates out of the body, should set himself to answer these vital questions, would naturally turn to the environments of consumptives —their homes and occupations. He would look more especially to the surroundings of those classes which suffer more from phthisis than other classes, with a view to discover the whereabouts of the matrix or matrices that support the vegetation by the products of which they are infected. Where the sum of the disease has been sensibly reduced in a class, either generally or locally, he would try to elicit the causes which have brought about a diminution of the vegetation in the vicinity of the class, or have lessened the number of the spores, or the quantity of the fruit, which finds entrance into all or some members of the class. He would examine into the sources of the differentiation through which some of the inmates of particular houses die of consumption, and the other inmates remain free from all signs of tubercle. He would investigate with especial care the history of the families known as

"consumptive" and "scrophulous." In short, he would have to analyze the multitudinous phenomena connected with consumption, so as to evolve suitable matrices for the bacillus-vegetation in such positions and under such conditions as to account for the occurrence, and, in certain cases, even for the non-occurrence, of the disease. The etiologist would call in to his aid the pathology, the symptomatology, and the treatment of phthisis. The past and present prophylaxis would command his attentive consideration; for, though as empirical now as it was formerly, modern efforts to ward off the disease have been far more effective than were the ancient measures. He would inquire into exercise, open air, sea-voyages, health resorts, etc., etc., with a view to determine how much of their efficacy in treatment is due to purely tonic effects, and how much to reduction in the amount of the specific infection. In this part of his inquiry he would be attracted, no doubt, by the importance attached by most physicians to the subject of light and sunshine in treatment. This thread runs through the works of all writers from the earliest times. He would know, having the key, that the empiricism here is pregnant with some law by which, when it shall be possible to educe it, the growth of the bacillus-vegetation may be governed. He might be baffled for a time in his attempts to make out precisely how the solar ray is involved here, and then, when he should come to apprehend the actual nature of the relations of the sun to the bacillus, he would find himself impeded in many ways by the limited knowledge we have of the chemical power of solar radiation. He would find it impossible to arrive at exact results here, because of the nescience (unavoidable) of physicists in this matter; and he would have to fall back on his own resources, and get as nearly as he could to correct approximations. Many of his conclusions are thrown into the following proposition.

It is submitted :—that the bacillus of tubercle, or its parent-form, exists in nature in all known regions, except those which are covered with perpetual snow, or are at a certain altitude; that it occurs on the surface of certain descriptions of organic matter under certain conditions; that its matrices are widely distributed outside the habitations of man, but that the principal matrices upon which it grows at the time when it becomes the cause of human phthisis are what are commonly known as "dust" and "dirt" in dwellings, and especially the organic matters contributed thereto by the breath and exhalations of the inmates; that these matrices adhere to, or are deposited on, the walls, floors, and projections, and in the cracks, crevices, holes, and recesses, and behind wainscotting and heavy or fixed pieces of furniture, and below the flooring boards of rooms, workshops, cells, dormitories, cellars, underground apartments, mines, or any confined space occupied by human beings; that among the conditions required for the growth of the vegetation are a certain degree of temperature, a certain quantity of moisture, and a certain amount of solar light; that these conditions concur in a certain proportion of rooms, workshops, etc., and more favourably for the growth of the vegetation at certain periods or seasons; that the spores or fruit of the vegetation are suspended in the air of the rooms, etc., as a miasm, or malaria, especially at night; that a certain proportion of the spores or fruit inhaled with the breath may be retained in the air-passages or alveoli of the lungs, though they may not germinate there, or become parasitic, or may not induce tuberculosis; that the potency or infective power of a local bacillary malaria is in a ratio to the extent or quality of the matrix-field, and to the conditions favouring the vegetation, or, in other words, to the number and value of the spores or fruit contained in a given volume of the air inspired; that

frequent or long-continued exposure of persons to the malaria during a certain period of time may end in the vegetation becoming parasitic in their lungs, and in such numbers, or with such potency, as to constitute an efficient infection, or to cause phthisis; that the length of time required for efficient infection varies, or may vary, in different persons apparently equally exposed to the same bacillary miasm, or to miasms of equal potency; that the bacilli, or their spores, when discharged from the host, will grow, as has been shown experimentally, outside the animal organism, on the surface of certain artificial soils, at a temperature of blood-heat, and may also grow under exceptionally favouring conditions outside the organism, on soils more nearly allied to their natural matrices, at ordinary temperatures; that the bacilli or their spores contained in dry sputum, when introduced into the blood or lymph streams, induce tuberculosis, but when inhaled by the breath as "sputum-dust" do not cause phthisis; and that although in certain rare contingencies the discharges of the phthisical may have been, and may yet be, the means of starting a bacillus-vegetation on a matrix-field previously or hitherto uninvaded by the form, yet, seeing the wide distribution of the vegetation in the natural flora and in and about dwellings, etc., it is in the highest degree improbable that the phthisical invalid contributes, or has for many centuries contributed, directly or indirectly, to the general or to any local malaria, in the settled countries of Europe, such an amount of bacillus-vegetation as to make, or to have made, any material or appreciable addition to the sum, or to the infective power, of the local bacillary miasms of a country.

Many points of interest are left out of this proposition, but some of them will be taken up in the course of the argument. The critic may find, too, that the proposition,

made up as it is of conclusions from numerous data, should have come after the facts: but, as this would have entailed much repetition, I have discarded method to economize space. Before going to the facts, a few remarks may relieve some of the subjects in the proposition of their baldness or obscurity.

It will have been observed that a distinction between the bacillus-vegetation occurring naturally in the open and that fostered on made soils in dwellings, is implied. From the occurrence of tuberculosis in cattle, it is an inference that the natural habitats of the vegetation are to be looked for in deep ravines, shady forests, thick scrub, etc.; and it is a further inference that the bacillus-vegetation in the bedrooms of sparsely populated places is an extension of the bacillus-vegetation in the flora of the district. I cannot stay to produce the evidence now, but it will appear further on, and it will then be seen, or will suggest itself, that the almost ubiquitous wandering spores or fruit of the natural vegetation find their way sooner or later into every part of a house, and that it depends upon their meeting in any of the rooms with a suitable soil and the favouring conditions whether or not they strike and grow and cause a local malaria. This view, it will be borne in mind, does not refer to the bacillus-spores in sputum-dust, which, it is assumed, are not only incapable of infecting the healthy by way of the lungs, but very rarely, if ever, germinate on the matrices and under the conditions met with in habitations.

Now as to these conditions. With regard to temperature, it need only be said that whilst no heat which man can endure would seem to prevent the growth of the vegetation, yet it is shut out from very high (but habitable) regions. The explanation of this may be left for the present. The water necessary for the vegetation to flourish is contained in moist or damp air. In very dry countries, swept over by

hot winds, the vegetation is much restricted, but not precluded. The one condition, however, which is of paramount importance—the condition which transcends all others in interest—is SUNLIGHT. The required condition, formulated in the words "a certain amount of solar light," is vague; but the unknown does not admit of precision. Exact knowledge here is not to be had, and the inquirer must trust to get fairly good results by educing as best he may the laws by which the bacillus-vegetation is governed by the sun.

It is an easy process to arrive at the rough general principle that this vegetation (other conditions being favourable) luxuriates in the dark, grows freely in deep shade, holds on, though more and more feebly in proportion as the shade diminishes, and perishes outright in bright sunlight. But this gives us a most inadequate view—a mere glimpse of the subject. Question after question arises. What constituents of the solar radiation affect the vegetation? Have these constituents the same effect on it in all seasons, at all places, during all hours of the day? To what extent do clouds, fogs, smoke, or other atmospheric conditions, affect the chemical intensity of the radiation and influence the growth of the vegetation? Is solar radiation directly inimical to the vegetation itself; or does it destroy, or simply sterilize, its matrices? If the latter, for how long? These and a host of cognate questions press to the front.

The observations of Messrs. Downes and Blunt, before referred to, coupled with well-observed phenomena, leave no room for doubt that the ultra-violet rays are principally concerned in the diminution, prohibition, and destruction of the vegetation; and the probabilities are that these rays, according to their degree of chemical intensity at a particular time and place, have the effect of inducing such changes in uninvaded or unoccupied matrices as would, if

the vegetation spread to them, materially interfere with its nutrition or altogether prevent its existence; and also that these rays cause the more or less speedy death of such existing bacillus-vegetation as they may impinge upon. The property solar radiation and total daylight have of reducing, or oxidizing, or otherwise destroying or sterilizing the matrices of the vegetation in human habitations make them our great allies in the treatment of consumption, and, as we shall see, will afford us a certain means of its prevention, as well as of the prevention of a considerable amount of tuberculosis. (Moreover, the affiliated breeding-grounds of some other pathogenic forms will, inferentially, admit of being controlled by chemical light scientifically brought to bear on them. For the local artificially made soils of many of these forms are, if not identical, *ejusdem generis* with the soil of the bacillus-vegetation: for example, those of acute rheumatism, erysipelas, erythema, epidemic pneumonia, some forms of puerperal fever, many kinds of "hospitalism," etc.; but with this short notice I must dismiss this large subject.)

The practical bearings of this question of solar radiation, which will be brought out more fully towards the conclusion, will be made evident enough here by observing that an inch or two more or less in the height or width of a bedroom window may make all the difference between the infection of an occupant with consumption and the non-infection of the occupant. For where a window is large enough to admit an amount of sunlight and of daylight just sufficient to inhibit the vegetation, a slight diminution from any cause in the number or in the chemical value of the admitted rays may, if long continued, end in the establishment of a local bacillary malaria. Thus a tree, a blind, or a shutter, or even a curtain, may become deadly. Moreover, prolonged cloudy, "muggy," or rainy weather will not only

intercept light, but will supply moisture and otherwise favour the germination of the fruit of this vegetation; and although, on the return of sunshine or of sufficient chemical light, the local malaria may be suppressed, it may have lasted long enough to cause a fresh introduction of bacillus-vegetation into the perhaps closing cavities in the lungs of a consumptive. So that a man's life may turn upon a few square inches of glass in his bedroom window. This, however, is anticipatory—the facts have yet to be produced; and, as all the other parts of the proposition sufficiently explain themselves, I will pass on at once to the facts upon which the whole proposition depends.

HEREDITY.

If the conception that the bacillus is an accidental parasite is sound, it follows that every phenomenon connected with phthisis will square with it. It matters not, therefore, what branch or division of the subject, or what class of facts, is taken first, or what order or arrangement, or whether any order or arrangement, is observed. All the questions are so interlaced and so interdependent that no one question can be dealt with thoroughly without leading up to many others. As this is the case, and as it is not possible to examine into every question relating to phthisis, I shall examine what appear to be the most important, the most complex, and the most interesting questions, and those which will most clearly bring out the central view in this speculative inquiry—accidental parasitism.

I begin with Heredity because the subject bristles with nice and knotty points, and furnishes, moreover, some of the strongest arguments in support of the view of pure parasitism of the bacillus. The view, indeed, dovetails so perfectly with the whole doctrine of contagion, of which heredity is or was a vital part, that the school regarded the one as the complement of the other.

It would be superfluous to cite instances of the recognized view that phthisis "runs in families." Nor is it necessary for my purpose to produce any of the voluminous evidence collected as to the degrees of consanguinity which

have been found by the authorities to entail more or less proneness to phthisis. Every writer on consumption has had to deal with the subject, and every layman knows of "consumptive" and "scrophulous" families. The fact that certain families have been more subject to the disease than other families is indisputable; and the inference that it was transmitted from parent to offspring was, up to a comparatively recent period, almost universally considered certain. It was taught that the taint was in the blood. During the last twenty years, however, the view as to the precise relation of heredity to consumption, if it has not completely changed front, has been rapidly turning round, and by the time of Koch's discovery the highest etiological authorities of the English school had come to the conclusion that heredity was not a proximate, but a predisposing, cause of phthisis. Some few writers still held by the ancient dogma of a transmitted taint, but the general opinion was opposed to the supposition that a virus, or a *contagium vivum*, or an entity of any kind, was concerned in entailed phthisis. Nor has the discovery of the bacillus caused any change or modification of this opinion, but has rather tended to confirm the English school in the conclusion that the large influence heredity has in determining an attack of phthisis is due wholly to the predisposition to the disease acquired from, or handed down by, the parent. The bulk of the faculty in English-speaking countries reject the theory that the bacillus-forms in the lungs of the consumptive are lineal descendants of bacillus-forms in the father or the mother.

On the other hand, one section of the Continental school has been moved to its centre by the contemplation of the bacillus in its relation to heredity, and finds in this relation the strongest proof of the contagious origin of phthisis—although Koch himself is opposed to the view of true heredity. It is maintained by some that the infected father or mother

may transmit the bacillus itself or its spore; by others, that the bacillus is transmitted, potentially only, to the progeny; but both these sub-sections of the school concur in the view that the bacilli finally developed in the lung of the consumptive progeny are in organic continuity with the bacilli which existed in the consumptive father or mother. We need not wander far into this botanical, biological, and etiological maze, yet, as men of high repute have accepted and advocated this interpretation of the phenomena connected with heredity, it cannot be brushed aside or left behind.

The case, condensed, is this. The bacillus of tubercle is a pure parasite; the microbe of syphilis and the bacillus of leprosy (and some other pathophytes) are pure parasites: syphilis and leprosy (?) are hereditary. The microbe of syphilis is undoubtedly conveyed from father to son in the shape of a fecund sporule, or of a potential cell, which may develope into specific vegetative or reproductive forms in the fœtus, or mature later in the child, or later still in the adult. But whatever may be the actual phase in which the vegetation exists at the period of conveyance or transmission, and whatever may be the precise mode by which it finds entrance into the embryo or fœtus, it is admitted that the syphilitic taint is substantially an inherited microbe. The bacillus of tubercle, demonstrated to be the cause of phthisis, is shown to be a pure parasite: the fœtus *in utero* has been found to be affected with tuberculosis; numbers of young children die of tuberculosis, and adolescents contract phthisis. The parallel, therefore, between phthisis and syphilis is exact; the analogy is perfect; and the two diseases are on all-fours as regards the transmission of the taint. As the microbe of syphilis, so the bacillus of tubercle, goes from generation to generation. This chain, strong as it has been made to look, will not, however, bear much strain.

Although, superficially, the two forms may here seem to be on an equal parasitic footing, yet a little reflection upon the manifestations of the disease each causes will serve to show that there can be but very little in common between the vegetative or reproductive life of a parasite which fastens on at once, and enters the blood and clings so tenaciously to its host that it rarely dies out of itself, and is with the utmost difficulty dislodged or destroyed by any known agents, and the vegetative and reproductive life of a parasite so weak that it may take years to effect a lodgment in the organism, and, when established, is generally incapable of making its way into the fluids of the body, and perishes in a few weeks by self-limitation, or permits itself to be encapsuled and rendered innocuous. There is obviously a wide difference between these two forms of vegetation—a difference which points to the supposition that the microbe of syphilis may be a gamogenetic form, and the bacillus of tubercle an agamogenetic form. This may determine their relative powers of continuance in the host. But without speculating on the sexual or asexual generations of these parasites, in which we might easily go wrong, let us examine the other data for the parallel drawn between syphilis and phthisis, where we cannot go far astray.

So rarely has the fœtus of any animal, and still more rarely the human fœtus, been found tuberculous, that it is an inference that it never is tuberculous, unless there is general tuberculosis, or local tuberculosis of the genito-urinary organs, of one or other parent. How, then, can it rationally be assumed that phthisical parents, or parents of phthisical antecedents, into whose blood or lymph bacilli from the lung had not escaped up to the time of the birth of their last child, had nevertheless transmitted bacilli to some of their children? And herein lies a radical difference

between the cases of syphilis and phthisis. In syphilis, the microbe is in the blood from the beginning to the end of its parasitism; in phthisis, the irruption of the bacillus into the blood is the first scene of the last short act in its career as a parasite in the one host. Hereditary syphilis, therefore, in addition to its being clearly established, may be readily apprehended; but hereditary phthisis, in the sense implied, would appear to be a physical impossibility.* Unless there be some occult mode by which a parasitic vegetation living outside the circulating fluids of its animal host is enabled to transmit its specific germens through the walls of vessels into the circulation in such wise that the fœtus shall be infected without the blood of the host being contaminated, it is inconceivable how the bacilli contained in and confined to the lungs of phthisical parents can be transmitted to their children; or how the bacilli thus transmitted, after a latent period of fifteen or twenty years in some unknown part of the organism, should then leave the organism and re-enter it by way of the air-passages and commence active parasitic life in the alveoli of the lungs. And by how much more is it inconceivable that parents, both of whom have come of a "consumptive stock," but have lived and died at a ripe age without having exhibited a trace of tuberculosis, should nevertheless have communicated the bacilli of tubercle to their children?

With the English school, however, heredity has fallen from its high estate as a proximate cause down to the subordinate position of a predisposing cause of phthisis. Yet even in this position the influence assigned to it is somewhat obscurely manifested and, as I conceive, greatly

* I do not overlook the experiments of MM. Landouzy and Martin (*Revue de Médecine*, December, 1883), undertaken to show the transmission of the bacillus from the guinea-pig to its fœtus. But even if their results were confirmed, their cases of congenital tuberculosis were induced traumatically, whereas I refer to ordinary phthisis beginning in the lung.

over-estimated. Or rather, it would seem that a misconception exists as to the nature of the relations between consanguinity and consumption; for, although it is obvious enough that consumption attends upon particular families through successive generations, it does not follow that the occurrence of the disease is a consequence of blood-relationship. The tie of blood has always been looked upon as a factor of greater or less importance among the causes of consumption, but is not the environment of the members of a family occupying the same house for generations more concerned than kindred in the implication of the family? Has heredity in any shape, or has locality, determined the incidence of phthisis in these families? Has not the family house been tainted instead of the family blood?

From data which will be given, I conclude that the share hereditary tendency, or predisposition, or "soil," has had in the whole mortality from consumption has been exceedingly limited—fractional, indeed. For it is almost an inferential certainty from the facts that any one of these so-called "consumptive families" could at any time have broken (what has appeared to be) the entail of early death to many of them, by simply removing from the family house to a house free from a local bacillary malaria in all the bedrooms; or even by abandoning the malarious bedrooms in the old family house; and, conversely, that a family in which consumption has been unknown, by taking a house left by a "consumptive family," and occupying the same rooms in it under the same or essentially similar conditions to those obtaining when they were occupied by the "consumptive family," would become converted into a "consumptive family" in due process of time; and chance would determine which of the new occupants of the house would be the first to be efficiently infected in its malarious bedrooms. In fine, the view here taken of the immense

effect a quasi-hereditary influence has unquestionably had in swelling the sum of phthisis in a country, is that it has been due to topographical and sociological, and not to physiological or pathological, causes.

To get at this view requires that large bodies of facts should be brought together and placed in certain lights. The difficulty is in the selection of the facts; for it may safely be said that there is not a phenomenon of etiological interest connected with the occurrence of phthisis that does not bear in some way upon this point, and upon all points, in heredity. Heredity is thus brought into touch with every question in this study. The first series of facts that press to the front, however, are those which go directly to establish the accidental parasitism of the bacillus, for it is obviously a necessity of the position to show that the bacillus-vegetation grows outside the animal body below blood-heat. Upon the soundness of this inference depends the question as to the infection of the family house. Wherefore I shall leave my immediate subject for the present, in order to examine some of the phenomena that lead to the conclusion that ordinary phthisis is invariably caused by a LOCAL BACILLARY MALARIA.

A local bacillary malaria may, theoretically, be of two kinds: one in which the malaria derives the bacilli or their spores from phthisical patients or from tuberculous animals, and from no other sources; the other in which the malaria derives the bacilli or their spores, or the parent-forms of the bacilli and their spores or other fruit, from matrices external to the body. The first of these theoretical forms of malaria holds the bacilli and their spores from the breath, but principally from the sputa, of phthisical patients; and this mode of disseminating phthisis by way of the air, though it is not called a malaria, is the one which takes the foremost place in the views of Koch and

the pure contagionists, and may be shortly described as the "sputum-dust" theory. It is palpable that a malaria dependent on sputum-dust must be short-lived, and will disappear from a locality when the sputum-dust is exhausted and dispersed. The second of these theoretical forms of malaria is set forth in the Proposition, and is put before etiologists, as I believe, for the first time—that is, in its entirety. There is nothing novel in the view that phhtisis is caused by a malaria. Far back—in the pre-bacillus period—all writers set down a "vitiated atmosphere," which of course is a malaria, as one amongst the most certain causes of consumption; and since Koch's discovery, the non-contagionists have more or less relied upon a malaria in support of their argument against contagion. My attempt, therefore, to give a concrete form to this bacillary malaria; to trace it to its natural origin in the flora of a country; to track it thence to its artificial sources in the habitations of the region; and to show what these artificial sources are, how they are formed, in what parts of habitations they are to be found, and by what conditions their presence there is determined, is, practically, merely an amplification or extension of long-enunciated doctrine. The only substantial addition made is in the view of the accidental parasitism of the bacillus, which fits in with the view of a local malaria precisely. In point of fact, I claim the long-existing belief and the more modern arguments in favour of a malarial cause of phthisis as strong presumptive evidence of the soundness of the assumption of accidental parasitism. Every phenomenon cited and every inference drawn tending to show the existence of a bacillary malaria, weakens by so much, in proportion to its value, the view that the bacillus cannot develope outside the body, and tells in like proportion for the antagonistic view. Consequently, an immense body of facts and a

vast amount of solid argument are ready to hand for appropriation.

One of the best collection of facts handled in the most masterly way to show that phthisis must, inferentially, be caused by a local malaria, is to be found in the *Lumleian Lectures*, "On the Ætiology of Phthisis," delivered in 1884, by Dr. Andrew (*Lancet*, 1884, pp. 693, 785, 833). We have drawn largely upon this fund, and may say that the reader who would fairly grasp the view of a bacillary malaria must possess himself of the facts brought forward by Dr. Andrew, and, more especially, should examine the lucid, powerful, and conclusive argument founded on them —an argument which upsets contagion, and leaves a malaria as the one cause of phthisis. I venture to take his facts *in globo*, and the greater portion of his argument against contagion, and shall refer to him at some length, to support the contention as to parasitism and all that it entails.

Dr. Andrew deals with the views of Dr. Budd, and quotes him; and, as it is as well to bear in mind the line taken by the pronounced contagionist, the quotation is reproduced in part:—"'Among the data relating to geographical distribution the following striking facts may be 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 interpretation of a new and specific morbific germ. Try every other supposition and the effects are inexplicable; make this one supposition and they are at once explained. . . .'" Dr. Andrew has sufficiently answered

this view and the other views of Dr. Budd. The evidence he has put together completely rebuts Dr. Budd's case; but it may be remarked that a knowledge of accidental parasitism and of the chemical effects of light on the bacillus-vegetation would have simplified matters. For the phenomena of the occurrence of phthisis amongst all the different races referred to by Dr. Andrew will be perfectly apprehended by the view of an artificially created bacillary malaria in the dwelling-places of natives of all parts of the world—a local malaria, the existence, extent, and potency of which are dependent on several conditions, but are very largely governed by chemical light. For instance, Dr. Rush's graphic summary, quoted in the Lecture, of the incidence of consumption in North America may, with the key, be interpreted without the necessity for assuming the agency of hereditary tendency in the persons infected. The conditions indicated by Dr. Rush point clearly to their having been implicated through a local bacillary malaria engendered in the shade. Again, the singular immunity of the negro race in one place and its marked susceptibility at another, alluded to by Dr. Andrew, is seen to depend on their huts or lodgments; and although it might not be possible for the distant etiologist to lay his finger on the precise cause of the differentiation in all cases, the barracks of the West Indian soldiers stationed on the Gold Coast fix it plainly in their case; for the dormitories in barracks are always indicated when soldiers of any nation suffer from phthisis. With regard to the consumption among the Melanesians, its cause is glaring. In the training establishment on Norfolk Island, where, Dr. Andrew mentions, about a hundred and fifty young natives stay for one or two years to qualify as teachers, these youths abandon their own native dress, or undress rather, and take to European or some kind of clothing; and this involves the discontinuance of

the ancient custom of besmearing themselves with cocoa-nut or other oil (inferentially a potent bacillicide: see OCCUPATION); but, what is of more importance, their studies keep them confined to the schoolroom, probably a church or chapel as well, and most of such buildings in these latitudes are kept as cool as possible, by thick thatch and by excluding the sun. Here would rapidly be got together all the conditions for starting a local malaria—dirt, dust, rebreathed air, exhalations, warmth and moisture, dull diffused light, and the almost omnipresent bacillus-vegetation. These conditions are, inferentially, the cause of the consumption which waits on all missions in the South Sea Islands, and seizes upon missionaries and their families as well as upon their converts. (It will not be overlooked that it has been inferred by some physicists that the chemical intensity of total daylight is less for the same hour (before or after noon) of the same day in low than in high latitudes—a matter of the greatest moment in this connection, inasmuch as it may determine the formation, and the rapidity of the formation, of the malaria. The inference, however, has been challenged.) The enormous death-rate from phthisis at Moose, mentioned by Dr. Andrew, may also be explained by the establishment of a local bacillary malaria in the dark, closely confined, and heated sleeping-rooms. The Norwegian cases are on all-fours. The immunity of Köros is no doubt correctly ascribed by Dr. Andrew to the copper-smelting. (See OCCUPATION.)

2. Some interesting facts are given relating to *Dampness of Soil.* They are all significant as to a local bacillary malaria. Dr. Bowditch's conclusion "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 of each

other," is luminous on the point. As regards Dr. Buchanan's Table showing the results of improvements in English towns, I have to confess, with Dr. Andrew, my inability to understand fully, with our present lights, why, with "*some drying*," the mortality from phthisis increased nineteen per cent. at Ashby, unless it be that rooms too damp to sleep in before were used as bedrooms after the "drying;" in which case it would depend upon the aspect of the bedrooms, and the presence or absence in them of the other required conditions, whether they would be subject to a local bacillary malaria.

It is, however, not to be lost sight of that other low forms as well as the bacillus-forms invade rooms, and, finding the conditions favourable to their vigorous growth, take possession of the soil to the exclusion of the bacillus-vegetation. As Dr. Maclagan has contended, it is an inferential certainty that acute rheumatism is caused by a specific form of vegetation. No doubt the disease is in many instances brought on by exposure to a natural miasm holding the specific forms growing in the flora of a district; but in a large proportion of cases there is no history of any such exposure. The patients have not remained for hours in wet clothing, in damp, cold, marshy places, boating, or shooting, or fishing, or what not, either by night or by day; but they have been attacked whilst following their usual occupations in towns or cities. In such cases it is a supposition that the disease has been caused by an artificial miasm, created under somewhat similar conditions to those assumed to be required for the establishment of a bacillary malaria in bedrooms. The same soil will presumably suit both forms, but the rheumatic fever forms require, inferentially, much more moisture than the consumption forms. Assuming the damp houses of Ashby before drainage to have been overrun by the rheumatic fever forms, or by

other, perhaps innocuous, forms, also requiring much moisture, then after drainage these water-requiring forms would have been at once enfeebled and would soon have been ousted by the tubercle bacillus and other forms. This view may help to explain the phenomena at Ashby, and to throw some light, by reflection, on the supposed heredity of acute rheumatism. This disease, indeed, and ague, stand much upon the same footing in respect to their local miasms as consumption stands to its local miasm.

3. "Altitude and dampness," Dr. Andrew observes, "have undoubtedly influence, the first in preventing, the second in developing, phthisis, and in both the facts indicate that *there is a something, in large measure independent alike of the constitution and social habits of the populations subject to it,* but without which phthisis cannot exist. It is certainly improbable that altitude acts only by producing, sooner or later, a race of men which is proof, or all but proof, against certain morbific 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 anti-phthisical constitution,* if it be in any way due to altitude, *must be a very temporary affair, easily acquired and easily lost.* . . . 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. . . . 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 not yet spread in the ordinary course of things by direct contagion."

In the foregoing words Dr. Andrew foreshadows, if he

does not actually indicate, the view of accidental parasitism; and, moreover, he comes very near detecting the fallacy in the doctrine of hereditary predisposition. He had but to push his argument on the phthisical and anti-phthisical constitution a step further. It is plain that on the chance presence or absence of the bacillus-vegetation will depend whether a man shall have one or the other form of constitution; or whether, indeed, he shall not have, at different, though not very remote, periods of his life, both forms of constitution, seeing that either is so "easily acquired and easily lost." Dr. Andrew lets us see that he perceives clearly that the anti-phthisical constitution of the highlander and the phthisical constitution of the lowlander are pure illusions, and that "some external agent" determines whether the highlander or lowlander shall contract phthisis or not. If the protection thrown over the one and the susceptivity of the other are imaginary constitutional peculiarities—mere conceptions of the etiologist—then, to bring the matter nearer home, what difference should we expect to find in the constitutions of members of a "consumptive family" and of a non-consumptive family in England? Or, to press the question closer still, what constitutes the line of demarcation between the constitutions of the infected and the non-infected members of a "consumptive family?" Is it not patent that the highest type of anti-phthisical constitution shall succumb to phthisis if laid hold of by the bacillus? and that the descendant of many generations of consumptives, with consumptive collaterals in each generation, cannot by any possibility become the subject of consumption if kept out of the region of this vegetation?

4. The Lumleian Lecturer says, "Now, the anomalies in the geographical and local distribution of phthisis, and in its incidence upon different races, families, and individuals,

the undoubted existence not only of general laws governing its manifestations in both these directions, but also of startling exceptions to these laws, admit of no rational explanation on the developmental theory, or, let me add by anticipation, on that of direct contagion. Given the bacillus or some other micro-organism, and both sets of difficulties, the geographical and anthropological, wide apart as they appear to be, receive the selfsame easy solution. The bacillus, *whether inside or outside the body of man, or any other of its 'hosts,' cannot thrive unless certain conditions be present.* . . . The exemption, then, of certain localities and of certain races from phthisis may be, and is, due to the fact that the bacillus has been unable to contend with some adverse circumstances or other with which it has met in the first case *during the independent*, and in the second *during the parasitical*, phase of its life." *

The view of a malarial cause of phthisis has been advanced before, but had never been so thoroughly brought out as in these lectures. Indeed, this extract from them, coupled with the former one, would seem to indicate that Dr. Andrew has, in effect, anticipated the view of accidental parasitism. We do not know, however, whether he intended to attack the position taken up by Koch, for the matter is left in doubt by reason of the omission to challenge explicitly the view that the bacillus is a pure parasite. Dr. Andrew states expressly that he is quite prepared to accept the doctrine, "No Tubercle without Bacillus, No Bacillus without Tubercle," but when he comes to consider the nature of the parasite, he unaccountably omits to open the question of its pure or accidental parasitism—though it may be glanced at obliquely in the suggestion that it is more nearly allied to the bacillus of malarial fever than to other bacilli. Dr. Andrew touches

* I have italicized the words in these quotations.

upon the very point when speaking of the bacilli of typhus, small-pox, scarlatina, etc., yet he ignores it as regards the bacillus of tubercle, and from first to last takes no notice whatever of Koch's dictum that it must be a pure parasite because it cannot grow outside the body below blood-heat. It is difficult to apprehend this. It is hardly to be supposed that Dr. Andrew would have treated an important question of this kind, coming from such a source, with silent contempt—that he would knowingly and deliberately have traversed or overridden Koch's views without once alluding to them. It may therefore be assumed that in some way he overlooked the views and worked out his own with regard to the malarial cause of phthisis, all unconscious of the fact that Koch had pronounced that the independent growth of the bacillus is an impossibility. The oversight, however, if it is to be taken as one, was felicitous, inasmuch as Dr. Andrew's conclusions show what conclusions an etiologist may arrive at on this subject by drawing them from the phenomena connected with the occurrence of phthisis. I think it will be seen that although Dr. Andrew has arrived at the same results as myself in one important matter, and has preceded me in reaching these results, yet that I have gone by a line of thought distinct from his. We have each taken parallel lines for the greater part of the way towards a local bacillary malaria, and though the lines have been connected here and there at certain points, a local malaria is but a branch, or "feeding," line here. At the same time I have to acknowledge frankly my extreme indebtedness to the Lumleian lecturer, not only for the valuable aid he has brought in material, and more especially for the reasons he has furnished for the assumption of a malarial cause of phthisis—for this is a necessary condition precedent to the establishment of the terminus of accidental parasitism—but

for the support his views have been in my attempt to exhibit what I conceive to be the false botanical position originally assigned to the bacillus. I have looked in vain for any objection to, or protest against, this vicious conclusion, but Dr. Andrew is the only etiologist who has assailed it; and though he may have done so inadvertently, or indirectly, or by implication, it matters not: the force and effect of his argument is as great as, if not greater than, if he had undertaken expressly and declaredly the overthrow of pure parasitism. If I have had misgivings at times about being on the right track, these Lumleian Lectures have been the "blazed line" by which confidence was restored. For if phthisis is caused by a malaria—and Dr. Andrew well-nigh forces the conclusion that it is—pure parasitism of the bacillus cannot be. I will only add that if explanation be sought for the fact that Dr. Andrew's able and thoughtful work has led to no further efforts in the same direction by others, by which a full knowledge of the etiology of tubercle might have been attained, and effective measures for the prevention of phthisis might have been taken, it will, probably, be found in the unpreparedness—or the preoccupation rather—of the "soil." The etiological mind has been and is so overrun with the pure parasite that other forms of thought have found no place.

Before leaving the Lumleian Lectures, attention may be drawn to the facts and arguments relating to the analogy between phthisis and malarial fever. One point of divergence Dr. Andrew notes is that "they differ in their habitats, phthisis being most prevalent in cities which afford all but complete protection against malaria." "Clinically," he observes, "the diseases agree in the fact that recrudescence takes place after long periods of apparent health. In the case of ague this frequently happens without any fresh exposure to infection. . . . The history of many cases

of phthisis makes it probable that the same thing takes place in it too." It may be remarked that the fact that the habitats of phthisis and ague are in such contrast may be readily apprehended by the supposition of a local bacillary malaria, limited to the dwelling-house, and perhaps even to a certain room in the house. With regard to the instances of recrudescence in phthisis, I infer from the phenomena observed in connection with its occurrence that it depends upon reinfection through fresh introductions of the bacillus-vegetation; but the point will be specially referred to.

Instances innumerable of a malarial cause of phthisis might be cited. Indeed, the whole history of the malady, properly read, is found to be a long record of such cases. I shall go, therefore, to the class of cases exemplifying the sharply defined and narrowly circumscribed area within which the bacillus-vegetation is most frequently confined when it becomes the efficient agent of ordinary consumption. The most striking and instructive cases illustrating the purely local nature of the bacillary malaria are those met with in the residences of the "consumptive families." But, before going further, a few observations on the short parasitical life of the bacillus-vegetation may serve to explain many points that might be obscure.

Cases of Complete and Partial Recovery from Phthisis.—Instances of obsolescence and of cretified or cretaceous nodules of tubercle being found in the lungs after death are common; and instances constantly occur of persons recovering from unmistakable phthisical lesions. In many of these cases the cure is clearly due to a change of country, but many consumptives get well in their own country. This is excellently well brought out in a paper by Professor Austin Flint, of New York ("The Self-Limited Duration of Pulmonary Phthisis," *Lancet*, August 26, 1882). I can

merely glance at this interesting and instructive contribution to the etiology of phthisis. Professor Flint, after citing a number of illustrative cases, says, "I therefore submit, as substantiated by the clinical facts which I have cited, the following proposition:—Pulmonary phthisis, in a certain proportion of cases, has a self-limited duration, the disease ceasing to exist after more or less progress of the local affection, all symptoms referable to the lungs disappearing, and recovery, as regards the general health, being complete. The disease is also self-limited in a certain proportion of cases in which lesions remain, giving rise to more or less of cough and expectoration, the persistence of these lesions not being incompatible with good general health and long duration of life." He further observes, "It is an interesting fact that self-limitation is exemplified in the majority of the fatal cases of phthisis. As is well known, the disease, as a rule, advances not by a continuous progress, but by a series of successive invasions, separated by variable intervals. After each invasion, or, as it has been termed, tuberculous eruption, there is a temporary self-limitation of the disease. . . . The fact suggests a capital object in the treatment—namely, prevention of a renewed invasion."

Professor Flint asks—

"How is self-limitation to be proved as applied to phthisis or to any other disease? Facts pertaining to morbid anatomy and to therapeutics may render the application of the doctrine probable; but, evidently, positive and complete proof can only be afforded by a collection of cases in which the disease pursued its course without active interference in the way of treatment, either medicinal or hygienic, and without notable changes in habits of life, or in any of the conditions under which the patients were situated when the disease became developed. For obvious reasons these requirements for absolute proof are not easily

obtained in cases of a disease like pulmonary phthisis. Yet cases involving these requirements occasionally come under medical observation. The hopeful mental state which generally accompanies phthisis sometimes leads patients to trust altogether to nature for restoration to health, and to continue their usual manner of living without any alteration. Some patients do this from a conviction that they have not a malady of sufficient consequence to claim attention, beyond, perhaps, palliative remedies; and some from circumstances which render it difficult to do otherwise. Again, there are phthisical patients who do nothing in the way of either therapeutics or hygiene from a thorough scepticism as to the advantage of doing anything. . . . In a few cases no appreciable influences, either of medication, diet, or regimen, had been brought to bear on the disease; the patients took no active remedies, and continued unchanged the same habits of living as before the development of the disease. It seemed a logical inference that in these cases the disease was not arrested, but that the recovery was owing to an intrinsic tendency thereto; in other words, the disease ended in recovery from self-limitation. . . . Recovery has taken place under the employment of divers remedies; yet these remedies have so generally failed that, for the most part, they are now obsolete. The explanation of their apparent efficacy is to be found in the doctrine of self-limitation. The disease ended favourably, not from a specific influence of the remedies, but from an intrinsic tendency."

We have room but for one more quotation:

"The doctrine of self-limitation bears on the climatic and other measures entering into the hygienic treatment of cases of phthisis with not less force than on the employment of drugs. As regards climate, is there a practical theorem more perplexing to the practitioner of medicine

than that of selecting the best resorts for phthisical patients, provided the selection be made on the basis of an impartial consideration of the reported results of climatic agencies in different situations? Underlying the exaggerations on the one hand, and on the other hand the depreciations of particular climatic resorts, founded on the different results in a few cases, is the factor of unknown power, self-limitation, the existence of which is generally ignored. Here is the explanation, at least in part, of the discrepancies of testimony concerning the results of climatic influences of different situations."

In a discussion in the Royal Medical and Chirurgical Society (January, 1885) on the Tubercle Bacillus, Dr. Coupland observed that no matter where the local manifestation of the disease was, whether in the kidney or the lung, or elsewhere, "it obeyed the same laws, and thus might in any part become 'obsolete,' fibroid, or cretified and non-infective, instead of softening and infective" (*Medical Times*, January 24, 1885). Dr. Hermann Weber, in his Croonian Lectures (*Lancet*, March 14, 1885), insists strongly on the curability of phthisis. He gives illustrative cases, and suggests the question whether "the encapsuled cretaceous and semi-cretaceous masses still contained bacilli and spores. . . . If the bacilli and their spores can retain their life for years in such encapsuled masses, then it is very possible, as has been suggested by others, that the fresh attacks of phthisis which occasionally occur after years of apparently perfect cure are in some cases due to self-infection from the escape of perfectly shut-up parasites."

The inability of the bacillus to establish itself in the true skin in such numbers, or with such parasitical power, as to force its way into the subcutaneous cellular tissue has been demonstrated, traumatically, by Koch and others; but it has also been demonstrated naturally. Warty tuber-

culous *plaques* occur occasionally on the skin, more especially, it would seem, of persons who have to do with living or dead animals (coachmen, butchers, cooks, etc.), and remain for years, inducing caseous degeneration, perhaps, and chronic or sub-acute inflammation, with more or less infiltration into the superficial layer of the cutis, but without inducing such necrotic changes as to enable them to gain admittance into the fluids. In this category come the rare "corpse-warts" of anatomists, post-mortem room attendants, etc., which have been found to contain tubercle bacilli; unless, from some accidental occurrence, general tuberculosis does not result as a consequence of their presence in the skin.

Other cases, showing the limitation of the bacillus, will be glanced at further on; but from the facts alluded to in these classes of cases, standing by themselves, it would be a fair presumption that the bacillus is not a pure parasite. Its behaviour in all its relations with animal organisms would seem to stamp it as a form of vegetation which adapts itself with infinite reluctance, or with extreme difficulty, to enforced incarceration in a living tissue outside the circulating fluids. The restricted limits within which it vegetates in the skin, in the few instances in which it is arrested there, and its inability to escape and make its way into lymph or blood streams—which are, from many facts, inferentially the most congenial soils to be had in the foreign (?) medium of the organism—point to a low degree of parasitical power, and indicate or suggest that a sojourn in the animal host is a non-natural phase of its existence. But, setting aside this evidence of feebleness in the vegetation, how are we to reconcile the assumption that the bacillus is a pure parasite in animal organisms with the fact that the vegetation shall pass in vast quantity over the whole mucous surface of the air-passages for months or

years, and yet never effect a lodgment and naturalize itself anywhere along the track? None of the explanations of this singular inaptitude (in a pure parasite) for laying hold and starting what is supposed to be its normal parasitic life, meet the case. Ciliary movement does not, for other low vegetal forms (and even accidental parasites) affix themselves readily enough—too readily—to the parts over which the bacillus-vegetation travels without attaching itself. Nor does the "suitable soil" theory answer here. Members of a "consumptive family" shall be surrounded with phthisis from their birth, and shall nurse brothers or sisters, father or mother, in their illnesses, and come scatheless out. Here we have the most suitable soil and abundance of seed, but no crop. We have the anomaly of a form persistently declining or refusing or unable to grow on its indigenous soil—the only soil, moreover, on which it can live.

To turn to the bacillus as a guest fairly lodged in the lung. If its seeming reluctance to enter the epithelioid cells suggests capture and imprisonment of the errant spores or other fruit of a vegetation growing on a free surface somewhere near the future host, its career as a parasite, viewed from the clinical and pathological standpoints, leaves it to be inferred that its parasitism in the animal body is not only compulsory, but short-lived. The number of instances in which persons are found after death to have had tubercular disease of the lung, from which they had recovered, is hostile to the assumption that the organism is the natural habitat of the tubercle bacillus. Even when admitted into the blood, the vegetation may die out; for the records of traumatic experiments show that the introduction of tubercle and of cultivated bacilli into the systems of animals, which has induced tuberculosis, has nevertheless been followed in some instances by the complete recovery

of the animals. Looking at these facts with those given in the admirable paper of Professor Flint, and regarding them in the light of "Self-Limitation," the conclusion is almost forced that the unwilling bacillus guest soon perishes in the lung of the unfriendly animal host. This, too, is the lesson to be learnt from the brave words of physicians who insist on the possibility, if not of an actual *restitutio in integrum*, at all events of a healing process even in seemingly hopeless cases of consumption. And what is the interpretation of a relapse? It is of common occurrence that patients become cured, perhaps by change of locality in their own country, or by residence at some health resort, or by the long sea-voyage, and then within a year after their return home have fresh attacks of lung-disease. On favourite Alpine heights there are scores of returned invalids, some of whom have gone back, not once or twice, but several times, because of recurrences of phthisical symptoms in their native place. Knowing what we know now of the histogenesis of tubercle, the explanation of these facts is simple enough. They say plainly that the colonies of bacilli which set up the tuberculous processes in the lungs have died out (or have become encapsuled and rendered, at all events for the time being, parasitically impotent) or have been expelled; and that, therefore, the lungs, freed from further inhalations of the bacillary malaria, have soon repaired damages. Cavities have been closed, and the deposits of tubercle not yet broken down have begun their conversion into cretaceous material. Then, of course, the re-submission of the recently healed or patched-up lungs to the local malaria by which they were originally infected will sooner or later bring about their reinfection—and this all the more readily because their vulnerability is now increased by loss of substance (never altogether compensated for by expansion or hypertrophy),

by the cicatrix or cicatrices of cavities, and by general impairment and obstruction to function from the foreign tuberculous matter left in them. These lesions favour parasitism, and the destructive changes caused by the bacilli are more rapid and pronounced.

Numerous facts bearing directly and indirectly on this question of pure parasitism remain to be produced, but enough has been advanced for present purposes.

HEREDITY—(*Continued*).

"CONSUMPTIVE FAMILIES."

THIS kingdom is studded with palaces, castles, halls, manor-houses, country seats, farm-houses, and family residences of all kinds, some of which have been in the possession of certain families for centuries, and many have been occupied by one family for several generations. The aspect of the bedrooms in them depends on the aspect and construction of the building. The larger bedrooms with the best views have probably been occupied by the heads of families, by guests, and in course of time, perhaps, by the elder children. The smaller bedrooms and nurseries are generally at the side or back of the house; and in most of the great mansions certain bedrooms have descended, by a sort of prescriptive right, to the firstborn and the other children in a regular order of succession. It is obvious that the principal as well as the minor bedrooms may be so placed as to face east, west, north, or south, or so as to have two or more aspects, according to the lie of the land or to chance. Some of these rooms admit the sun's rays through narrow casements, and are always more or less dim or gloomy; others admit abundance of light through good-sized windows. Most of them, though not all, are provided with fireplaces. Built of solid masonry, stone, or brick, and being above the ground-floor, the rooms are dry, so far as admitting

rain-water or sucking up moisture from the foundation is concerned. The walls may have been wainscotted, or covered with tapestry or hangings of some sort, but in these later times are generally papered or painted. The floors are of polished or unpolished wood, the planks more or less apart, uncarpeted formerly, but latterly carpeted partly or wholly. Odd nooks and corners, bays, recesses, cupboards, and old solid wardrobes are common in these rooms, and, though many have been modernized, their main features remain.

To the interior of some of these rooms occupants, by their breath and exhalations, have contributed a large fund of solid matter, much of which has been dissipated in many ways, but the balance has been deposited in positions (suggested in the Proposition) from which it could not well have been dislodged in the ordinary way of housekeeping. If to this remanet of animal matter be added such other organic and inorganic matters as fall as dust, or accumulate as dirt, it will not have taken long to establish an extensive field for the growth of low forms of vegetation. If, now, we take such of these rooms as furnish the required degree of shade for the growth of the parent-forms of the bacillus, and assume that these specific forms (everywhere present in the air of Great Britain) have found their way into them, there will be sufficient warmth and moisture to enable them to germinate and overspread the artificial matrix abundantly provided. As has been observed, the precise amount of aqueous vapour necessary for the bacillus-vegetation cannot be determined, yet that moist state of the air which obtains in all badly lighted bedrooms will suffice. Occupied rooms from which the morning or mid-day or early afternoon sun is excluded, are all always more or less damp, and "strike quite a cold chill into one" even in summer; and the humidity of the air, roughly indicated by this phrase, may be assumed to be sufficient

for the growth of the vegetation, and to be required, moreover, for its suspension in the air of the room at night, so as to constitute an efficient bacillary malaria.

Our forefathers thought but little of ventilation, but no ordinary form of ventilation would prevent the presence of fungi in a room of this kind. The displacement of internal by external air relieves the room of a proportion of breathed air, no doubt, and gets rid of some of the suspended vegetation. It lessens also the nitrogenized material that would otherwise go to the accretions already present. But it does not ensure the destruction of the vegetation, nor does it materially affect the damp state of the residual air. The best household ventilation, in short, salutary though it may be in many ways, cannot preclude the local vegetation in these sunless bedrooms, any more than the natural and perfect ventilation of the air surrounding a tree standing by itself in the open will prevent a growth of Confervæ on one side of its trunk. The bark of such a tree may have a luxuriant crop of confervoid vegetation extending round the northern aspect of the stem, ending on the east side where the morning sun's rays fall, and on the south-west side in a less defined line where those of the afternoon sun begin to strike. This growth shows plainly enough that the chemical intensity of solar radiation, and not ventilation or changed air, determines, in some cases, whether a given vegetal form shall or shall not live in a given position. Hence it may be that the most perfectly ventilated bedroom, scrupulously kept, may nevertheless contain a noxious vegetation, and may convey to those who enter it more or less of the sensation of being in a vault, a cellar, or a dungeon. Whilst there may not be more than a mere trace or suspicion of that vile "musty" or "fusty" odour which is so powerful in some of the dirtiest habitations, and is well characterized as a "poor smell," yet sometimes

there is this touch of humanity even in the chambers of the great.

Other bedrooms in these houses, similar in every respect as to interior with those just described, are so placed in the building that the sun may shine freely into them a good distance for three or four hours of a morning, or during midday; and if it is not prevented from shining into them by foliage, blinds, shutters, balconies, verandahs, etc., the air of such rooms will be sweet and wholesome. The marked and striking contrast in this respect between rooms in the same house is a matter of homely observation, and has come within the experience of most of us. Everybody feels the difference between the sunny and cheerful and the sunless and cheerless rooms of his own house, and the thoughtful man knows that the indefinable shivering, or better, perhaps, described as "shivery," or "shuddery," sensation he gets in the latter does not depend on temperature, or upon the amount of aqueous vapour in the room. The atmosphere in the other rooms never gives the same chilly, uncomfortable feeling, summer or winter, but is always pleasant and agreeable. All housekeepers know the vast difference in the bedrooms of a large house. The bedding in some of them cannot be kept free from damp, and boots and shoes, etc., left in them are rapidly overrun with the common moulds; whereas in the other rooms there are no indications of an ever-present moisture, and fires and airing processes in summer or fine weather are rarely, if ever, required. Even contiguous rooms with different aspects differ so greatly as regards humidity, that some shrewd and observant housewives see clearly that the morning sun has most to do with it, and insist on having the blankets, paillasses, etc., set out in his direct rays, if possible, instead of placing them before the fire which may be in the room. They distinguish, as well as washerwomen,

the sun's *bleaching* from his *drying* power, and have come to believe there is a virtue in him over and above the warmth he gives. These facts are so familiar, or may be so easily verified, that we need not refer to any of the saws and proverbs expressive of the belief, not alone of the vulgar but of the learned, of all ages and nations, in the salutary effects of letting the sun, and especially the morning and midday sun, into habitations. It may be observed, however, that what is merely a mild belief in the Englishman is an active creed in the Italian and in the people of the south of Europe.

A scientific explanation of the difference in the air of bedrooms in the same house is not possible. It is a supposition, from the facts known, that solar radiation oxidizes, reduces, or decomposes, in proportion to its chemical intensity at the earth's surface, the organic matter deposited in rooms, and converts it into inorganic matter. If it be assumed that the intensity is sufficient to destroy all the organic matters in a room, at all events some forms of vegetative life in it are impossible: and this is the typically sweet and wholesome bedroom. The atmosphere of this country is loaded with myriads of the spores and reproductive organs of low forms of vegetation, which, though taken in with every breath, are perfectly innocuous (unless when present in extraordinary numbers within a limited volume of air). Millions of such forms are constantly present in the most wholesome of bedrooms, together, no doubt, with a small proportion of the wandering spores, etc., of more noxious forms; but none of the forms can find breeding-grounds of any extent, and consequently their presence is not felt.

But let it be assumed that the chemical intensity of total daylight in the bedroom is insufficient to destroy the organic matters, and we have the unwholesome bedroom. Most of the forms of vegetation wafted into it will find

suitable soil and the required conditions for growth. The most vigorous, or those which thrive best under the conditions present, take possession first, and, dividing the matrix between them in a ratio with their numbers and their rapidity of spread, exclude the then weaker growers. The turn of these does not come until the first possessors, after a certain number of crops, become exhausted and linger or die down. These are followed by others again in their season; and thus a rotation of crops of different forms is obtained, and each one flourishes in its turn and contributes by its decay organic material to the common fund of soil for all. The presence of these forms of vegetation within a room—in the best class of bedrooms, principally behind skirting-boards, or below the flooring and between it and the ceiling of the room below, or in positions where it can neither be seen nor got at by housemaids—ensures constant moisture in the air of the room, that no heat from a fire, which shall fall short of desiccating and killing the vegetation, will get rid of. This serves to explain the hopeless, persistent dampness of the room, and the relatively anhydrous air of an adjoining sunny room. Then the spores and particles of even an innocuous vegetation growing in the room, being carried by currents of air or wafted by watery vapour and added to the spores, mycelium, etc., ordinarily present in all rooms of the house, and being, therefore, in excess of the normal proportion of such vegetation, may be supposed to give somewhat of an uneasy feeling. But if to these generally innocuous forms be superadded forms noxious to health, and even deadly when introduced into the organism in large numbers or under certain conditions—the forms, for instance, which cause catarrhs, sore throats, influenza, pneumonia, acute rheumatism, typhus, small-pox, tuberculosis, etc., etc.—it may readily be conceived that the system of a person going into the

room experiences a kind of shock which is expressed by a decided shudder, the warning note of danger. In these noxious rooms the air holds more than the normal proportion of particles of vegetation at all times, but it is principally at night that they are suspended in the greatest abundance. By analogy with marsh, jungle, and forest miasms, and from phenomena connected with the occurrence of phthisis, it is an inference that the heavier portions of the forms then rise with the moisture surrounding, and given off from, the growing local vegetation, and hang in the still air up to a certain height in the room—the bacillary malarial line—determined by meteorological conditions.* It is hardly necessary to add that, so far as bacillary malaria is concerned, the degree of infective power will be governed by the amount of organic impurity in, and by the value of the chemical light admitted into, the room, as well as by seasonal and other conditions.

It is admitted that there is no sufficient explanation extant of every fact relating to the incidence of phthisis in families. I submit, however, that this imperfect sketch of a local malaria furnishes, or suggests, a full and sufficient explanation; and, further, that exhaustion leaves no other. By it we apprehend the mode by which members of the upper classes are brought into relation with their poorer brethren in the matter of consumption; and how they may receive, under apparently the best sanitary conditions, such an amount of the bacillus-vegetation into their lungs for a sufficient length of time to become efficiently infected. By it we see how members of a family may be infected whilst other members of it escape infection; how an infected member of a family who leaves home and returns cured is

* This "bacillary malarial line" might be of grave importance in some cases—as where men or youths are berthed at night one above the other, in tiers. Those in the upper berths might be perfectly free from the bacillary malaria which was infecting the others below.

reinfected; how the recovery of one infected member who remains at home may be brought about; how recrudescence occurs; how the great decrease of phthisis in the old "consumptive families" is to be explained; how it is that these old families have not died out root and branch long since; how it is that so few old families are altogether free from the so-called "taint"; and how every complex question of intermarriages and collaterals may be resolved.

It will now be seen, too, more clearly on what grounds I ventured to question the long-cherished view that heredity, or hereditary influence, or a "suitable soil," determines the incidence of phthisis in a family. If the inferences I have drawn be sound, it follows that, as I observed, it is a portion of the family house, and not the whole of the family blood, which is "tainted." The sequence does not of necessity carry with it the view that there is no such thing as hereditary predisposition; though I strongly suspect that, as commonly referred to, this predisposition, if not imaginary, is of kin to the "phthisical constitution" of the lowlander, which, as Dr. Andrew points out, is "easily acquired and easily lost." At all events, the hereditary predisposition is lost so easily that a change of climate—nay, even a change of bedroom, a distance of a few yards—may suffice to break the entail. It is the mere accidental distribution of the members of a family in the sleeping-chambers of a house which, as I suppose, brings out the predisposition, and, causing the mythical leaven to work converts one of the family into a quasi-lowlander and leaves another to become a quasi-highlander. The house of a "consumptive family" and that of a non-consumptive family stand precisely in the relation of lowlands to highlands. There is not a shadow of difference in kind or in principle between the two positions. Transplant the bacillus-stricken family from their malarious house—their

lowlands—to a wholesome sunny residence but half a mile away, and the phthisical quasi-lowlanders are soon transformed into the hardy anti-phthisical quasi-highlanders. In effect this is done every day, undesignedly, by frequent change from country to town, to the Continent, to watering-places, etc. Continuous residence in a family house is unknown nowadays, and the conjoint result of change and more light is marked in the large reduction of phthisis in the "consumptive families."

I conclude that in the aggregate a strain of blood neither induces proneness to, nor affords protection from, phthisis; but I conceive that long-continued inhalation of the bacillus-vegetation in such proportion as to fall short of an efficient "dose" may confer a kind of immunity, or afford a degree of protection. It is evident that inherited personal traits, bodily conformation, and physiological peculiarities may lead to sedentary or other habits, by which members of some families may become more than ordinarily exposed to the bacillus-vegetation, and may inhale a relatively large proportion of it in a given time, where other persons would take in little or none; but it is no less evident that other "consumptive families" inherit opposite qualities of mind and body, and are active, given to field sports, well formed, and fully equipped for the battle of life. Some of the finest specimens of the English race have come of such families, and have lived to a good old age. It is not until some time after the doomed members of a family of this description come within the sphere of the malaria, that they begin to acquire the "phthisical habit" which ends in their falling into the condition which has been called "physiological misery," whilst their brothers and sisters remain in superb health. This "physiological misery" we take to be the sign, therefore, not that an inherited predisposition is paving the way for an attack—preparing the suitable soil for a

bacillus crop—but the sign that the victim is the bacillus host *in esse*, and has long been engaged in the struggle with the involuntary guest; that the powers of resistance against the efficient lodgment of the parasite are well-nigh exhausted or completely overcome; and that the patient is already in the throes of tubercle-formation at the apex of the lung, though it may not be revealed by physical signs to the physician. For months, or even years perhaps, this struggle has been going on nightly, with longer or shorter intervals of relief from invasion, probably through absence from home, or in consequence of periods of diminution in the value of the bacillus-vegetation inhaled, owing to seasonal conditions.

In point of fact, as I conceive, the bacillus-guest prepares its own soil in the host, and the parents of the host do not contribute to the fertility of this soil, but hold an ancillary relation to the guest nevertheless. They foster, unwittingly, the bacillus-vegetation, by providing for it a suitable soil and favouring conditions outside the future host.

Numberless points relating to phthisis in particular families may be cleared up by the supposition of a malarial bedroom, not one of which admits of an adequate explanation by the theory of contagion. Many of the facts connected with other occurrences of the disease may, and do serve both the contagionist and anti-contagionist to support their tenets; but in the "consumptive families" we have series after series of phenomena that are utterly irreconcilable with contagion. It being a necessity of all etiologists, of whatever school, that the bacillus shall be got into the epithelioid cells of the alveoli of the lung to account for ordinary phthisis, the contagionist (finding that he has to discard the ingesta, inoculation processes, and all other modes by which it has been supposed or suggested that

a pure parasite can find its way there) has nothing left but sputum-dust and hereditary predisposition, or, as some insist, true heredity. This last I have dealt with, and need only add that, as a cause of ordinary phthisis, it is unnecessary as well as incomprehensible.

The explanation that ordinary phthisis is caused by means of dry sputum is certainly not so wild or visionary as the explanation by heredity, but it is just as insufficient and untenable; for [setting aside for the present the moot question as to the efficiency of the bacillus-spore in sputum as an infective agent], when examined by the incidence of phthisis in families, as well as by other facts, it is soon apparent that it is an inadequate explanation. It fails signally to account for any one of the phenomena that I have just been considering, and for many others. For example, say that three or four of a family of eight or ten develope phthisis at the usual age during three or four generations, and that some of those attacked recover, or that they all recover or all die; that one or both parents in each generation at one time or other have been affected, or that neither parent has at any time been affected; or take any other of the interminable variations in the incidence of phthisis in these families during the past century;—then, even if we waive the important question of the length of time the bacillus-spore retains its vitality outside the body, and grant for illustration sake that it can live in a dry state for twenty years or more, how is it possible to conceive that the phthisical of one generation can leave such numbers of spores in the family residence as to account for the first attack of phthisis in the next generation? The quantity of the sputum, or the total value of the bacilli and their spores, retained in a room immediately after the death in it of a phthisical patient of the well-to-do classes must be inconsiderable as compared with the value of the bacilli and

spores given off during the lingering illness of the patient, to the effects of which several of the relatives were largely exposed, probably, without having been attacked with the disease. But the residuum, whatever its value, of the spores and bacilli contributed to the room by a dying aunt or uncle of one generation, we must suppose to remain there after all the cleansing processes and other disturbances (with or without occupation of the room), until a niece or nephew of the deceased consumptive comes to occupy it; and that what remains of the original spores and bacilli is sufficient both in numbers and potency to cause efficient infection of the occupant. And, unless it may be assumed or can be shown that the bacillus-spore acquires such virulence by age that a single old spore shall have the parasitical power of ten thousand recent spores, it is simply impossible to conceive that a scion of one generation has become phthisical consequent upon spores handed down from the previous generation. For it is palpable that a brother or sister during one week's or one night's attendance on the dying patient shall be exposed to ten thousandfold the danger a nephew or niece will be exposed to from occupying the room of an uncle or aunt who died some twenty, fifteen, or even ten years before he or she went into it. But the chances that the first phthisical attack of a member of a family is dependent on bacillus-spores left in a room by one of the previous generation, are seen to be still more remote in the case where none of the previous generation die from phthisis, or get cavities in the lung. It may have happened that the room of a phthisical lad may have been occupied by two or three of his uncles in turn, who may have shown premonitory signs of lung-mischief, and may even have had small deposits of tubercle, but they have gone off one by one (before caseation) to the universities, or to the army or navy, or elsewhere, and have

either recovered and gone through their career or have died perhaps of phthisis away from home. In such a case the contagionist has to go back yet another generation for the efficient contamination of the room; for he is driven to a purely local cause—a fund of old spores—to account for the first attack in the latest generation. He cannot assume a common cause, such as dry bacillus-spores equally disseminated through the house, or the whole family (having the hereditary predisposition and being equally exposed) would be involved. In short, the difficulties surrounding this one facet of the subject are insurmountable.

If we go on with the history of the family after the first phthisical attack in a generation, we have fresh as well as dry bacillus-spores to deal with; and here it should be far easier for the contagionist to make his case good. But supposing him to bridge over the space between two generations by chains of dry spores, he then finds himself face to face with anomalies, incongruities, and eccentricities without end. They who should be infected to a certainty unaccountably escape infection, and the converse. Both parents, though of undoubted phthisical lineage (and age gives no protection), remain free from attack throughout the successive fatal illnesses of their children; the daughter most in attendance on the infected keeps well, and the others, and perhaps the sons, sicken; and so on.

Writers have remarked certain sequences in the incidence of what has been called "family phthisis." In some cases the first or second son, or first or second daughter, die in a regular order in each generation. Dr. Brehmer, of Berlin, has observed that "the tendency to inherit is greater in children born within twelve months of their elder brother or sister than those born after a longer interval." Dr. Brehmer also, according to the *Medical Times* (September 5, 1885), "presents a series of one

hundred cases to show the circumstances under which isolated members of families, not labouring under hereditary taint, have become the subjects of tubercular disease, . . . and it is remarkable that nearly all were members of large families, generally the sixth in order." I do not profess to see a way to a clear explanation of either of these curious illustrations of the apparent freaks in the incidences of phthisis, though probably the facts might disclose that local malaria was concerned, especially where the sixth of a family becomes infected; for it may very well be supposed that the available decent bedrooms are occupied, and that the "sixth in order" has to be stowed away in some ill-lighted corner of the house. But how is the contagionist to accommodate these phenomena with the view of a transmission of bacillus-spores from a previous case of tuberculosis? It would not be easy to do this, even if in the hundred cases the families were "labouring under hereditary taint," but these instances of "spontaneous," or "acquired phthisis," occurring in the "sixth in order," must be extremely difficult to square with sputum-dust. Perhaps these are the cases in which it is better to doubt the facts.

I have seen it stated somewhere, though whether in a medical work or not I cannot recall, that the succession to titles and estates in England has been largely governed by phthisis. The gist of the statement, as well as I can recollect it, is that in some families the eldest son has succeeded the father for three or four or more generations, and that one or more of the younger sons and daughters have died from phthisis, and always in a certain order in each generation. In other families the eldest son has invariably died from phthisis, and the second, third, or fourth son, as the case may be, has as invariably come into possession—the eldest son and elder sons having died from the disease in a regular order of succession according to priority

of birth. The same phenomena have also been observed with respect to the daughters. Whether all this is founded on fact or not, or whether it can be verified or disproved by exact investigation, I must leave for the present. I give it for what it is worth, merely observing that any series of incidences of phthisis exhibiting regularity and apparently occurring in obedience to some law in each generation may easily be conceived, and admit of ready explanation by the view of a bacillary malaria existing in and restricted to particular bedrooms; and that neither such cases nor any of the cases of a similar nature anywhere cited can be adequately explained by heredity or contagion.

Atavism has been adopted by Dr. R. Thompson ("Family Phthisis," Dr. R. Thompson: 1884) as a way of accounting for the non-transmission of hereditary influence to certain members of "consumptive families," and for the non-occurrence of phthisis in one generation or several generations of a phthisical family. When a strain of phthisical blood is found to be silent for two or three generations, and then to assert itself loudly and with cumulative effect in the descendants, it is plainly atavism, for there are numerous instances of it, and Darwin and the naturalists call skips of this sort atavism. But, seeing that the parasitism of the tubercle bacillus had been demonstrated to be, and was recognized as, the specific cause of phthisis, it is somewhat difficult to see where atavism comes in—unless, by ignoring the bacillus, Dr. Thompson intended to convey his dissent from the view that the parasite causes the disease. If he did not mean this, the question then arises whether atavism in phthisis refers to the host or the guest; and until this point is cleared up, the question may be allowed to stand over.

In one matter involving the house the contagionist has

made out a *primâ facie* case; namely, the implication of the sound wife by the phthisical husband, or the converse. Many instances are recorded where marriage with a consumptive has been followed by the sound mate—the sound wife more frequently than the sound husband—falling into consumption; and these phenomena are held by some etiologists to indicate direct contagion, and by others to be the results of exposure to the same conditions that caused phthisis in the first-affected mate. It has been brought out clearly, by collections of clinical facts, that the sound wife is more liable to contract the disease than the sound husband is. In England, one of the principal exponents of contagion in this matter is Dr. Hermann Weber, and in the Croonian Lectures delivered by him (*Lancet*, March, 1885) he observes, "On the question of contagiousness of phthisis, the views of medical men will probably remain divided for a long time to come. The facts brought forward by Drs. Theodore Williams, Pollock, and Andrew are very encouraging, and we may be sure that phthisis is not in the same way communicable as scarlet fever and small-pox are. But we must bear in mind that there is more intimate intercourse between husband and wife, and near relatives sleeping in the same room, and even in the same bed, with consumptive persons, than between nurses or doctors and patients; and that, besides, the hygienic arrangements at the Brompton Hospital, and at some of the best general hospitals, are much better than those of many small private houses. Since I read a paper on the "Communicability of Phthisis between Husband and Wife" at the Clinical Society more than ten years ago, I have not met in my own practice with any such striking cases as those which I then related, so that I am happy to think they are rarer than it seemed to me at that time; but I remain convinced, from clinical facts, of the communicability of phthisis under

certain circumstances, and the experimental researches of Tappeiner, Veraguth, and Schäffer ought not to be forgotten. In preventive medicine, therefore, the infectious character of phthisis ought to lead to certain precautions. . . ."

Without going deeply into pathological questions, it is to be observed that it has been recognized that tuberculosis may certainly be communicated where local tuberculosis of the genito-urinary organs exists. It is of course possible, in the case of husband and wife, and it may have happened, that a quasi-traumatic tuberculosis has been induced, and that the bacilli may have gotten into the lungs, as well as other organs, by way of the blood-stream, and that a form of galloping consumption has occurred concurrently with the general tuberculosis. Such cases, however, must be exceedingly rare, and need not enter into the question here. Our inquiry is simply whether ordinary phthisis is ever "caught" from a consumptive husband by a sound wife through her inhaling bacillus-spores given off from his organism, or, conversely, whether the husband can "catch" phthisis in the same way from the consumptive wife. This is the plain issue. The fact that when the disease exists in the one it not unfrequently declares itself in the other, is undisputed. What is required is the explanation of the fact. Is the second affection dependent or consequent upon the first? Or are both affections due to the same or a like cause, and therefore independent the one of the other?

It will be obvious that I conceive that husband and wife are both infected (almost invariably) by the one local malaria. The more frequent subsequent infection of the wife, however, lends strength to this view. Writers have assigned several good reasons for the proportionately greater number of instances in which she succumbs, such as her indoor and sedentary occupations, and her long and ex-

hausting attendance on the sick husband—attendance which is not so frequently given by the sound husband to the sick wife. These conditions, all pressing more severely on the wife, point significantly to greater exposure to a local bacillary malaria. But there is one social factor which has not been noted. It happens that the man generally takes the woman to his own house. It is seldom that the husband goes to live in the house occupied by the wife before marriage. And it is this custom, probably, which largely influences the results, for a host of household arrangements and worldly concerns come into play. A man succeeds to an estate, for instance, and goes to live in the house on it a year or so before marriage. Another, living in his own house when he marries, has found it necessary to give up his bachelor's quarters and to occupy another room. A third may have taken an old house and furnished and lived in it. A fourth may have built a house. A fifth may be compelled to take his wife to whatever house or lodging he has, and so on. It may readily be perceived, therefore, that in so many thousand marriages, it will happen in a certain number of instances that the arrangements for the marriage end by the man being exposed (inferentially) to a local bacillary malaria, in the room to be afterwards occupied by the married couple, for several months, or a year, or longer, before the marriage; and that, although phthisis may not have declared itself in him at the time of the marriage, it may develope shortly after, or in a few months, or not for a year or two (according to the degree of infective power of the bacillary malaria and the seasonal conditions affecting the bacillus-vegetation). In any case, the long prior exposure he has had will almost certainly insure his being attacked first; but the wife is likely to be infected in less time than the husband, because of her greater exposure within a given time. For the same reason when the pair

commence married life by both occupying a malarious bedroom which neither had occupied before, the probabilities are that the wife will succumb to the parasite first.

Dr. Weber's cases (referred to in the Croonian Lectures), in which consumption, as he infers, was communicated in wedded life by purely contagious processes, will admit also of the interpretation that they had a malarial source; and some of them disclose this source very plainly. I can only find space to refer to the one case which has been so much commented on, but it is a typical case, and will serve for all. The case, abridged, is this: A young man lost his mother, two brothers, and a sister from phthisis; twice had hæmorrhage from the lungs himself, but got well, and married at twenty-seven. His wife, in good health then, died of consumption after her third confinement. He again married an apparently healthy woman, who died in a year of galloping consumption. He then married a third wife, a healthy young woman of twenty-five, of an exceptionally healthy family. During her second pregnancy she developed symptoms of phthisis, which ran a rapid course, and her death occurred in eight months. He married a fourth time, a perfectly healthy young woman, twenty-three years old, with healthy family antecedents. Three months after her first child was born she showed symptoms of phthisis, and after two sea-voyages she died, after an illness of nine months, with tubercle in the liver, spleen, and intestines, as well as in the lungs. The husband, who was a sailor, remained in apparently good health until he had to lie by on account of a severe fracture, when he contracted phthisis, and died within two years.

This unique case has, at first blush, a suspicious look of consumption *a sanguine* in the wives. The fact that such a succession of instances of phthisis should have occurred in the four wives of one man certainly lends transient

colour to the view that the husband was the principal, if not sole, agent by which the bacillus-spores were transmitted; but even the short account of the phenomena enables us to look beyond this superficial view. The history of the man himself suffices to show that he was free from active local and general tuberculosis from the period when he had the second attack of hæmorrhage in his youth, until his middle or old age—whichever it was—when he contracted phthisis and died. With the exception of the second, it is perfectly clear that the wives contracted phthisis in the ordinary way by the bacillus-vegetation passing through the air-passages: and with regard to the second wife, there are no valid grounds for assuming that her galloping consumption was started by general tuberculosis. On the contrary, the plainly indicated commencements of the phthisis of the first and third wives almost force the conclusion that the phthisis of the second wife also originated in the lungs; for it is in the last degree improbable that the husband contracted a local tuberculosis after the death of the first wife, and recovered from it before the marriage with the third wife. If we can thus eliminate the element of marital intercourse, the occurrence of the disease in all the wives is reduced to an occurrence of ordinary pulmonary consumption; and if we then couple the fate of the mother, two brothers, and sister of the husband, and the husband's first attack before he went to sea, with the subsequent fate of the wives and with the final death of the husband, we have very strong presumptive evidence that all these deaths from ordinary consumption were due to the same local bacillary malaria.

Apart from the human interest that attaches to a case of this kind, it has a high etiological interest. I can but glance at a few of the instructive inferences to be drawn from it. The house, or malarial shambles, rather, in which

all these persons were slaughtered, it is to be inferred, was exceptionally damp, from some extrinsic or intrinsic cause, throughout, and the bedrooms—especially the husband's bedroom—were unusually dark, and contained, consequently perhaps, an excessive amount of organic impurity. Perhaps, however, the usual quantity of organic matter in the bedrooms, together with very dull diffused light and an extreme degree of moisture from swampy ground, with artificial warmth, might account for the obviously high degree of virulence of the resultant bacillary malaria—a degree which implies far greater vitiation of the atmosphere than obtains in the bedrooms of the higher classes; a degree quite, if not more than, equal to that in the very poorest and most squalid of underground dormitories in England. From some cause the bacillus-vegetation in this house was developed in almost tropical luxuriance, and held its ground with singular persistency, as may be inferred from early lodgment, rapid caseation, and galloping consumption.

But the chief etiological interest in the case centres in the husband. He contracts phthisis, or gets into the stage of early tubercle-formation, but goes to sea and is cured. Dr. Weber, we observe, describes the disappearance of the symptoms as an "abeyance" of the disease; but I accept Professor Flint's view that such cases are veritable cures by "self-limitation." The assumption that the encapsuled bacilli may be set free, may commence vigorous parasitic life, and cause serious lesions in the lungs, may stop an etiological gap, but the probabilities are greater that a recurrence of phthisis and the occurrence of recrudescence depend on a reintroduction of bacillus-vegetation into the lung from without. This is to be inferred from the case of this sailor, who was not efficiently reinfected until he was accidentally laid up, and thus had to succumb through exposure to the malaria which had infected his four wives. But for this

accident, he might have continued his seafaring life to old age without a return of phthisical mischief. That the cure was a valid one is further evident from the fact that he was largely exposed at times to the local cause which involved his wives. First and last, he must have inhaled a large volume of the air in his house without being reinfected; and, whether we regard the local contamination of the house as dependent on bacillus-spores derived solely from the infected persons who had inhabited it, or look at it as due to a malaria caused by a free growth of bacillus-vegetation, the fact that this sailor remained free from consumption whilst his wives were successively dying of it, shows almost conclusively that he had radically cured himself by going to sea.

Without, however, stopping to consider the cure itself, for it was so plainly due to his avoidance of the malaria of his own house, we will just glance at the striking and significant exemplification afforded us of the slight tenure on which the "phthisical" and "anti-phthisical constitutions" are held. Here we have the two forms of constitution brought into juxtaposition in the same house, and so contrasted that the relative value of each form may be readily appreciated. We see four singularly healthy young women, without a phthisical flaw, carried off by consumption, while their husband, phthisical before marriage, and of pronounced phthisical antecedents, cures himself by going to sea, remains well through all their illnesses, and then, when compelled to leave the sea and lay up, is once more brought within the influence of the malaria, and dies of phthisis at last, though not without a stout struggle of two years. The mode of his death accentuates the cure, and, taken in connection with the history of his life, fixes reinfection on the house.

I must leave several minor points of interest in this

remarkable case, but will add that though it may not contain in itself sufficient to establish that the phthisis which occurred in this one house was caused by a malaria from bacillus-vegetation growing and multiplying in the rooms, yet, when the facts are placed side by side with the facts and deductions relating to the houses of "consumptive families" which *are* sufficient, I submit, to sustain the inference of a local malaria of this nature—the two sets of facts are found to accord perfectly; whereas the difficulties in the way of showing that the phthisis in this house occurred from bacillus-spores derived from tuberculous organisms incapable of growing and multiplying in the rooms are, I conceive, insurmountable.

In Klein's paper in *The Practitioner* for August, 1881, on "The Etiology of Miliary Tuberculosis," are some cases of tuberculosis ending in phthisis which would appear to have been directly communicated from person to person. Guérin's case seems to refer to tuberculosis only. Villemin's cases, however, include phthisis, but they are similar to Dr. Weber's cases, and call for no special remark. In these and all other cases relating to husband and wife, the unknown quantity of the house and local bacillary malaria has not been taken into account.

There is one aspect of the husband and wife question involving the dwelling-house which, though important to the etiologist, has not been brought out into strong light by the faculty. The rays from it are invisible, but they are made fluorescent by sociological means—such as the chatty reminiscences of an old country physician, or even of some elderly man of the world with a retentive memory and a knowledge of everybody's family history. Though there are no clinical records, we might learn from these other sources that there is another side to the ghastly picture of the four consumptive wives. We might hear of

young women whose fate was looked upon as sealed, for some of their brothers and sisters had died from phthisis, and the seeds of it had manifestly been sown in them, but who have married and, contrary to the predictions of the family physician and the fears of the friends, have recovered perfectly and reared families of healthy children. Such facts do not reach the case-book, but they are not to be ignored. There are no statistics, but we may get a fair grip of the subject, nevertheless.

Within this century many thousands of young women belonging to "consumptive families" have married in England, but we will restrict the view to the marriages of the higher social classes. It is well known that many of these so-called "tainted" families have intermarried, and that "collaterals" abound. In the south and west, and in the midland counties, indeed, there are few families whose blood is not "tainted" more or less. The further north the less is the proportion of this "taint," and in Scotland it is still less. Now, although the greater proportion of these marriages have unquestionably resulted most unfortunately, sometimes by husband or wife or both dying before a child is born, sometimes by several of the children becoming phthisical, and sometimes by one or both parents dying and leaving their young progeny: yet it is no less unquestionable that many of the mothers of tuberculous and consumptive children have themselves been rescued from imminent death from phthisis by marriage into even closely allied families. The ethics of these consanguineous marriages, either in the past or the future, we are not concerned with. What is required is the explanation of the fact that a young woman in the stage of "physiological misery," or beyond it, who marries a first cousin shall get well, bear children, live to old age, and die from some disease other than phthisis.

And I say unhesitatingly that exhaustion of all the

conditions by which the differentiation before and after marriage can possibly be brought about, leaves but one sufficient cause for her recovery, namely, her withdrawal from an atmosphere holding sufficient bacillus-vegetation to induce efficient infection, and her removal to an atmosphere either free from the vegetation, or holding it in such scant proportion as to be innocuous even to one in whose lung parasitism had commenced. This, I submit, is a forced conclusion. If we are on sound ground here, it follows that the change in the air she inspires is a consequence of the change of residence. It is an inferential certainty that she was breathing an efficient malaria in the paternal house, and that she is outside the sphere of an efficient malaria in her husband's house. And from this fixed point we may not only safely infer the nature of her environments hitherto, but the nature of her changed environments, and we may predict the future fate of her children, so far as tuberculosis is concerned, with unerring precision for any number of generations. Her bedroom in her father's house was deficient in chemical light. In the husband's house all the rooms she occupies, at all events her bedroom, dressing-room, boudoir, etc., are probably on the south and east side, with ample windows, and are provided with sufficient light of the requisite chemical intensity to keep the air surrounding her at night sweet and wholesome, or free from bacillus-vegetation. Here the lesions of one or both lungs, undisturbed by frequent invasions of detached bacillus-vegetation, are repaired as soon as the parasitism of the bacillus-forms then in possession comes to an end; and this may soon happen through their death or sterility, even where, as has been seen, large masses of tubercle, or even cavities, are present. From this time, if not absolutely consumption-proof, she will be free from phthisis so long as she shall inhabit the same or similar rooms.

If the nursery and children's bedrooms are equally favoured with her own in the matter of sunlight, her offspring will grow to maturity without a sign of tubercle in spite of their double heritage of "taint;" but such favourable conditions for all the branches of a large family obtain in but a small proportion of the greatest houses. It has happened in many instances, no doubt, that whole families have been so exceptionally well placed in regard to light, that they have been reared, consequently, free from scrophula and consumption; yet in the majority—the large majority—of cases, in some parts of the country (especially during the first half of the century), some one or more of the rooms set apart for the younger children and for the youths have received an amount of chemical light far below the standard. It has not been thought of any importance to give the young folk sunny chambers. If, therefore, some of the minor bedrooms in the house of the matron who has been snatched from untimely death by union with her cousin be sunless, she will to a certainty see one or more of her sons or daughters going the way of her brothers and sisters. It may be her eldest or youngest born children, her sons only, or daughters only, that perish, as the rooms may be accidentally apportioned; or all may die, if placed in a wing of the house overshadowed by other portions of the building. The healthy survivors, if any, fulfil their career and marry, and the mother becomes a grandmother, perhaps a great-grandmother; and in each generation she will see, probably, somewhat similar gaps left by phthisis in the families of perhaps all her descendants. Thus the circle of "collaterals" widens, and the instances of atavism accumulate.

As the alliances made by the family have been amongst the titled or wealthier classes, we shall find in all its branches, if we bring its history down to the present day,

a very marked proportionate decrease in the "scrophulous," or tuberculous, affections, and in the mortality from consumption during the last twenty-five years. This result is due to a decrease in the sum or value of the bacillus-vegetation surrounding these classes; and it is evident that this decrement has occurred through the agency of many factors, the principal of which have been the increased facilities for travelling and the consequent more frequent change of place, the greater fondness for outdoor sports and games in both sexes, the more enlightened (though still empirical) curative and prophylactic treatment, but above all things by the "modernizing" of old country seats, and by the tendency of architecture to more light than formerly. English folk during the last two or three generations have been slowly but surely acquiring a taste for brighter and more cheerful surroundings. When old places have been restored or added to, or when new houses have been built, bay windows, perhaps, or "Queen Anne" or French windows, or longer, wider, and more sashes of some kind have been introduced, and in most instances a southerly aspect, when practicable, has been selected. All these factors collectively have either converted a proportion of deadly into salubrious bedrooms, or have resulted in many newly built bedrooms being fashioned on sound, wholesome principles. And this deduction as to the causes of the decrease of phthisis may not alone be made from the phenomena of the incidence of phthisis in this class of habitations, but also from the phenomena of the diminution of phthisis amongst certain sections of the community—soldiers, for example, whenever their dormitories have been changed from sunless into sunny rooms—the diminution being in a ratio with the chemical value of the solar radiation admitted into them by the changed conditions.

Many other facts bearing on the question of heredity,

whether in its true sense or in its modified meaning, might be here adduced in support of the contention that the bacillus is not handed down in any shape or form from parent to child, and that the tendency or influence acquired from parentage, admittedly great, has cut both ways; so that, while on the one hand the issue of some progenitors have inherited both the physical and the moral attributes which conduce to their infection and reduce their power of resistance, on the other hand the progeny of other consumptive parents, or parents coming of "consumptive families," are of a totally different breed to the former class of descendants, and are splendid young men and women, and as "anti-phthisical" in every way as they can be. Further evidence could also be brought forward to show that the real influence of parentage in the matter of phthisis has consisted principally, if not wholly, in bringing together a number of human beings under one roof, and not in transmitting particular qualities to the offspring. All the phenomena relating to the incidence of phthisis among the servants, retainers, and tenants of the old "consumptive families" would throw strong light on the concurrent incidence of phthisis in the families. A multitude of facts connected with what is called "acquired phthisis" might serve to bring out more clearly, perhaps, how illusory is the "phthisical constitution" or the "hereditary influence," and how, therefore, the distinction between "acquired" and "hereditary" phthisis is verbal and unsubstantial, and drawn for convenience sake. But I have produced facts of such weight and significance that more would be surplusage. If the data already submitted do not warrant the conclusions, there is an end. I have made my position good, or it is radically bad; and in either case it would be profitless to produce more data.

Before leaving the subject of heredity, I would observe

that I have dwelt at length on the clinical facts relating to the occurrence of phthisis in successive generations of the families of the upper classes, because they have a larger value, etiologically, than all other clinical facts relating to the occurrence of phthisis put together. By confining the inquiry for a time to these classes, we are able to exclude many possible factors which tend to obscure etiological speculation. We can at once exhaust out all visible, recognized unsanitary conditions—the ingesta, trades, or occupations, destitution, and most of the factors which at one time or other have been suggested as remote or proximate causes of phthisis. We see at a glance that the bacilli in the lungs of patricians cannot be there as a consequence of such surroundings as those of the masses. Want and misery, dirt, overcrowding in dark alleys and cellars, etc., are clearly not essential, therefore, to the existence or to the dissemination of the bacillus-vegetation. By keeping steadily in mind that we have to bring out a common cause by which the upper and lower classes are placed on an equal footing as regards the bacillus, we may narrow down the issues to four, viz. true heredity, hereditary influence, contagion in the shape of sputum, and a local bacillary malaria from a free-growing vegetation. And as the first and second of these may be got rid of without much trouble—for it is palpable that no amount of influence, or habit, or proneness, or vulnerability, or of any transmitted quality or "taint," will suffice to cause phthisis without an introduction from without of the bacillus-vegetation—we are driven for our sufficient cause to one of the two things—contagion or bacillaria. This residuum must remain from all etiological analyses of phthisis, but we get it more rapidly and simply by the agency of the English country-seat of the old families. The initial difficulty of reducing some of these habitations to the level of some of the lowest abodes, and of raising

some of the filthiest hovels infinitely above some of the stateliest piles (so far as the risk of acquiring phthisis by residence in them is concerned), is overcome by the view of chemical light. With this key we may go through every dwelling and lay our finger on the plague-spots; but, what is of more importance, whilst we can point to the home of the bacillus-vegetation with even greater certainty in the dwellings of the old families than in the lowlier dwellings in their neighbourhood, we can also more readily get rid of heredity and contagion in the old families.

I find that all clinical facts are in accord with a non-contagious cause of phthisis, though a large proportion of them may be made to accord to some extent with a contagious cause as well—hence, of course, the extreme diversity of opinion; but the clinical facts to be gathered from the phthisical records of these ancient family mansions are, I submit, repugnant to, and subversive of, the doctrine of contagion, and that of heredity as well. Inherited bacillus-tainted blood, even if it be a possibility, and bacillus-spores derived from sputa, etc., viewed as causes of phthisis, are both alike incompatible with these clinical facts.

Koch has protested that "the validity of his views is not to be shaken by clinical facts" (*Lancet*, March 15, 1884, p. 481), and considers that the results of "clinical observations" are "inconclusive and uncertain" (*Ibid.*, p. 493). But this attitude would appear to have been taken up rather late, unless it is intended to intimate that clinical facts and observations are "inconclusive and uncertain" when used by others. For with regard to his objection to this class of facts, it is to be observed that Koch himself has not omitted to use them freely, and that all contagionists rely upon them also, to show how the bacillus-spore travels from organism to organism; and it is not apparent why they should not pass current—with the usual discount—when

put into circulation by the anti-contagionist. Further, it may be remarked that the actual value of well-observed clinical facts is far higher than that of an inference welded to a demonstrated fact in such wise that the inference seems to be a part of the fact, or is accepted as having been demonstrated as well as the fact. I have no intention of detracting from the value of Koch's discovery, as will have been seen. His feat in micro-pathology was a great effort, and I do all honour to it. But Koch's etiological and botanical views have not been demonstrated. His experimental research may have been perfect, but it does not follow that all his conclusions from the results are sound. Because he tacked on to a brilliantly demonstrated fact of the highest order an inference from another fact of minor, if not of small, value in itself, an inference raising issues distinct from those which were settled by the determination of the primary question in his inquiry, that inference is not to remain unquestioned through all time, as though it had the same force and validity as an inferential certainty. To have that rank, it must go through the fire; it must be shown to be a forced conclusion, and certainly the conclusion is not forced from the isolated premiss from which it is drawn. Unsupported by mycological and biological facts, this inference of Koch's is at least as "inconclusive and uncertain" as are the inferences to be had from well-garnered clinical facts.

Notwithstanding the contempt of the pathologist for clinical observations—when they do not chime in with his views—the etiologist will hardly be deterred from utilizing them; for, though clinical facts may be, and have been, misinterpreted at times, and even twisted and tortured to fit in with particular doctrines and dogmas, the facts themselves are unharmed, and stand as firmly as the micro-pathological facts which may have led to false conclusions in their day.

Moreover, the clinical facts as to phthisis are so plainly ear-marked by the occurrence of the manifestations of the disease in regular order and in a similar manner through successive generations of the same family. This is a special feature, indelibly stamped upon this set of clinical facts, which the rough handling of controversialists cannot disturb. Such facts must have their meaning, and are not to be discarded in any study of phthisis.

On the contrary, it is clear that any views of the causes of phthisis which are not comprehensive enough to take in all the clinical facts connected with its incidence, and to reconcile every one of them with every other of them, and with all micro-pathological and other facts, are unsound. And by virtually admitting that his etiological views are not to be reconciled with the clinical facts, Koch has furnished one of the strongest proofs that the views themselves are at fault. If the conception he formed as to parasitism had been sound, it would have been sufficiently large to have included and accommodated clinical and all other facts readily enough, and would have been accepted by the world accordingly. That it has not done this shows that his conception is out of joint with the facts. Therefore it is that etiologists all the world over persist in testing Koch's views by the aid of clinical, in default of botanical, facts; and sooner or later he will find that either he must prove that the ordinary phenomena connected with the incidence of phthisis have not occurred, or have not been correctly described by observers hitherto, or he must reconstruct his views so as to square with these phenomena.

OCCUPATION.

One thing which has long been observed to affect the incidence of phthisis is the trade, work, calling, or occupation of a person. The facts in this matter are all significant, and all jump with the view of a local bacillary malaria. Setting aside the knife-grinders, potters, and other workers whose lung-affections are caused principally by irritant particles, and may, or may not, be complicated by lodgments of bacillus-vegetation, it has been found that some occupations conduce to phthisis and others shield from it. The mortality from consumption, for instance, among tailors, straw-plaiters, lace-makers, seamstresses, printers, servants, and others is far larger proportionately than among tanners, butchers, smelters, soap-boilers, and others. Sailors who follow the sea regularly, or who do not take long spells ashore, are largely exempt from the malady.

The conditions surrounding the tailor, the seamstress, and the like classes, in large towns especially, are well known. The close, dark workshop and the underground dormitory furnish abundance of soil and the required conditions for the growth of the bacillus-vegetation. In such places the main difficulty lies, not in accounting for the occurrence of consumption, but in seeing how such a large number escape death from it. This etiological point is, indeed, hard to clear up thoroughly by the view of a local bacillary malaria, but the attempt gives some in-

structive results. It may be inferred—1. That the perfectly or nearly still air of midnight affords the most favourable conditions both for the growth of the vegetation and for its suspension in the air; and that, consequently, the bustle and stir in a workshop, even by gaslight, or dull diffused light, in the daytime, will materially reduce the infective value of the malaria. 2. That many of the people who work in such places sleep in high garrets, where the altitude and the sun together keep the air free from the vegetation in efficient quantity. Yet where persons like compositors work all night in dirty offices, and sleep in the day in rooms often darkened purposely to ensure sleep, the implication of so few, relatively, of these night-workers is only to be sufficiently explained by the assumption that the infective power of the local malaria is diminished by conditions not yet taken into account. 3. That, whilst the atmosphere of London and some large manufacturing towns holds finely divided solids which largely absorb the chemical energy of the ultra-violet rays, this favouring condition for the vegetation is counterbalanced by other conditions inimical to the vegetation in these towns. These conditions cannot be determined, but among them, probably, are the presence in the air of dwellings of the products of the combustion of coal as fuel or as gas, and the general reduction of moisture in the soil and in the houses both by the interception of the rainfall (by roofs and rapid drainage) over large areas, and by the drying effect of fires and of heat-radiation from brick walls. Such conditions, together with several minor conditions, may be assumed to keep down the crops of bacillus-vegetation. 4. That social and other conditions in large towns—the consumption of alcohol, for instance—may tend to lessen the mortality from phthisis.

The question of alcohol in this connection was gone

into some few years ago without very definite results, because of the tendency of inquirers to look beyond the abstract in this kind of investigation. Whether a moderate or an excessive quantity of alcohol, by bacillicidal or other properties, protects the London artizans and the classes below them against phthisis to any appreciable extent, we shall never learn, probably; but that the quantities of beer and spirits consumed in London, at all events, lessen its death-rate from phthisis, by the substitution of other causes of death, may be rationally assumed. Many a wretched workman, stricken with "physiological misery," takes to dram-drinking. Whether the alcoholization of the blood destroys the guests, or prevents further parasitism, is not now in question. Perhaps it may have both these effects, but, at all events, the practical result is that they who resort to this mode of relief frequently end by accidental and violent modes of death through *delirium tremens*, etc., or more commonly by diseases of the kidneys, liver, etc., and so the death-roll of phthisis is diminished.

5. That "self-limitation," or the death or expulsion of the bacillus, from some cause occurs more frequently amongst these classes. Possibly they move about more from place to place, and change their dormitories oftener than many classes. Other causes besides these enumerated might be assigned, but if we put them all together, it has to be admitted that the low mortality from phthisis in London and some large towns is not yet fully accounted for; though, at the same time, it has to be observed that this inability to explain it thoroughly or satisfactorily does not involve the proposition that this low mortality is incompatible with the assumption of a bacillary malaria. The soundness of the main inferences is untouched, but an element of uncertainty as to the precise conditions that govern the value of the malaria is introduced.

But let us try to get at the explication of the same phenomenon by the theory of sputum-dust. Presumably there is more sputum-dust in London than in the country, and more in its workshops and dens than in the country houses of "consumptive families." We know from Koch that the spores of the bacillus are tenacious of life, even in the presence of decomposing material, for a long period, and that they cannot multiply outside the body; therefore they are unaffected by local conditions. It may be assumed that the greater the disturbance of the air the greater the number of spores put into circulation, and that, consequently, they are distributed more effectively by day than by night. All things considered, no more favourable conditions for the spread of phthisis by contagion can possibly be supposed than those to be found in London. How is it, then, to be accounted for, on contagion principles, that the death-rate from phthisis of "consumptive families" in the country is, or has been, higher proportionately than that of dwellers in the lowest quarter of London? If we are told, as we might be by some etiologists, that the differentiation is the result of hereditary influence, then, shifting the point, let us change the venue to such cases as Dr. Weber's of the four wives of the sailor, Dr. Andrew's of the three apprentices to the dressmaker, and to numbers of others in which "acquired" phthisis has occurred wholesale. Having thus got rid of heredity, we then ask how it is to be explained, if sputum-dust is the common source of phthisis, that, whilst thousands shall be exposed to an atmosphere loaded with the dust and only a small portion shall be efficiently infected, a few persons living in an atmosphere holding a far less amount of the dust shall all be implicated and shall all die of consumption?

The conditions under which factory and mill hands in some places have suffered largely from phthisis are in strong

contrast to those surrounding the classes specially subject to the disease in London. The mills are well lighted, airy, and clean, and the sanitary arrangements are, as a rule, good of their kind, but the hands have probably been domiciled in underground dwellings. Here, again, the problem of the existence of any of these people beyond the consumptive age is not easier to solve completely by the view of accidental parasitism, than is that relating to the incidence of the disease in London, whilst it is simply impossible to solve it by the view of pure parasitism.

Some of the instances of exemption from phthisis of persons following certain callings are curiously instructive. Those mentioned by Dr. Andrew in the Lumleian Lectures point clearly to the destruction of the local bacillary malaria. The ultramarine manufacturers are said also to be exempt—probably from a similar destruction.* Dr. Bielczyk reports that the workmen in petroleum wells "are remarkably free from diseases of the respiratory organs, both of an inflammatory and a tubercular character" (*Lancet*, December 18, 1886). The inference here is that the vapour (and perhaps the oily nature) of the petroleum precludes other forms as well as the bacillus-vegetation. "Your tanner," too, enjoys an immunity, and he is obviously sheltered by the envelopment of everything about him in an atmosphere redolent of tan, which is inimical to the growth of many low forms; and at night his clothing, saturated with the fluid in the pits, keeps his bedroom, and probably his whole dwelling, free from the bacillus-vegetation; and if his wife and children also escape—which is an inference from the fact that the inhabitants of the copper-smelting town of Röros

* "*Alleged Immunity from Consumption.*—At a German ultramarine manufactory the director has observed that for forty-four years none of his workmen have ever suffered from consumption. He attributes their immunity to the fact that the process of manufacture involves the constant production of sulphurous acid by the burning of sulphur."—*Medical Times*, July 14, 1883.

are entirely free from phthisis—we see clearly the cause of the exemption. There is no other explanation which will cover the whole ground. In occupations in which chemical compounds shield the workers, the explanation of the phenomena is easy enough by the assumption of the preclusion of the growth of the bacillus-vegetation; but it is impossible to explain the phenomena by the view of pure parasitism. The germination and growth of the vegetation on its soil are essential to the view of a local bacillary malaria; but, as the natural development-process of the spore occurs, we are told, in the organism only, conditions which may dissipate or destroy the soil of the vegetation, and thus shut out the malaria, have no effect upon the dried spores in sputum-dust. These tenacious spores are not injured even when brought into intimate relations in fluids with certain chemical agents destructive to many low vegetal forms. The presumption, therefore, is that an atmosphere containing sulphurous or other vapours in such amounts as to be harmless, or not rapidly injurious, to animal life, would not destroy the germens of these spores in their dry state. For all the theoretical and practical purposes of contagion, therefore, there is no sufficient reason why sputum-dust should not be just as efficient in the houses of copper-smelters, tanners, and others as in houses where phthisis occurs. These non-occurrences of phthisis which furnish such valuable negative evidence for the assumption of an accidental, are a stumbling-block to the inference of a pure, parasite—as any contagionist will find who shall try to explain the freedom of Röros from phthisis.

Brief allusion may be made to the exemption of soap-boilers and others handling fats and oils. This class of phenomena points to a noxious effect to the bacillus-vegetation when brought into the presence of oil globules.

Cod-liver, dugong, olive, and other oils, cream, fatty substances, unguents, etc., which have been found more or less efficacious in the treatment or prophylaxis of phthisis, may owe their virtue to a direct bacillicidal property in the particles of oil sent into the blood-stream. We have not seen that any experimental research has been made in this direction, but from the facts alluded to, and from the well-known custom of many ancient and modern peoples, civilized and savage, to lubricate or besmear their bodies with various kinds of oils and fats, it is an inference that oil is obnoxious to this and perhaps to other forms of vegetation. The South Sea Islander may owe his protection to his cocoa-nut oil for the same reason that the English butcher's and soap-boiler's greasy occupations enable them to escape. This class of cases obviously serves both the contagionist and anti-contagionist.

The exemption of the sailor is instructive, and the cause of the exemption is the same in principle as the cause of the self-limitation of the disease in the consumptive sea-voyager—the absence of bacillus-vegetation in efficient amount in the air he breathes. But there is no hard-and-fast line between sailors and landsmen, and the exemption ceases to obtain either when the sailor spends a long time ashore, or when he is employed in short voyages or in small coasting craft moored frequently in quiet waters. Those on board stationary naval and other vessels or hulks, or naval vessels loitering about on a station and for a long time in port, have been found to contract phthisis. It is evident, therefore, that the bacillus-vegetation is not precluded or materially restricted by saline particles in the air, or by ozonic or other atmospheric conditions, or by the presence of the constituents of tar; but it is not so evident why ships on long voyages should be free from consumption. Possibly the rapid change from latitude to latitude and

from one hemisphere to another may, by changing the conditions or reversing the seasons, check the vegetation; but the disturbing causes may be the incessant motion of the vessel, the straining and vibration of all her timbers, and the greater change of air below the deck in the open sea. These may not seem very potent disturbing causes, but it may be assumed that even if they are insufficient to preclude the occurrence of the vegetation in the hold, or in the darkest and dirtiest parts of the vessel, yet they may have the effect of preventing the suspension of the particles of the vegetation in the air at night, in such numbers and value, and in such a way, as to become an efficient malaria to the sleepers in the cabins or in the forecastle.

This may not be a sufficient explanation of the non-occurrence of phthisis—or of its rare occurrence rather—in long voyages, yet it is not inconsistent with the view of a malarial cause of the malady. But the contagionist cannot explain either the occurrence or the non-occurrence, or the amelioration or the cure, of phthisis in vessels. Some etiologists, Koch among them, infer that a few wandering bacillus-spores entering the air-passages of a person are sufficient, or may be sufficient, to effect a lodgment in the alveoli of the lung and cause phthisis by their multiplication, and by the consequent formation of large quantities of tubercle. A few extreme contagionists, I observe, are deeply concerned and greatly exercised because some bacillus-spores have been found in the atmosphere of large cities. Koch has suggested this in his Address in the following words: "It can, therefore, be inferred that sputum which has dried upon the ground, on the clothes, etc., retains its virulence for a long time, and, *should it enter the lungs as dust, is capable of causing tuberculosis.*" Coupling this with what he says of guinea-pigs, namely, "The enlargement of the bronchial glands, and the evident

commencement of the process in the respiratory organs, leaves no room for doubt that spontaneous tuberculosis in these animals is caused by the inhalation *of a few* or *possibly only one single infectious germ*, and which, on that account, has taken a very slow course" (the italics in both quotations are mine), one may understand the uneasiness and alarm created in some minds. But if the contagionists do not all share this view (and it is not held now by the school generally, for the obvious reason that if it were sound one half of the civilized world would have cavities in six months, and the other half would be implicated in as many more); if they concur with the view that it takes a considerable number of spores, or a large amount of bacillus-vegetation, several weeks at least to cause ordinary phthisis; —then how, by that view, are they to explain a first occurrence of phthisis on board a man-of-war, on a station where there is but little cruising, or in harbour?

From what tuberculous organism, or organisms, is a sufficient number of spores to be got into a ship to account for the first case of spontaneous phthisis in her? Or, when several men in the ship are affected about the same time, and before a cavity forms in the lung of any one of them, where are the spores to be supposed to come from by which they were all infected? It would require some amount of sputum-dust to be deposited and left somewhere between-decks, to be converted into dust, to account for a continuous supply of spores to the lungs of several of the crew for several weeks. And by what possible combination of circumstances can it be imagined that such an amount could be got into such a position and left there for that time? It is absurd to suppose that the chance visits of consumptives from shore would suffice to provide the fund of spores required; for even supposing the visitors to expectorate freely and to be regardless of the proprieties for a

while, yet everybody who knows anything of a ship of war knows that indiscriminate expectoration would not be tolerated long, and that all evidence of an accidental introduction of sputum would quickly disappear. And it would take, moreover, the day's sputa of Brompton Hospital left for a week underfoot between-decks, to give a show of colour to the supposition that an occurrence of several cases of phthisis some weeks later in the ship was a consequence of the deposition of the sputa. In point of fact, the contagionist cannot conceive, or rationally assume, any mode by which an occurrence of this kind can be adequately accounted for by sputum-dust, even taking the infective power of the bacillus-spore at its present valuation. Sputum left about by phthisical patients anywhere has never been shown to be, and cannot be shown to be, a cause of phthisis, else nobody would be safe; but on board a man-of-war it is absolutely out of the question.

It may be suggested that a phthisical subject may be taken on board before caseation has begun, and that when the man is put on the sick-list for lung-cavities, his retention in the vessel may account for a constant supply of bacillus-spores to the crew. This will be met by pointing out that twenty or twenty-five years ago many consumptives made the long voyage to Australia in sailing vessels, the voyage then lasting from seventy to a hundred days. In many of these large ships, eight or ten or more consumptives have been distributed among the two, five, or six hundred cabin, steerage, and intermediate passengers. On the return voyage most of these ships brought back one or more of those who had either been cured, or had been benefited by the change; and both on the outward and homeward bound voyages consumptives occasionally died. The fleet of merchantmen in the Australian trade carrying passengers was a large one, and for a period of from ten to fifteen

years there was a steady emigration and immigration of phthisical patients; but not one single instance of phthisis originating on board any of these ships, either amongst the passengers or the crew, has, so far as I can ascertain, been recorded or heard of. Here, then, we have evidence of the distribution of enormous quantities of sputum-dust and no phthisis. For the steerage berths and quarters of a large passenger-ship give especial facilities for the collection and storage of a large amount of organic impurity, and, supposing an unfortunate and perhaps dying consumptive to be solicitous about cleanliness, how is it possible to be observed on all occasions, especially in rough weather, in such crowded spaces under such surroundings? The sum of sputum-dust contributed to the interior of one of these passengers-ships must have been many thousand times greater than that contributed to the interior of any British war-ship before the occurrence in her of the first case of phthisis. When, therefore, we reflect on the necessarily close relations of human beings on board ship, the non-occurrence of phthisis in the one class of vessels, and the occurrence in the other class, offer complex problems to be solved in accordance with the tenets of contagion.

Lunatics are specially liable to phthisis. With the key the explanation is at once to be had. Not only are the day-rooms and dormitories of asylums frequently constructed, for purposes of safety and through want of knowledge, so that a sufficient amount of chemical light is not admitted into them, but the demented and the melancholy will in some cases remain for many hours daily, for months together, in the darkest corners. In no places, perhaps, are there more loathsome evidences of a "poor smell" than in the associated dormitories of some public lunatic asylums.

Prisoners also have a large death-rate from consumption,

even in modern days, when their sanitary conditions are more carefully attended to. The filthy dungeon has been replaced by sufficiently airy and cleanly cells and dormitories, but there is generally a great deficiency of chemical light in many of them. This reminds me of the following announcement:—

"Whilst the theory of the communicability of phthisis is engaging the attention of collective investigators and scientific observers at home, it would appear that in Germany a more practical view of the question is being taken. The Minister of the Interior, in view of the prevalence of phthisis in prisons, has not waited for the final solution of the problem, but, having determined, in true judicial spirit, to give his prisoners the benefit of the doubt, has ordered the isolation of phthisical cases, the careful disinfection of linen and excreta, and all the usual sanitary precautions, such as are observed in the cases of the well-recognized infectious diseases in all the prisons and kindred institutions in Germany."—*Medical Times and Gazette*, February 23, 1884.

Perhaps no better illustration of the practical working of the theory of pure parasitism than this could be found. The isolation of the phthisical prisoners may have been an excellent thing for the phthisical, provided that the part of the prison to which they were sent received its full complement of solar radiation, and was not, therefore, subject to a local bacillary malaria. If the place to which they were consigned was absolutely free from the vegetation—which, however, is very unlikely—the sending them there would be equivalent, of course, to sending them to sea, or to an Alpine health resort. But as a measure designed for the safety of the prisoners from amongst whom the phthisical were removed, it was utterly futile, if the phthisis of the isolated prisoners had been contracted where the sound

prisoners were left; for the latter were in precisely the same position as before, unless the "usual sanitary precautions" included a disturbance of the field of bacillus-vegetation so great as to interfere with the yield, and thus to reduce for a time the degree of infective power of the malaria which had caused phthisis in the prisoners exposed to it. It is a comfort to know that this humane experiment could have done no harm, unless the unfortunate phthisical prisoners were removed out of a bad malaria into a worse one. As an effort to fulfil a specific purpose, this bit of hygiene is on a level with some of the highest flights in scientific sanitation.

In garrisons or barracks, and in some of the old endowed institutions for particular classes, and for the indigent and infirm, as well as in some of the parochial buildings for the poor, the mortality from consumption is largely in excess of that of the general population. In these and all similar phenomena of the incidence of phthisis upon a certain proportion of people living together under like, or apparently like, conditions, the cause of the differentiation through which some are infected and others escape infection, may be brought out by carefully examining into the conditions of their dormitories in respect to the admission into them of solar light. And inquirers will be greatly assisted in arriving at their conclusions in this matter, by taking into consideration the large number of instances in which a rapid diminution of phthisis has followed when light has been let into the sleeping-apartments of certain sections of the community by the demolition of old and the erection of new buildings, or by the widening of thoroughfares in densely populated towns. The marked decrease of the malady in all these cases is ascribed to the large admission of air, and to the better ventilation provided by these improvements, and the sun is supposed to play a secondary part in the results.

But, with a fuller knowledge, I conceive it will be found that the most perfect ventilation of a bedroom having treble the amount of cubic space calculated to be ample for the sleeper, or sleepers, in it, would not suffice to ensure them from phthisis, if its windows did not admit adequate solar radiation. Whereas in a bedroom one-fourth the size, with abundance of chemical light, whatever noxious effects the sleepers might suffer from a deficiency of air, phthisis could not be one of them. Besides, it is not to be overlooked that the sun is one of the greatest, if not the greatest, of the ventilating forces in nature, by virtue of his power of oxidizing, or of reducing or decomposing, the proteid compounds contributed to the interior of habitations. This is an effective ventilation in itself, as it ends by keeping the atmosphere of a room dry, thus establishing currents between the inside and outside air.

Throughout Europe, in most, if not in all, the educational establishments, public and private, for the youth of both sexes, there is a relatively large proportion of phthisis, both among the students and the teachers. This has been attributed to a variety of causes, and in some countries the State has intervened with regulations to meet obvious defects, with more or less salutary results; but, in the absence of the leading principle to guide them, the different Governments have mostly been wide of the mark. In the matter of precluding phthisis, the attention of inspectors has been given principally to the hours devoted to tuition and to recreation, and very valuable work has been done in the way of regimen and ordinary sanitation; but the hours of sleep have been in many instances left to the insidious vegetation in the bedrooms. Where inspection has shown that these rooms are cleanly, sufficiently ventilated, and of the regulation size, they are taken to have fulfilled all the requirements. Very little regard is had to sunning the bedroom,

and, indeed, medical men of high repute hold opposite views as to the necessity for admitting the sun into sleeping-chambers. Thus, in an interesting and useful work,* Mr. Brudenell Carter, in an article on "LIGHTING," says, "Of course, the artificial darkness must be exchanged, as soon as the period allotted to sleep is over, *for the freest possible admission of sunlight, in order to obtain its purifying influence.*" In the same work is an article on "THE NURSERY" by Dr. William Squire. Speaking of children's bedrooms, Dr. Squire observes, "No bed should be placed just between the window and the fireplace in a bedroom, and the windows in the day rooms should be large and low; in the bedrooms the old-fashioned broad window, either side made to open, or the dormer window, might reappear. *In the bedroom less light is needed,* more care is required as to draught, nor do children want to be constantly looking out of window. . . . The windows in the summer can be left a little open at the top. They should be *provided with shutters,* both to keep off draught *and to shut out some of the light when this may be necessary.* They aid materially in lessening the chill that in cold weather strikes in from the windows, and then require the aid of curtains for further preservation of warmth. A stout linen or jute fabric makes a good protective window-curtain for the winter."† Mr. Carter and Dr. Squire blow hot and cold on this vital subject, and it is no great wonder if some doubt and hesitancy exists as to the proper mode of dealing with dormitories in schools and colleges in the minds of those charged with inspecting them. It is time, however, that such discrepancies amongst the heads of the profession disappear, and that some definite understanding on the question of light in dormitories be arrived at.

* "Our Homes and How to Make them Healthy," Ed. by S. F. Murphy. 1833.

† I have italicized in these quotations.

Some other classes, as well as those which have been referred to, also suffer from phthisis disproportionately, but with the indications as to the source and nature of their infection, and as to the manner in which the supply of the infective agent is governed by the sun, all the various incidences of the disease may be disencumbered from the obscuring questions relating to contagion that still cling round them.

DEBILITY.

FORMERLY etiologists were so strongly impressed with the fact that the weakly are prone to fall into consumption, that they saw a causal relation between a loss of vital power and phthisis. The view was still clung to by some until Koch's discovery made it clear that although a system reduced by bad or insufficient food, by excesses, by illness, by injury, or by any other means, will succumb, sometimes very rapidly, from an attack of phthisis, yet that the lowest degree of long-continued debility cannot terminate in phthisis in the absence of the bacillus-vegetation, and without the actual lodgment of the parasite within the lungs.

Debility is the prolific parent of "acquired" phthisis, though the disease may be "acquired" by the most robust. Yet even then the stage of "physiological misery," which is reached just before or just after parasitism is established, or about the time it commences, is only another form of that debility which would seem to be a necessary condition precedent to the efficient lodgment of the bacillary guest. Hereditary influence, too, as I have endeavoured to show, consists in reducing the future host to the requisite degree of debility by bringing him into the presence of the malaria in the family house, and keeping him there until he can no longer withstand the infection. It matters not how the body is rendered vulnerable, and all forms of debility are alike in principle. The invalid suffering from exhaustion,

from injury, or from loss of blood, and who has landed himself *per saltum* in a condition of "physiological misery," is just as certain to contract phthisis, if brought within the sphere of the malaria, as one of the most "tainted" of the "consumptive race." The "physiological misery" induced by debilitating causes simply, or without the intervention of the bacillus-vegetation, is on the same plane with the "physiological misery" brought about by what is called "hereditary tendency," or the "phthisical habitus," or "phthisical constitution," etc.; and the only distinction between them is that, if a representative of each form of "misery," each in the same stage or degree of debility, were submitted to the same malaria holding bacillus-forms under the same conditions, or so that each one inhaled an equal sum or value of the vegetation during the same period, then, all other things being equal, the resultant "acquired" phthisis in the one would be developed sooner, would cause more serious lesions, and would run its course more rapidly, than would the resultant "hereditary" phthisis in the other. This conclusion, which is opposed to the views generally entertained, is to be deduced from a number of facts relating to "acquired" phthisis, but it is not advisable to stop to discuss the question here, or to produce the evidence for it, as it may be taken to verge too closely on the realm of pure speculation, although, it may be observed, it has a very practical side to it.

It might happen, and I suspect that something very like it has happened, that a girl of seventeen or eighteen, of a "non-consumptive" family, recovering, but still weak, from non-infective disease, goes to stay for change of air with a "consumptive family," one of the girls in which, of her own age, is also an invalid, with premonitory symptoms of phthisis, and, as they are old companions, they share the same room. The physicians may have been consulted as to

this arrangement, and as they know there is no consumption in the family of the convalescent they see no objection. Assuming that the two girls are able to take but little exercise, and are as nearly as possible in the same degree of debility, then, if it should happen to be the time of seasonal activity with the bacillus-vegetation in their joint room, two or three weeks may perhaps suffice slightly to infect the visitor, and, unless she be removed, a month's further exposure may end in large deposits of tubercle in one or both lungs, which may caseate rapidly. The final result will of course depend on many conditions. The other girl, the hereditary consumptive, who had been struggling with the vegetation off and on long before her companion arrived, and may even have small depositions of quiescent tubercle in her lung, will also suffer from further lodgments of the parasites; but the probabilities are, as I infer, that the lesions in her case will not be so extensive, or so soon caused, as in the case of the convalescent visitor. The questions, however, underlying this illustrative but supposititious case must be left.

An inference of great etiological value is to be reached from "acquired" phthisis when it supervenes upon a lingering illness during which the patient has been kept to one room. When this has happened it is a certainty that the efficient infection has been brought about in the room. We are thus enabled to bring all the questions relating to the source of the bacilli in the lungs of such a patient to a focus. We have simply to get a sufficient supply of the bacillus-spores, or of the bacillus-vegetation, to account for the presence of the bacillus-forms in the lungs, within the four walls of the room. We are not hampered in this case with "tendency," or "disposition," or "history of phthisis"—unless an atavismal history be supposed—and all side issues are struck out. Perhaps these instances of phthisis occur-

ring in the sick-room, being clinical cases, may be too "uncertain and inconclusive" to be taken into consideration by the followers of Koch: otherwise it would have been interesting to have learnt how it is conceived that the requisite quantity of sputum-dust is conveyed into the room to infect the inmate and cause phthisis; for this would appear to be almost an impossibility. It is palpable that the normal or abnormal number of bacillus-spores ordinarily floating in the atmosphere of the densest cities will not account for an attack of this kind, or every hospital in the capitals of Europe would be swept by phthisis, and recovery from severe accident or serious illness would be unheard of in the great towns. Therefore the atmosphere of the isolated sick-room now in question must be assumed to hold a number of bacillus-spores far exceeding the number held in the most septic atmosphere, and that for some two or three weeks at the very least. And as these spores do not, naturally, increase outside the living body, it is incomprehensible that such numbers of them should find their way into the sick-rooms of private houses. The attendance of an uncleanly consumptive nurse might explain the presence of a certain amount of sputum-dust; so might the wilful introduction of sputum; but nothing short of some such out-of-the-way mode of importing sputum into the room, *in every case,* would answer the purpose of the etiologist who holds the view that phthisis is caused by bacillus-spores derived solely from animal organisms.

The subject might be left there, for it is palpable that the contagionist cannot by any possibility evolve a sufficiency of the contagium; but there are two counts against his view—*one, that he cannot get the spores into the room; the other, that it would not advance his theory if he could,* for he would still have to show that the occurrence of the disease was consequent on, and not concomitant with, the introduction of the spores.

The etiological questions involved here have not been worked out anywhere, for it appears to have been taken for granted that, once sputum-dust can be shown, or can be assumed, to have been associated, however remotely, or in however small quantity, with the occurrence of phthisis, the dust causes the phthisis, and the chain, or link rather, is complete. The conclusion is forced and there is an end of the matter. A large and important section of the English school, and here and there an independent inquirer on the Continent, realizing the serious consequences hanging to this conclusion and finding that it clashes with many well-observed phenomena, have strenuously opposed it, but without much effect, and it would seem as if the anti-contagionists were losing ground so far as numbers are concerned. They are in a dilemma, for, having accepted the discovery of the bacillus and all that it entails, they have had little else than facts—and principally clinical facts —to pit against the dicta which Koch and the contagionists have pronounced to be its logical consequences. The anti-contagionists, indeed, have not succeeded in putting forth a complete and well-digested theory based on clinical facts which has invalidated Koch's views as to sputum-dust. Dr. Andrew's Lumleian Lectures, it is true, are one sustained and powerful argument for the malarial or (to coin a convenient term) bacillarial source of phthisis, but they do not cover the whole ground, and are deficient in that the cardinal point of pure parasitism is altogether overlooked or overridden. Consequently neither Koch nor his followers have felt called upon, apparently, for any great exertion to defend the position they have taken up. They are, *ad interim*, masters of the situation. They have possession of a theory which more or less dominates four-fifths, perhaps, of the whole medical world, and they retain possession by what lawyers would call a good holding title, which carries with it, unhappily for

humanity, the usufruct of the theory. Their averment is, in effect—"There's the bacillus in the lung, and there's the bacillus-spore in sputum-dust. We infer that the spores in sputum-dust are carried into the lung; and as you do not show us any other feasible mode, and as we see no other possible mode, by which they enter the lung, the inference is a certainty." And however firmly convinced the anti-contagionist may feel that there must be something wrong somewhere—"a something as yet unexplained"—in this line of argument, and however clearly he may perceive the insufficiency of the theory of sputum-dust, yet, in the presence of the bacillus, he has found himself paralyzed and incapable of conceiving any rational mode by which it may be assumed to invade the lung.

This etiological deadlock has come, as I have suggested, of what we must consider to have been the unfortunate inference of Koch in regard to the botanical position of the bacillus, and of what was, seemingly, the universal homage of the scientific world to a great name, when it bent the knee to an inference of this magnitude. I have striven to show how this deadlock may be overcome by converting the pure parasite into an accidental parasite. Whether this proposed inversion of Koch's inference will be seriously considered for a generation or two must be left; but that the question will ultimately have to undergo a searching investigation upon its merits, I have no manner of doubt. And whether my proposition will be found to be sound or not, must also be left; but, in the meanwhile, it will not be overlooked that the assumption here is that it is sound.

By this assumption the anti-contagionist is furnished with a theory founded on the same basic fact as the sputum-dust theory, and he will see at once that by the aid of the view of accidental parasitism he has a full explanation of the occurrence of phthisis in the sick-room without the

intervention of sputum-dust—an explanation which shows how an artificial malaria is created there, and how, therefore, the necessity for assuming sputum-dust is superseded. The bacillus can be got into the lung with certainty, facility, and efficiency by this view, and there is no occasion to suppose any unlikely or unheard-of collocations of conditions in order to explain its presence there. The theoretical phenomenon falls into its place amongst ordinary phenomena without effort. It wants no bolstering up by the supposition of wild and monstrous and bestial conditions, such as sputum-dust left about through inordinate filth, or with injurious or constructively murderous intent, but it fits in precisely with the ordinary environments and social relations of all classes in everyday life.

With a theory by which he can readily account for these and all other cases of phthisis—a theory which coheres in all its parts, and which will be found to be consistent with all known facts—the anti-contagionist can not only defend his own position, but may turn upon the advocates of the theory of sputum-dust. With this ally he is entitled to demand that the contagionist school shall furnish a connected and complete scheme of the distribution of sputum-dust. Hitherto, as has been pointed out, it has contented itself with enunciating its doctrine, with stating some facts and reasons that appear to tell in its favour, with ignoring those which cannot be reconciled with it, and, finally, with leaving it to others to disprove. Sooner or later, however, the school will have to accept the *onus probandi* and establish the view of sputum-dust by every kind of evidence, or abandon it.

One of the most remarkable things in connection with this dust is the complacency with which Continental etiologists marched straight on to the explanation, by its means, of every case of phthisis, without once halting to see whether

any case of phthisis had been explained by it. They did not stay to ask whether the bacillus-spore contained in sputum was an efficient agent in the spread of phthisis, but, observing that Koch had found the dried spores in sputum still virulent and efficient in causing tuberculosis when introduced into animals traumatically, they jumped, with him, to the conclusion that the same spores disseminated by the air were capable of establishing themselves in the alveoli of the lungs parasitically, and of causing phthisis, just in the same manner as the original vegetation from which they derived had caused phthisis. The conclusion, it may be admitted, was not an unnatural one. It looks, indeed, an absolute certainty. Still, I venture to question whether the spores in sputum possess the power of causing ordinary phthisis in man, and I shall produce such evidence as I think will show that the point has not been taken lightly. We will first examine it by the aid of phthisis of the purely "acquired" type occurring during debility in these sick-rooms.

Waiving the impossibility of getting sputum-dust into the room of an invalid by ordinary modes in sufficient quantity, and of retaining it there for a sufficient length of time, to cause phthisis, let it be supposed that every day, say, six or eight ounces of moist sputum, containing the largest known proportion of bacilli and their spores, be deposited and left on the carpet or on the floor of a room in which a person is confined to bed, or to a chair, from some recent illness other than phthisis; and let it be supposed that this daily deposit of sputum is continued for one, two, or three months, and that the room is not once cleansed during the time. But, further, let it be a condition that the room receives the full amount of solar radiation for the latitude, etc. (see LIGHT). A stronger or more improbable case could not well be put, but it will serve as

a test case. It is required to know whether the person confined to the room would to a certainty contract phthisis through the dissemination by the atmosphere of the spores contained in the sputum-dust, either by the ordinary, or by an extraordinary, amount of traffic through, or disturbance of, a sick-room.

The contagionist school would not deign, of course, to give such a case a moment's thought, and even the most pronounced anti-contagionist would probably have some hesitation about committing himself to an expression of opinion upon it, and might conceive that it was pushing a view to extremes, or submitting a principle to an unnecessary strain. But I submit, nevertheless, that if the etiologist shall be at the pains to think the matter out by the light of all the facts that bear upon it, he will have to conclude that it is not a certainty that phthisis would follow the inhalation of bacillus-spores under such conditions as have been laid down, and that, as the greater includes the less, it is not a certainty that phthisis has ever followed any inhalation of a smaller number of bacillus-spores under conditions like in kind and principle, but not in degree or intensity. In other words, if a maximum amount of exposure will not, of necessity, cause phthisis, a less or a minimum amount has not, of necessity, caused it. The etiologist would see, of course, that such an exposure to an atmosphere of sputum-dust as has been conceived might, in the case of denudation of a surface, lead to tuberculosis and to a secondary invasion of the lung; but this is not challenged, and is outside the present question.

The principal theoretical grounds upon which Koch would appear to have founded the doctrine that sputum-dust is the exciting cause of phthisis are: that the bacillus is a pure parasite; that guinea-pigs are singularly free from spontaneous tuberculosis, but that when they have been

kept for a time in the same place with, but separated from, a number of guinea-pigs which have been inoculated with the bacillus and are in different stages of tuberculosis, he has found tubercle developed spontaneously in their lungs;* that this development of tubercle in the lungs of the non-inoculated guinea-pigs, associated, but not mixed, with the inoculated tuberculous guinea-pigs, is proof that the discharges from these latter have been converted into dust, and that the bacillus-spores contained in this dust have been carried from their hutches by the air to the hutches of the sound guinea-pigs, have been inhaled by them and have been parasitically lodged in their lungs; that phthisis is widespread, and runs in families; that it has been shown that the sputum of consumptives contain bacilli and their spores in greater or less numbers according to the stage of caseation, etc.; that in the aggregate a vast number of the spores contained in the sputum of the infected are retained for an indefinite time without losing vitality or virulence (as is shown by inoculation) in or about habitations in the shape known as sputum-dust; that sputum-dust is disseminated in the air in some way, and, by analogy with the guinea-pigs, persons breathing the air take the bacillus-spores into their lungs and thereby acquire phthisis; and that although there may be some clinical and other phenomena as yet unexplained, yet, bearing in mind that it has been demonstrated that the bacillus is the specific cause of phthisis, that it cannot grow out of the animal organism at ordinary temperatures, and that it follows, therefore, that it is a pure parasite; and, further, bearing in mind that there is no other conceivable method by which the bacillus can rationally be supposed to enter the lungs,

* This occurrence of spontaneous phthisis, by the way, is an instructive illustration of "acquired" phthisis; for, from the fact that guinea-pigs were never once found by Koch to have tuberculosis naturally, both heredity and hereditary predisposition are out of the question as regards their infection.

it is an inferential certainty that phthisis is caused by sputum-dust.

Into a vast question such as this many minor facts and arguments have found their way, but there is no room here for them all; I have given what I take to be the pith and marrow of Koch's case. Having dealt at some length with the botanical nature of the bacillus, let us pass on to the phenomenon of the spontaneous infection of the guinea-pigs—a species of comparative clinical fact which, for some not very obvious reason, would seem to be considered more trustworthy and conclusive than ordinary clinical facts. Granting that the non-inoculated animals were free from tubercle when received, and that all errors in the results were precluded, what is the evidence that these animals were efficiently infected by spores delivered by the air, unchanged, except by drying, from the organisms of the tuberculous animals? It is simply that no other presumable mode suggested itself, and that, inferentially, it was the mode. And this inference is, no doubt, the natural sequence of the great primary inference as to the botanical nature of the parasite. But etiologists who shall reach the opposite inference to Koch's with regard to the parasite will find that there are two other and more likely modes by which the guinea-pigs may be presumed to have been infected. First, through the existence of a local malaria, or bacillaria, caused in the ordinary manner in the place where the guinea-pigs were kept. Secondly, through the creation of an extraordinary local bacillaria by the germination of the bacillus-spores in the excreta of the guinea-pigs, and by the reversion of the spores to their aerobiotic parent-forms.

Before looking at these two modes of infection, however, the details connected with the occurrence of spontaneous tuberculosis may be here given. In his address, Koch says:

"Among hundreds of guinea-pigs occasionally just purchased, which were killed in other investigations, not a single one have I found tuberculous. The spontaneous tuberculosis was manifested in single isolated cases, and never before three or four weeks after the animal began to live under the same conditions as those which had been inoculated with the disease. In the cases of spontaneous tuberculosis, the bronchial glands were found invariably very much swollen and broken down, and generally also large caseous deposits in the lungs, broken down in the centre, so that, as in human beings, cavities had been formed. The development of tubercles in the abdominal cavity was far less frequent than in the lungs. The enlargement of the bronchial glands, and the evident commencement of the process in the respiratory organs, leaves no room for doubt that spontaneous tuberculosis in these animals is caused by the inhalation of a few or possibly only one single infectious germ, and which, on that account, has taken a very slow course." With regard to the same subject, Koch further says: "Among many hundreds of rabbits and guinea-pigs, which were purchased and kept for experimental purposes, and finally subjected to post-mortem examinations, not one was tuberculous. Only after the infection-experiments were begun—and large numbers of tuberculous animals were kept in separate cages, but in the same room with animals kept for other experiments—did cases of spontaneous tuberculosis occur amongst the latter. Still, distinct symptoms of tuberculosis almost never presented themselves in such animals until they had passed at least three to four months in a room along with those which were tuberculous. It was a very characteristic phenomenon, also, that when the number of animals artificially infected with tuberculosis diminished, so in proportion did the cases of spontaneous tuberculosis become fewer, and *vice versâ*.

For a long time, when very few tuberculous animals were kept, spontaneous tuberculosis entirely ceased among the other guinea-pigs and rabbits, which were kept in large numbers" (Mittheilungen, vol. ii. p. 42).

With reference to the first of the presumed modes of infection of these guinea-pigs—by an ordinary bacillaria—little need be added to what has gone before as to the formation of the miasm. It is easily conceivable that in the room in which all these animals were confined for purposes of experiment, may have been brought together the conditions I have assumed to be required for the establishment of a local miasm. One of the principal conditions—solar radiation—not having been much regarded hitherto in the construction of human habitations, is not likely to have been taken into the account in providing a place for safely keeping guinea-pigs in. Therefore sufficient shade and sufficient soil for the growth of the vegetation might be assumed in this instance. And I strongly suspect that this unthought-of source of infection has vitiated many of the experiments of the micro-pathologist. The value of the control-animal is materially reduced by this view, and many of the records of phenomena observed in connection with spontaneous and traumatic tuberculosis (as well as with other infective diseases) would admit by it of other, and perhaps sounder, conclusions than those arrived at.

Perhaps, however, in this case the uninoculated guinea-pigs may not have been infected by an ordinary local bacillaria. The fact that there was a diminution of spontaneous tuberculosis in a ratio with the reduction in the number of traumatically infected guinea-pigs, and an entire cessation of it when very few tuberculous animals were kept, points to the freedom of the place from ordinary local bacillaria, and to the second mode of creating a malaria—*by the germination and growth of the bacillus-spores on*

the dung of the tuberculous guinea-pigs. But before going to the consideration of this subject, it may be as well to refer to a question arising out of the perfect freedom of the rabbits and guinea-pigs from tuberculosis when first bought. "Among many hundreds," we learn, not one of these animals was tuberculous. Now, seeing that they "acquire" phthisis when in the same room with the inoculated animals, it shows that they are susceptible through the air-passages to the bacillus-vegetation; and as it has been demonstrated that the bacillus of tubercle found in animals is identical with the bacillus found in man, and as rabbits and guinea-pigs are kept, more than most animals, in close proximity to man, how is it to be explained that they are not phthisically infected with human sputum-dust—the more especially since it is taught that there is "no room for doubt that spontaneous tuberculosis in these animals is caused by the inhalation of a few, or possibly only one single infectious germ?" Passing over guinea-pigs, the freedom of the domesticated rabbit in Germany from phthisis is very curious and significant as to the infective power of sputum-dust, for the wild rabbit of Australia has "acquired" phthisis on a large scale. The "rabbit pest" in that country, as is well known, has become very serious to owners of cattle and sheep by reason of the large amount of grass consumed. Among the proposals for the destruction of rabbits was the project of inoculating some of them with the bacillus of tubercle and turning them out, with the design of communicating tuberculosis to those in the burrows. The plan was under fierce discussion, when it was ascertained that it had been forestalled; for some rabbits that were shot were found to be tuberculous — to be phthisical, in fact—and further examination brought out that in some districts a large proportion of the rabbits had tubercular deposits in the lungs. This opened up the

question, of course, of their fitness for food; but the point here is how these rabbits became infected, and how the German rabbits escape infection when domesticated. Not to linger over the subject, it is to be inferred that the wild animals are exposed to a natural bacillaria—a bacillaria holding the particles of the bacillus-vegetation growing in the flora of the country; that the tame rabbits supplied to Koch had been protected from this form of malaria; and that, though susceptible enough to the vegetation received into the lung through the respiratory tract, they had not been infected by bacillus-spores discharged in the sputum of man.

The view that the bacillus-vegetation occurs on excreta breaks entirely new ground. The presence of bacilli and their spores in the alvine discharges of the phthisical patient can only occur when phthisis ends in general tuberculosis—unless, perhaps, in the case of lunatics, who sometimes swallow their sputum. And the probabilities that phthisis is ever caused through the spores in the excreta of patients are so infinitesimally small that the subject has not been dealt with, and need not detain us now. Yet the questions mooted in the view of the germination of the bacillus-spores in the excreta of the guinea-pig are on a different footing. It comes within practical etiology that some domesticated and caged animals may be infected by means of bacillus-vegetation discharged in the excreta of tuberculous animals. Although little is actually known of the matter, it is almost an inferential certainty that when the bacilli enter the blood of an animal, either traumatically or auto-genetically, the excreta of the animal will contain the bacilli and their spores. This occurs in most infective diseases with the pathophytes; and by analogy, and also by inference from certain facts connected with the tuberculosis of cattle, we may safely assume that living

bacilli or their spores find their way into the excreta of guinea-pigs affected with general tuberculosis; and, further, it may be presumed that the dung of the animals may furnish a congenial soil for the bacillus-vegetation. Then, given the requisite amount of moisture in the air (which will have been supplied by the breath of the guinea-pigs) and the adequate shade, and we have all the conditions for the germination of the spores and the growth of the vegetation, except the one that Koch found to be essential in his cultivations—a temperature at blood-heat. But, as Koch did not attempt to cultivate bacilli on excreta in dull diffused light, it does not follow that this temperature is necessary to the growth of the bacilli on this special soil, under the conditions suggested. It may be understood that Koch did not cultivate bacilli on the excreta of guinea-pigs or other animals for the reason that he required a pure cultivation for his purposes, but it is by no means improbable that if he had sown them on this material, without altering its composition by heat for sterilizing purposes, under proper conditions, he would have found that they would have grown readily enough at ordinary temperatures, though they might have been mixed with other forms. And possibly, indeed, the admixture of other forms, as has been suggested, may be a condition of their growth. Nature may require what to the micro-pathologist is an impure cultivation for her purposes. She has secrets that will never be learnt in a laboratory, and perhaps one of them is how to grow the bacillus-vegetation at ordinary temperatures.

Be this as it may, I conceive that when the final determination of the question can be arrived at, it will be found, or may be inferred, that the spontaneous infection of these guinea-pigs occurred from one of these two, or perhaps from both, kinds of malaria—the forms on the

local soil overrunning the adventitious soil, or the reverse—and not from bacillus-spores derived and conveyed from the tuberculous animals in the manner assumed by Koch. Indeed, from a large group of facts it is a strong presumption that the bacillus-spores, either moist from the organism or in the dried state, are rarely, if ever, concerned in the dissemination of phthisis through the respiratory tract. It is highly improbable that the *bacillus, as a bacillus,* or *bacillus-spores as bacillus-spores*—that is, without undergoing morphosis on some matrix after leaving the host—are concerned in causing ordinary spontaneous phthisis by their presence in the air in any numbers, the experiments of Tappeiner, Giboux, Weichselbaum, and others, to the contrary notwithstanding. Their observations with dogs and rabbits, which may seem to tell against this view, not only lack precision, but prove too much, or are contradictory. An experiment in this field that does not provide a co-efficient of some sort for local bacillaria is worth little; and the formation of tubercles in the lungs of dogs after one inhalation of atomized sputum shows too great an alacrity in the bacilli. The infection of two rabbits in one hutch by the expired air of consumptives, and the non-infection of two other rabbits in another hutch in another room provided with similar air filtered through tow impregnated with carbolic acid, are interesting facts; but, as we know nothing of the local conditions of the rooms, and as the experiments are open to numerous other objections, they determine nothing. Perhaps the room in which the rabbits became infected was dark and the other was well lighted, which would account for the results without the exhaled breath of the consumptives. Besides, as has been well observed by others, even if we had absolute proof of the artificial infection of some of the lower animals by inhalations either of moist atomized sputum, or of sputum-

dust, or of the expired air of the phthisical, it would not carry with it the certainty that all other animals and man would be artificially infected by the same or similar inhalations. Comparative pathology shows that the bacillus in general tuberculosis behaves differently in different hosts, and affects, or colonizes on, a tissue in one animal which it cannot or does not colonize on in another animal. But I do not rest the conception of the inability of the bacillus-spore to infect man on the shortcomings of these experiments.

There is another well-known experiment—that with the rabbits in the extraction flues of the Brompton Hospital. The tubercle in these rabbits was fairly well shown to have been a consequence of their submission to the atmosphere of the flues. This much may be granted; but it does not follow that the rabbits were infected by the bacillus-forms delivered into the flues from the wards, as has been inferred. It is so far from certain that the sum and value of the bacillus-forms contained in every cubic foot of air which enters the flues are the same in every cubic foot of air which is discharged from the flues into the open air, that whilst I should not have the slightest objection, on grounds of personal safety, to breathe the atmosphere of any one of the wards for consumptives for a year, or any length of time, I should very strongly object to have to breathe that atmosphere for two or three months *plus* the sum and value of the bacillus-vegetation acquired by it in passing through the flues. If the Brompton flues were clear glass tubes or cylinders exposed freely to chemical light, this objection would not hold good; but assuming them to be air-conduits with dark interiors, I conceive that their inner surfaces will give lodgment to abundance of the most suitable soil for the growth of the bacillus-vegetation—especially in places out of the full current of the extracted

air—and that the rabbits in the flues were infected by the forms given off from this free-growing vegetation, and not by the effete bacillus-spores sent forth by the phthisical patients in the wards.

Dr. Sturges, in a lecture on Tuberculosis,* says of the conditions of life among the London poor: "What we want to ascertain is the relative action of these several factors. Considering that many of the circumstances of poverty—such, for instance, as concern food, clothing, and exposure—are much the same both in town and country, it is at least plausible to maintain that one chief agent of tuberculosis, and part cause of the intolerable fatality of the disease in London, is overcrowding and dwelling underground away from the light and sun—such conditions of living, in a word, as are known to cause tuberculosis in rabbits and guinea-pigs!" Dr. Sturges hits the right nail on the head; but it wants clinching.

As a set-off to the Brompton experiment may be cited those of Tappeiner, made after the discovery of the bacillus. In a letter (to the *Weiner Med. Presse*, January 14, 1883), he gives details of these on a number of rabbits with finely divided pulverized sputum, and in no instance did one of the animals compelled to inhale this dust in quantity on several successive days, show any sign of tuberculosis, even after one or two months. Tappeiner observes that these negative results should calm the public mind, so much excited by Koch's bacillus. As these experiments have, I believe, been confirmed, they tend to show that whatever effect moist sputum, or the expired breath of consumptives, may have when inspired by rabbits, sputum-dust does not induce phthisis when taken into their lungs. Assuming, therefore, that Tappeiner's experiments were conducted with due precaution, it is an inference that the Brompton

* *Medical Times and Gazette*, July 18, 1883.

rabbits might just as easily have been infected in the flues if the air from the wards had been cut off, or in similar flues which extracted air from apartments in which phthisis had not been known. Yet the sputum-dust used by Tappeiner might have induced acute tuberculosis in the rabbits if inoculated into their fluids. His experiments afford an illustration of the diminution which is assumed to take place in the infective power of the bacillus-vegetation when it assumes the form of the bacillus—to be referred to more at length presently. They point, moreover, to the inference that the guinea-pigs and rabbits that contracted spontaneous tuberculosis in Koch's room were not infected by bacillus-spore dust from the inoculated animals, and that the bacillus-spores in sputum-dust are also inert when inhaled by man.

I say inert, not because the bacillus-spores are supposed to be sterilized, or incapable of germinating and growing under some conditions. Their vegetative germens are intact, as is shown by cultivation processes and by the results of inoculation. It is the higher reproductive functions of the vegetation that, it is supposed, are impaired in these representative forms by the enforced parasitism of the parent forms. I conceive the bacillus-spores to be inert, therefore, in the sense that they are practically innocuous to man under ordinary circumstances, and so far as phthisis is concerned. Possibly a compulsory inhalation of an atmosphere highly charged with these bacillus-forms, kept up continuously night and day under certain conditions by artificial means, might, even if due precautions were taken to exclude a local malaria, end in causing pulmonary tuberculosis in some of the lower animals. It is conceivable that the continuous strain on the air-passages and smaller bronchial ramifications caused by such an atmosphere might induce such congestion and engorge-

ment of the vessels as to lead to rupture, and the breach might and would facilitate parasitism. I am not prepared to deny or to question the possibilities of such results from such unnatural conditions; but I say that if local pulmonary tuberculosis is established by such means, it is not of the nature of ordinary, spontaneous, or chronic phthisis, and is not to be confounded with it. It is to all intents and purposes an inoculation. Even amongst the lower animals I conceive that nothing short of such artificial conditions would induce such a form of local tuberculosis, and it is by no means certain that even such conditions would implicate a perfectly sound man, though it would be useless to discuss the point, seeing that man is not likely to be submitted to the conditions. The point now is that, as such conditions do not meet, except in the laboratory, they do not affect the proposition that the bacilli and their spores as given off from tuberculous organisms are, in effect, harmless to living animals—*qua* phthisis—and that in this respect they are virtually, but not botanically, or when used traumatically, inert or effete. Their inability to infect through the respiratory tract (always provided there is no solution of continuity of surfaces), not their disability in regard of the formation of tubercle, it is supposed, renders them practically innocuous to man, and even to animals. For when tuberculous animals intermix with sound animals, or when they are closely confined together, as in the case of Koch's guinea-pigs, whatever bacillus-forms may be given off from the infected and may be inhaled, before undergoing further change than drying, by the uninfected animals, are, I consider, inoperative, and are not concerned in after phthisical processes among the animals.

As this view is a clear departure from all recognized views of the bacillus, and may not find favour either

with the anti-contagionist or the contagionist, and as the questions depending on its soundness may have grave consequences, it is incumbent on me to show cause for venturing to suggest organic change in first principles. I am perfectly conscious that the enunciation of such doctrine will at first appear too wildly speculative for serious consideration; but, as the inferences on which I depend have been reached from facts which will be submitted, and as the processes by which I have arrived at the result admit of being readily checked, and as, moreover, the phenomena to be passed in review in the attempt to sustain the position taken up will go towards the completion of the case I bring against the pure parasitism of the bacillus, which case, it may be observed, is by no way dependent on or endangered by this collateral issue, I have not been deterred from stating my conclusion.

One inference from a thoughtful study of the bacillus in its relations to phthisical man, whether we regard it as a pure or as an accidental parasite, is that the parasitic power of each and every one of its aërial forms, in whatever shape or from whatever place it enters the air, is, taken singly, of the feeblest description of parasitism, although collectively the forms are so destructive to the human race. Enormous numbers of these forms must be suspended in the atmosphere and be inhaled by man for a lengthened period in order to cause an efficient attack of ordinary phthisis. It is patent that in the aggregate a vast multitude of them are inspired before any of them can find what is called a "suitable soil," or can effect lodgment and become parasitic. Whatever theory may be constructed as to the mode by which the bacillus-vegetation enters the air so as to be carried into the lungs and cause phthisis, it must provide for continuous or frequent supplies of efficient forms of the vegetation in the air breathed; for these supplies being

kept up for months or years, with certain intermissions or variations in the number or nature of the forms in a given volume of air; and for these supplies being often limited, or nearly limited, to the atmosphere surrounding or breathed by one particular person, or limited so that other persons in the same house in close proximity—even in an adjoining room—shall not be subjected to the inhalation of a noxious, or of an efficient, quantity of the forms.

If we turn to the only recognized theory on this subject —the theory which attributes to sputum-dust the efficient distribution of bacillus-spores in the air—we shall find that it does not meet any one of these demands upon it. It is an inference, therefore, or one inference, that *sputum-dust is an impossible agent in the dissemination of ordinary phthisis.*

Koch supposed that a very limited number of bacillus-spores will cause spontaneous infection in the lower animals —"possibly only one single infectious germ." This supposition, having in view the rarity with which the vegetation fastens on to open wounds or denuded surfaces, is hardly to be reconciled with the fact that the spontaneous tuberculosis in his uninoculated animals was so evidently governed by the number of the traumatically infected animals in their vicinity. As the bacillus-spores contained in the discharges of a single infected animal must have been numerically large, the phenomenon of rising and falling infection would seem to point rather to the conclusion that one spore or a few spores will not suffice, but that great numbers of spores are required, for efficient infection. However, whatever potency Koch may assign to the bacillus-spore in the case of guinea-pigs, he abandons the view as regards the infective value of a single spore in the case of man, inasmuch as he recognizes "hereditary disposition," with all that it entails. He could not well get over the fact, even though

bordering on clinical observation, that long exposure to an atmosphere holding the bacillus-vegetation largely in excess of the amount ordinarily floating in the air is necessary to establish spontaneous human phthisis, and that even long-continued exposure to such an atmosphere may fail to induce infection. It is, indeed, the unaccountable resistance of people to infection who ought, theoretically, to be infected that called into existence the "hereditary disposition," and has preserved it from dissolution. However, the very necessity for the assumption implies that the bacillus-vegetation has a low degree of infective power, and that neither "one single infectious germ" nor a few germs will induce phthisis.

In most cases of chronic phthisis, and in almost all cases occurring among the wealthy, it is almost an inferential certainty that the place in which the bacillus-vegetation is efficiently transferred from the outer world to the lung of the patient is the bedroom. It is to be inferred, further, that the principal, or the efficient, fund or source of the vegetation in the air of the bedroom is localized somewhere within the room; that the period during which the greatest amount of the vegetation passes through the air-passages is the ordinary time for sleep; that that period is the most favourable for the efficient diffusion of the forms in the air; that as, during the course of the disease, the patient undergoes variations in the severity of the symptoms (variations having in some instances a periodicity which is dependent apparently on seasonal changes, inasmuch as most consumptives within a certain radius have concurrent similar aggravations and remissions of cough, pyrexia, etc.), these periods of quiescence and recrudescence correspond to periods during which there is a diminution or an increase in the numbers or value, or in both numbers and value, of the forms floating in the air of the chamber; and that these

periods are not associated with any arrangements or changes connected with the bedroom by which the diminution or increase of the vegetation is to be explained.

If we now assume that the bedroom of the patient either had not been previously occupied by a phthisical patient, or had not been so occupied for many years; that the ventilation, though not perhaps answering to modern requirements, was yet equal to that of most (uncontaminated) bedrooms in this country; that the door or doors and window or windows were kept open at the usual times; that the room was swept and properly attended to; that a fire was kept up in cold, wet, or damp weather; and that, in short, the room was a well-ordered cleanly bedchamber of one of the higher social classes;—the first question that will naturally occur to the etiologist is the one as to the original introduction into this room of an adequate supply of sputum-dust. But, shelving the question as to the source and mode of introduction of this imaginary fund of bacillus-spores, we will assume this difficulty removed, and suppose that by some accident a considerable quantity of the sputum from a phthisical patient has been left in the chamber in a position where it has escaped observation and from which it has not been dislodged by ordinary household processes.* It is obviously useless to suppose it to be left on the surface, or where it would be swept away bodily, or be dissipated rapidly by currents of air. Nothing but the retention of sputum within the room for years will meet the requirements of our supposititious case. But we will further assume that this obstacle is surmounted. Then, having got the sputum into the bedroom of the future patient, we

* It is, of course, possible that sputum may get between and beneath flooring boards, or behind wainscots, or skirting-boards, or immovable furniture, etc., in the bedroom of a phthisical patient; but how it can be assumed to have been left in such positions in the bedrooms of all phthisical patients, or in all "contaminated" bedrooms that have not been occupied by the phthisical, is more difficult to see.

come to the question of his efficient infection by means of that sputum in accordance with the doctrine of contagion. We will suppose that the occupant of this room begins to show the usual premonitory signs of the affection in a few months, but that a cavity is not established in the lung for a year; that the other lung becomes soon after involved; and that the duration of the disease is from two to three years, or more or less, according to the frequency and length of salutary or unfortunate changes of air. (For it may and does happen, occasionally, that a patient moves from a bad bedroom to a worse.)

To account for the efficient infection of a person by the spores in the hidden or unseen sputum in the room, the first requirement is to evolve the conditions under which a sufficient number of spores are detached from the mass of sputum in such a way that they shall be wafted on the night air of the bedroom so as to reach the sleeper. As adequate numbers of the spores cannot be supposed to be given off the surfaces of the sputum so long as it coheres, the disintegration of the mucus, pus, shreds of tissue, etc., composing the sputum—its crumbling into dust—is a necessary condition precedent to spore-distribution. These finely divided particles of sputum, therefore, to which the spores adhere, constitute what Professor Tyndall calls "rafts" by which the spores are floated; and it is quite consistent with physical laws that they should be launched into the air of the bedroom in numbers proportionate to the strength of the air-currents, and that they should be carried to the air-passages of one sleeping in the room. We have thus a *modus operandi;* and as we know from experiment and analogy that the spores in sputum retain their vitality for a long period, and as it is, or must be, assumed for fifteen or twenty years, we have also a *modus vivendi* for the fund of bacillus-spores in the bedroom.

But other difficulties now present themselves. If the sputum was deposited long before the room was occupied by the future patient, the chances are, if the room is dry, either that the sputum-dust has been all dispersed by the movements of the air in the room, or that it has been buried under a heavy layer of dust. It will be rapidly dissipated if the currents of air are strong, and if the air is but little stirred it will be weighted down so that the spores cannot enter the atmosphere of the room. If, on the other hand, the room is damp, the sputum will be rapidly overrun and appropriated by various low forms of vegetation, by which the spores—if not appropriated also—will be covered up. The probabilities are, however, that the spores, being kept in a moist condition and debarred from germination, would soon be converted into soil by some forms or other. A damp room must soon prove fatal to bacillus-spores, or render them physically incapable of contaminating the atmosphere ; whilst the air of a dry room will either dissipate the fund of dust by whirling the particles or "rafts" into circulation in the room and by turning them into the outside air, or it will permit the settlement of solids which will overlay and overweight the "rafts."

Passing over certain physical considerations relating to the larger amount of spores that would be disseminated in the room in the stir of daytime than during the still night, as well as some other points that might be taken, it will be seen that the period of the supposed efficiency of sputum-dust as a theoretical air-poison is much more restricted than its advocates take it to be. It would seem, indeed, impossible that they could have tackled the problem of sputum-dust distribution with a view to see if the theory is a practicable theory. They appear to have regarded neither the mechanical, nor the botanical, nor the clinical, nor any other difficulties that stood in its way. And

even if the objections already thrown out are deemed to have but little or no force, there is one great insuperable difficulty that prohibits the reception of the theory of sputum-dust as the agent of infection in phthisis—a difficulty that, if there were no others, would effectually shut out the theory.

The recurrence of severe phthisical symptoms amongst a number of patients at particular times has been so marked occasionally that writers have described these outbreaks as "phthisis epidemics." Generally some time during spring and autumn, though occasionally it happens as well in warm, moist, or "muggy" weather in summer, and more rarely in winter, all the consumptives of a country side, many of whose cavities are perhaps in a state of quiescence, and whose general health is so improved that they have higher hopes of speedy recovery than usual, are suddenly and simultaneously thrown back, and some into galloping consumption, whilst the incipient consumptives, those with doubtful signs of "decline," or "falling away," or "overgrowing," are at the same time attacked with cough, spitting of blood, and other pronounced indications of tubercle-formation. *These phenomena preclude the supposition of the agency of a spore which does not reproduce itself outside the body.* For it is impossible to suppose that the interiors of a number of rooms in different houses should be so disturbed that largely increased numbers of these spores are accidentally distributed within them precisely at the same period. It is, indeed, almost an inferential certainty that these outbursts of parasitism of the bacilli are the outcome of invasions of crops of the specific vegetation matured at the usual seasonal periods, or forced into renewed activity by unusual and highly favourable conditions; but whatever the interpretation of these phenomena, it is palpable that the theory of sputum-dust utterly fails to explain them.

The theory provides no working machinery for the production of such manifestations of the disease. The phenomenon of periodical "epidemic" recrudescence in phthisis was evidently not contemplated by the framer of the theory, or he would have seen that it is fatal to it. But the theory lay invitingly on the surface, and he took it. The other objections pointed out should suffice to upset it, but this radical defect destroys it. As it is absurd to suppose that "seasonal" conditions can affect this pure parasite, either in or out of the body, it may be inferred that *the bacillus-spores in sputum-dust are not concerned in the periodical exacerbations* in phthisis; and, to paraphrase the legal maxim—*exceptio unius, exclusio alterius.* If sputum-dust is not concerned in these severer manifestations, it is not concerned in the milder, or more insidious, or in any manifestations of the disease; and thus we arrive at the conclusion that *whenever bacillus-spores in sputum can be shown to be, or are, present in the atmosphere of the sick-room, they are inert so far as sound persons are concerned,* even if they may be instrumental in causing a reinfection of the already phthisical by attaching themselves to denuded surfaces. As a sequence, *sputum-dust is not the agent in the occurrence of an original attack of ordinary phthisis.*

Much remains to be said, but, for the foregoing reasons, I submit that the doctrine of sputum-dust is at least premature; and that, as a preliminary step to its promulgation, it should have been shown conclusively that some one occurrence of the disease has been due to sputum-dust. This has not been done or attempted, but has been taken for granted, and, as the matter stands, I contend that such a phenomenon as ordinary phthisis caused in man by the direct agency of bacillus-spores from any animal organism, in any way whatever, has not been seen. It has been evolved from insufficient data, and has been fostered by the

contagionist, and I concur with Professor Stricker, of Vienna, that the view "arose in the laboratories."

Going back now to the case in which, for the sake of illustration, I submitted that a sick person should be exposed to the utmost conceivable accumulation of sputum-dust—to the effects of an atmosphere holding more bacillus-spores than were ever concentrated, probably, by any combination of circumstances, in one room—it will, perhaps, be perceived that the conclusion there arrived at is not so hazardous as it may then have seemed. But there are other cogent reasons for the inference that the sick person would not contract phthisis. One of the terms in the case put was that the bedroom of the invalid should receive the full amount of chemical light required by the latitude, etc. The objects of this proviso were, first, to guard against an element of uncertainty; and, secondly, to exhibit the effect I infer the chemistry of the sun to have on this vegetation. As regards the first point, it may be admitted that a darkened room, with the filthy conditions stipulated for, might, and probably would, cause phthisis in the sick occupant, but not because of the bacillus-spores included in the filth. As phthisis is supposed to occur every day in darkened rooms under the cleanliest of conditions, and where sputum-dust cannot be traced, it would still have to be shown whether an occurrence of phthisis in a darkened room full of sputum was a consequence of the presence of the sputum, or a consequence of the existence of a bacillary malaria in the room because of the absence of sufficient light. I assume the latter, but to avoid the discussion of nice questions it was made a condition that the sick-chamber should have abundance of solar radiation.

The precise amount of chemical light required to keep a room of a given size and shape in a given latitude dry and sweet and wholesome, or free from noxious vegetation, has

yet to be ascertained. The subject will be dealt with presently, as well as the few data before me will admit; but though a sufficiency or an insufficiency of solar light for a bedroom, say in the extreme north or the extreme south of England, is somewhat difficult to determine, yet an abundance of sunlight in a bedroom will be understood to mean the admittance of the direct rays of the sun and total daylight into the chamber through large and high windows for some hours daily before and after noon all the year round. This rough definition will serve the present purpose. The air of a room of this description will be so dry as rapidly to cause the desiccation of large quantities of sputum; and, assuming the sputum to be left on the floor, as in the case put, there is no doubt that multitudes of bacillus-spores would be sent on their "rafts" into the atmosphere of the room at every step and by every movement of those present, as well as by currents of air otherwise caused, and that although a proportion of these spores would be carried out of the room, yet enormous numbers would remain to be inhaled by the sick occupant. It is not to be supposed that such an atmosphere could be breathed with impunity; but we need not stop to inquire into all the probable results of breathing it. All that it imports us to consider is the specific effect that would be caused by the bacillus-spores contained in the room; and, assuming that the person died after three months', or survived six or twelve months', occupancy of such a chamber, I conceive that he would neither die from nor contract spontaneous phthisis. The case is analogous to Tappeiner's experiments with rabbits, but I do not depend on these. I conceive that the person would not be infected for two reasons. The first is, that *the effect of solar radiation of high chemical intensity is to destroy all forms whatever of the bacillus-vegetation*, the dried spores in sputum included. The

second is, that I infer that *either moist or dried spores in sputum, uninjured in any way, are incapable of establishing themselves parasitically in the sound lung in the same manner as the parent-forms from which they derived.*

That the action of the sun soon destroys these forms is an inference to be reached from phenomena already and to be presently referred to. No special experiments have been made that I am cognizant of in this direction, for it appears not to have occurred to any one to test the power of the sun in this field of research; but, save for the assurance and certainty given to some minds by actual experiment, it is hardly necessary. Nature, through man's agency, has experimented for us here on a large scale, and there is no difficulty in reading off the results when once we have seized the meaning of her symbols. Every instance of the non-occurrence of phthisis in a so-called "consumptive family" is a brilliant and most conclusive experiment in solar chemistry—to be apprehended and interpreted readily enough with the key. Wherefore we may safely infer that solar radiation of a given intensity is destructive to all forms of bacillus-vegetation. But assuming that the processes of reasoning in this matter are faulty, and that the sun either does not kill bacillus-spores or takes a long time to kill them, we may deduce from many facts that they are inoperative in regard of causing phthisis.

The lessons from clinical facts are not to be ignored by the etiologist, although they are out of favour. One of these lessons is to be learnt from the few instances of even apparent transmission of phthisis from person to person in England. The evidence brought forward by Dr. Pollock in his Croonian Lectures (*Lancet*, May, 1883) is especially valuable. He says: "My colleague, Dr. Reginald Thompson, considers that he has seen fifteen instances of wives becoming infected through nursing consumptive husbands

out of a total of fifteen thousand consumptives. He evidently inclines to the opinion that the symptoms of phthisis in these cases are rather due to septic than specific tubercular germs, the latter phenomena being pyæmic in most instances. In these communicated cases the symptoms were acute, and the morbid appearances almost pyæmic. My own experience, which has not been inconsiderable, and has extended over thirty years of hospital and private practice, does not supply other than occasional instances of the apparent communicability of phthisis, either in the case of husband and wife, or of attendant on the sick. In families whose members successively fell victims to the disease, the attack seemed rather due to the peculiar age at which persons closely related by blood begin to exhibit its symptoms. On the other hand, I have seen many instances in which the most assiduous personal nursing of the sick, living in the same room, sleeping in the same bed, and undergoing the same influences of air and lodging, of anxiety and harass, as the sick, have failed to produce it. There have been waste of flesh and strength, loss of sleep and appetite, and all the evidences of depressed vital powers, in numerous cases, but no phthisis. The apprehension of the disease has added to the risk, but the tried and trusty attendant has outlived the trial, and survived (often unwillingly survived) the object on which these attentions have been unselfishly lavished. This, too, has occurred again and again where an inherited taint has rendered the disease most probable to invade. But I confess that further and searching investigations are needed on this question—investigations which shall extend over this and other countries, which shall not have been dictated by already-formed theories, and which shall stand the closest and most impartial scrutiny. . . . In Southern Europe, the opinion of the contagiousness of phthisis has long been held, and in Italy especially, where I have often

witnessed the expense to which surviving relatives have been put after a death from phthisis in a lodging or hotel. I fear that the advanced views of Professor Crudeli in Rome will not tend to lessen this apprehension of contagion, nor render the residence of consumptive invalids more agreeable in that city."

Other physicians have met with a similar, others again with a larger, proportion of cases in which phthisis appeared to have been communicated from one person to another. The information obtained on the subject by collective investigation in England has been sharply criticized at home and abroad; but, however unsatisfactory the replies may have been deemed, they have been useful in many ways nevertheless. They enable us, at all events, to see clearly that, whatever views members of the profession may hold upon the contagiousness of phthisis, there are very few of them but could count upon their fingers all the cases they have seen in which they believe persons "caught" the disease from the infected. If we could get an accurate return of the actual number of consumptives treated by every medical man in this country, and of the number of instances in which he considered he had reasonable grounds for concluding that the malady had been actually transferred from a given patient, it would probably be found that not more than one per cent. of the whole could or would be attributed to direct contagion from one known person to another. But whatever the proportion may be, analysis of such cases will soon disclose that they may all be resolved into four classes, viz. (1) cases of secondary phthisis from general or local tuberculosis; (2) cases of primary or ordinary phthisis occurring in a sound wife or husband of a phthisical mate; (3) cases in which relatives or other persons in close attendance on consumptives become infected; (4) cases in which chance visits have been made

to phthisical patients by their friends, or in which phthisical patients have visited the houses of their friends, and the visits have been followed by the infection of the friends.

The first class of cases is outside my present subject, and is limited in number. The second has been touched upon in the remarks on Dr. Weber's case of the sailor with the four phthisical wives, and elsewhere. It need only be added here that most of the recorded cases under this head are *ejusdem generis* with this case. With regard to the third and fourth classes of cases, the same element—the bacillarial environment—is to be found in almost every instance in which a given consumptive is inferred to have been the means of infecting a person; and as this element cannot be eliminated it vitiates the inference. It might be added, as to the fourth class, that as phthisis is not communicable from man to man by "one single infectious germ," as small-pox and the plague are, or are said to be, such cases are probably *post hoc.*

When we reflect upon the few instances in which observers have been able to trace, as they have believed, an occurrence of phthisis to a known patient—marvellously few, seeing how confidently, and even absolutely, it is laid down that every instance of its occurrence is due to a known or an unknown patient—and when we poise this fact and scrutinize it in conjunction with the fact that the disease does not spread amongst the resident physicians, students, nurses, and others in close attendance for years on consumptives in the large hospitals where they are specially treated; with the fact of its non-occurrence in many members of the "consumptive families," or in such cases as those mentioned by Dr. Pollock, or on passenger-ships with consumptive emigrants, or at Alpine health resorts; and with the fact of its non-occurrence in all other cases in which it should (theoretically) occur from sputum-dust;—

when we think all this out carefully, we are driven to the conclusion that its occurrence or non-occurrence is altogether independent of bacillus-spores from the animal body. Then, if we look upon this conclusion by the light thrown upon it from the bacillarial aspect of the question, or from its botanical side, we may, perhaps, get at the explanation of these apparent instances of the communicability of phthisis. Indeed, I strongly suspect that if the medical men who have recorded such instances could divest their minds of long-established views of contagion, and would re-examine and reconsider their cases from the point of view suggested, they might come to the conclusion that the occurrences of the disease they had observed were coincident with, and not consequent upon, contact or association with a patient.

Of high interest and importance in this study of the competence or incompetence of bacillus-spores to cause infection, is the occurrence of spontaneous tuberculosis in the lungs of domesticated and caged animals; but we can only glance at them, even though it is impossible to get an adequate conception of the life-cycle of the bacillus-vegetation, or of the question of the parasitic value of the bacillus-phase of the form, if we limit our view to man alone. The phenomena observed in the incidence of bovine tuberculosis, for example, especially in countries where they are depastured, are most significant upon many points. Among the papers presented to the Parliament of the Colony of Victoria for the year 1885 is the Report of a Board to inquire into the subject of Tuberculosis in Cattle. The voluminous evidence given by graziers, butchers, stockowners, veterinary surgeons, and others, is attached to the excellent Report; and although the Board (including Dr. A. Plummer, Chairman; Professor Allen, Melbourne University; and Dr. J. Jamieson, Lecturer at the University) had not the

question of the pure parasitism of the bacillus present to their minds, yet the facts elicited by them will enable the etiologist who is possessed of this view to evolve the conditions under which cattle are infected in the colony. Excepting dairy cows and some valuable "pedigree" stock, cattle are not housed in Victoria; consequently, whatever the nature of the infective processes, the tuberculous cattle must receive the bacillus-vegetation into their lungs in the open air on the pasture lands. This enables us to eliminate all questions relating to sputum-dust, or its equivalent nasal-discharge dust, in stalls or sheds—questions which, so far, have complicated and obscured the inquiry in Europe. The simplification, indeed, as a little reflection will show, absolutely bars sputum or nasal-discharge dust from being supposed to be the agent of infection of Australian cattle, and entails the necessity of assuming some other mode of infection. For the dispersion of the spores in the open air precludes the inference that bacillus-dust is an efficient means of contagion here. I cannot pursue the subject through all its ramifications, but, having indicated a mine of wealth to the European etiologist, must content myself with giving some of the results I have extracted from it.

The deductions I draw from the Report and the evidence on the tuberculosis of Australian cattle, are—1. That eighty or ninety per cent. of the infected animals suffer from bovine phthisis, and that the disease runs an essentially similar course with ordinary phthisis in man. Cattle recover sometimes even when kept in a paddock where the disease originated, and frequently when removed to higher or drier country. They have their seasonal periods of exacerbation and remission. They waste for two or three years, and die rapidly at last with all the signs of general tuberculosis and galloping consumption. 2. That,

as the bacillus in man and beast are specifically identical, and as the symptomatology and pathology of human and bovine spontaneous phthisis are intrinsically the same, the modes of the infection of man and beast are like in principle. 3. That the cattle infected * are infected by the bacillus-vegetation being inhaled with their breath somewhat after the following manner.

A few of the herd select their camping-ground down in the hollows by the watercourses, in deep gullies or ravines, in reed beds, in thick timber or scrub, or in places that are cool, damp, dark, or shady by daylight; and they may resort, if not disturbed or dispossessed, to the same resting-place, or an adjacent one, at night for weeks or months. Lying down in these positions, they are enveloped in mists at certain seasons, holding the forms of a bacillus-vegetation growing near at hand, and after a due amount of exposure to this natural miasm become infected—just as persons exposed to the night air of fens may be infected with ague. This I take to be the simple, and only sufficient, explanation of spontaneous bovine phthisis in Victoria; and it would have been an obvious and conspicuous explanation but for the paralyzing dictum that the bacillus is limited in its growth to the animal body, and cannot possibly propagate itself in the open air at ordinary temperatures.

The question of the infection of cattle in Europe is a little more complex by reason that they may not only be partly infected by a natural bacillaria occurring in places similar to those in which it occurs in Australia, but may be subjected during the winter months to an artificial bacillaria caused by an extension of the natural bacillus-vegetation of the district to the interior of the buildings

* "This Board is of opinion that not more than two per cent. of the cattle in Victoria can possibly be affected by tuberculosis, and that even this estimate is probably much too high."

or sheds in which they are housed. As in human habitations so in cattle-sheds, it will depend on chemical light whether the bacillus-forms will grow or perish; and as regards "suitable soil" for the vegetation and all other conditions, the sheds of these cattle are in principle on all fours with the country mansions of England. In the multiplicity of questions, however, which surround a large speculative inquiry like this, it would not be possible to make all this stand out clearly without going a long way out of the course. Besides, it is superfluous; for the differentiation in the conditions under which European and Australian cattle become infected, brings out most clearly and conclusively that the phthisis of the latter cannot be ascribed to contagion by bacillus-dust, and that a circumscribed natural bacillaria is the only possible adequate supposition left.

(Whilst on this subject, space must be found for a suggestion as regards excreta. It has been pointed out that although there are grounds for assuming that the bacillus-vegetation contained in the sputum or nasal discharges does not germinate or grow under ordinary conditions outside the body, yet there are strong reasons for inferring that the forms will grow on the solid excreta of animals affected with tuberculosis, and will revert to their parent-forms and overrun the surface of the excreta—if the conditions as to light, moisture, etc., are not inimical. In the case of man, the probabilities that this has been, or is, a source of danger are remote; but in the case of domesticated and caged animals, it may easily be conceived that contaminated excreta may be the means of adding largely to the sum of bacillus-vegetation within a circumscribed space. The questions arising out of this view must be left to those within whose province they more especially come).

Amongst the most instructive facts in this connection are those relating to the freedom of Alpine regions from phthisis, inasmuch as they shed so much side-light on the position here taken up with regard to the incompetency of bacillus-spores in sputum to induce spontaneous phthisis. I will first explain in a few words the cause of the absence of the disease from these heights by the view based on the inference of accidental parasitism. The reason that the bedrooms of Alpine dwellings have not been invaded by the bacillus-vegetation is that the conditions met with in the space intervening between the lowlands and the uplands prohibit the transit through it of the fruit of this vegetation, which grows freely enough below. The soil and the conditions of growth are there in the highland bedrooms, but the true reproductive organs of the vegetation cannot get up to the bedrooms. Hence the non-occurrence of phthisis sporadically. Then, when introduced into a bedroom by a phthisical patient, the bacillus-spores in his sputum are incapable of directly infecting the healthy, or of germinating and growing on the matrices in the dwelling—and of thus infecting the healthy indirectly. Hence the non-extension of the disease. This interpretation of the phenomenon of the absence of phthisis from the heights fits in at once with the assumption of accidental parasitism. But, seeing that the space which is impassable to the natural bacillus-vegetation of the lowlands has been bridged over for sputum-dust by the numerous highlanders who have descended to the plains to contract phthisis, and have returned home to recover or to die, it is simply impossible for the contagionist to account for the non-transmission of the disease. Cold will not assist him, for the temperature of the interior of the bedroom of the indigen is not so low, even in the cottages of the peasants, as to destroy the bacillus-spore.

The argument of the "anti-phthisical constitution" of the mountaineer is too thin for his purpose, for it is disposed of by the patients themselves, as Dr. Andrew shows in his Lumleian Lectures. And there is no other even plausible reason to assign for the fact that sputum-dust does not hang about the houses of these returned consumptive highlanders in an efficient state for years, as it does, it is taught, in the houses of lowland consumptives —or, for that matter, in lowland houses into which a phthisical patient has not been known to enter.

In effect the contagionist has on hand here an awkward surplus of efficient sputum-dust, which, do what he may, he cannot get rid of, or render inert or innocuous. But supposing he does attempt to refine it away—supposing him to maintain that cold does materially affect the spores, that the "anti-phthisical constitution" is a much more serious bar to the reception of contagion than has been made out, that authentic records as to transmission or non-transmission are wanting, that the known instances of non-transmission are too few for generalization, that there may have been many unrecorded instances of communication of the disease, that the open outdoor life of an Alpine family ensures that the members are very little exposed to the atmosphere surrounding a sick member, etc.—supposing that by these or other specious arguments the contagionist shall convince himself that the non-extension of phthisis among the indigenous inhabitants of Alpine regions is compatible with his tenets as to sputum-dust—he will then find himself confronted with the far harder task of reconciling with those tenets the non-extension of phthisis amongst the foreign residents at the numerous Alpine health resorts, and the amelioration and recovery of a large proportion of the phthisical patients who have gone to these heights.

The problem in connection with these phthisical com-

munities must have been a sore puzzle to those contagionist physicians who have sent their patients to swell their numbers. It is easy to perceive, indeed, the strange conflict between the theoretical and the practical in their minds. At first there was fear and trembling about consigning consumptives, tentatively, to a great bureau or exchange for sputum-dust; for by all the laws of contagion this should have been a deadly experiment. Yet as years passed by, and hundreds and thousands in the aggregate flocked to these resorts with marked and lasting benefit, in spite of a larger accumulation of sputum-dust within a given area than the world had ever seen, and as there has not been an instance of the extension of the disease to the healthy residents—the hotel-keepers, servants, physicians, tradesfolk, mechanics, visitors, and others—some of the strongest advocates of the principles of contagion have become the boldest exponents of the advantages of Alpine air. They admit that the exemption of all the healthy and the improvement of all the patients (excepting, of course, a proportion of those with large cavities and other hopeless lesions and complications) is not altogether satisfactorily explained; but, reserving for themselves a *locus penitentiæ* in the shape of cautious references to the possibility of a future outbreak, they accept actual results and shrewdly throw their theory over. Others, again, still true to their faith, will give only a grudging assent to their patients' going to the Alps. Their position seemingly is that if patients *will* go and recover under conditions which are subversive of the great laws of contagion, they must be allowed to do so—under protest. So they wash their hands of all untoward consequences, and let them go.

The gathering together of these phthisical patients on Alpine heights is undoubtedly the grandest experiment that could have been devised to test the view that the bacillus-

spores in sputum are incapable of causing phthisis, and it does not detract in the slightest from the value of the lessons to be learnt from it that the experiment was initiated by those who were moved by the rule of thumb. The facts brought out are indisputable, for, although of the nature of clinical facts, they are neither uncertain nor inconclusive, but, on the contrary, are absolutely certain and perfectly conclusive, and they serve admirably to try the questions of pure parasitism and of sputum-dust. The etiologist who shall attempt to elucidate the mystery of the cure of these phthisical patients, and of the immunity of those in close relations with them, with no other light than that derived from the doctrine of pure parasitism, and to elucidate it in such wise as not to clash with the fundamental dogmas of contagion, must inevitably tie himself in a knot, or will have to obscure the matter by a cloud of sophistry. He cannot get over the one plain and obvious conclusion from his own views, namely, that the enormous sum of sputum-dust deposited in and distributed through the habitations of the different Alpine health resorts should have produced the most pernicious contagious effects. And he has cut himself off from all escape from the conclusion by the nature of the arguments he has used to show how phthisis is caused by sputum-dust among other communities. We see this at a glance by looking at his main arguments.

Heredity and "hereditary predisposition," the hypothetical "suitable soil," and all the cognate reasons for "catching" consumption by sputum-dust, or for not "catching" it, are thrown away here. They are palpably valueless where we have to deal with a whole population of consumptives, almost all of whom unite in their several persons every cogent theoretical reason for perishing rapidly through reinfection, either from their own or their neighbour's or their predecessor's sputum-dust. These patients present

the greatest facilities and the most favourable conditions for the parasitism of the bacillus in the highest degree of concentration possible, and they breathe at night and when indoors an atmosphere laden with bacillus-spores, and yet the spores, strangely enough, are powerless to effect further lodgments in their lungs; and so, as the parasites in possession soon die out, or are encapsuled, the patients get well, or as well as may be, without a sign of reinfection (and without, it should specially be noted, any such simultaneous recrudescence as obtains at lower altitudes). Moreover, none of the population brought into intimate relations with the phthisical have "acquired" phthisis, which, though less marvellous, is still sufficiently remarkable as being singularly out of accord with the incidence of phthisis in other communities. In a word, the noxious and deadly functions assigned to sputum-dust are all held in absolute abeyance.

The only factor left to the contagionist by which he can attempt to explain away the anomaly of an abundance of sputum-dust with an absence of phthisical effects is altitude. And this has stood him in good stead, and enabled him to tide over the difficulty, after a fashion, so far. The rarity of the air and its aseptic qualities, ozonic, and other conditions known and unknown, extreme dryness, and the intense degree of cold, but above all things the great rarefaction of the air in these elevated regions, have been urged as sources of differentiation between phthisis on the heights and phthisis on the plains. But these are the very shadows of reasons. They would only have form and substance and some force if every particle of sputum from the patients at these altitudes were deposited outside their habitations. Such conditions indubitably affect the patients themselves when they go into the open air; but as the atmosphere in the hotels and lodging-houses and private dwellings is not kept at so many degrees below zero, the sputum of the

patients in them is not in any way affected by the altitude. The issue in this matter is not what happens to the patients or their sputum outside, but whether their sputum is destroyed inside, the dwellings, though by an ingenious twist, or by a convenient suppression, the first and false issue has been made to come uppermost, so as to overwhelm the other. Dragging it from under the superincumbent issue, the plain question before us is simply whether a larger proportion of the sputum of the patients inside the dwellings at these health resorts is destroyed or dissipated than is the case elsewhere. This is the real issue, and upon it hangs the soundness or otherwise of the doctrine of sputum-dust. If it can be shown that all the sputum of the patients, or a far larger proportion of it than in other places, is destroyed forthwith, or rapidly got rid of, as a consequence of altitude, then the non-extension of the disease to the uninfected inhabitants and the absence of reinfection among the phthisical is fully and satisfactorily explained. But if this cannot be shown clearly and distinctly, or if the reverse stands out palpably and unmistakably; and, further, if it can be shown that the proportion of sputum with living bacillus-spores retained in these Alpine houses is equal to that left in the English homes of these patients, or in the homes of English patients of the same class; then the phenomena of non-extension and the absence of reinfection are incompatible with and upset the view that sputum-dust is the agent of ordinary phthisis. If the theory is not wide enough to take in and reconcile with it all the points submitted, it is incurably unsound, or at least seriously defective.

As the apartments occupied by the invalids, as well as those of the other dwellers in these regions, are kept warm artificially, of necessity, it would be absurd to suppose that the spores in the sputum left in them are specially

affected in any way by the altitude; and there are no reasonable grounds that we know of for assuming that the care and attention given to cleanliness in the arrangements connected with Alpine sick-rooms are so far superior to the care and attention shown in lowland houses as to make any material difference in the sum of sputum that would be left in, say, a hundred Alpine and in a hundred lowland houses containing the same number of consumptives for a year. One cannot suppose that the best Alpine hotel where patients are received is, in respect to the depuration of sputum, so infinitely above the houses of the middle or higher classes of "consumptive families" in England as to convert the hotel into a perfect sanatorium whilst the English residences are turned into veritable pest-houses. A very little reflection will show that the history of many of the houses at these resorts would disclose that within the last few years they have received tenfold, or a hundredfold, the amount of sputum that has been received into the oldest private house in the world. And if sputum is supposed to have been left in efficient quantity *in every private house belonging to a "consumptive family" in England*, it is irrational to suppose that it has *not once been left in efficient quantity in any one* of the houses at these health resorts. In point of fact, the theory of sputum-dust unmistakably fails to account for the phenomenon of non-infection here where infection should be most rife.

The history of phthisis contains numberless instances of the occurrence of the disease when sputum-dust is absent, and the converse, but sufficient have been cited. Reverting now to the special question left far back—that of the incompetency of sputum-dust to cause ordinary phthisis—it is submitted that the evidence is almost, if not quite, conclusive that sputum-dust is not concerned as the exciting cause of the disease. I have no doubt whatever upon

this point, as I consider the evidence to be overwhelming; but the point upon which I have some doubts is that of the explanation of the incompetency of the bacillus-spores in sputum to resume spontaneous parasitism in a sound lung after being cast out of the host. The only explanation I have got at appears to cohere, but I give it for what it is worth.

I conceive that the bacilli in sputum represent, but are not identical with, the forms of the vegetation which entered the organism and commenced parasitism; that the bacillus is a parasitic form of a species of fungus growing on matrices external to the body; that the reproductive organs or fruit of this parent-form, entering the air and being inhaled by a person, may be lodged in the lung and become parasitic; that they then take the form of bacilli; that the accidental parasite, or bacillus, is a modified and to some extent a degenerate phase of the free parent-form; that the spores developed in the bacillus are not true fruit but only vegetative forms; that, consequent upon this retrogression, when these spores are set free in sputum from the human host and are introduced by the air-passages into the lungs of another person, or when reintroduced into the lungs of their former host, they cannot resume parasitic life there in either case in the way in which it was begun by the parent-form; that, notwithstanding this diminution of parasitic power, they become actively parasitic and reproduce themselves when introduced into the blood and lymph streams; that they do not germinate and grow and continue the species on the matrices of the parent-forms under ordinary conditions, though they may do so under conditions rarely brought together; and that the bacillus-spores contained in the solid excreta may and probably do grow and revert to the parent-forms, under favourable conditions, at ordinary temperatures, and may then extend

to and overrun the natural or artificial matrices of the vegetation.

This purely speculative view contains nothing inconsistent with botanical or biological knowledge. The two main objections to it are, first, the inference which has been tacitly accepted, that the bacillus is a pure parasite and an independent species; and, secondly, the fact that forms have not been observed in the neighbourhood of the bacillus in the lung which have been shown to be transition-forms, or to be in organic continuity with the bacillus-forms. Letting the first of these objections go, it may be remarked with regard to the second that many and various forms have been found along the air-passages of the phthisical, as well as in such close relation with bacilli in the lung as to require successive cultivations to obtain a pure cultivation; and, further, that some observers have described forms which they have taken to be genetically connected with the bacilli. As Professor Klebs says (Dr. Cheyne's Report, *The Practitioner*, April, 1883, p. 251): "I can definitely state that even in the purest cultivations (Koch's own) there are always present finely granular masses which appear to possess the characters of micrococci." (These micrococci, it may be presumed, are distinct from the *Monas tuberculosum* of Klebs himself.) Drs. Paltauf and Riehl have found cocci (*Staphylococcus aureus* and *albus*) associated with tubercle bacilli. Malassez describes zooglœa, sometimes of spheroids exclusively, and sometimes composed of a mixture of cocci and rodlets, which he takes to be forms of the bacillus-vegetation. Professor Cornil and M. Babès found, sometimes by the side of bacilli, grains which are coloured of the same colour as bacilli by the same manipulations, grains which are not the elements seen by Klebs and Toussaint. Then there are the shorter and thicker rods which have been mistaken by some observers for the tubercle bacilli.

Micro-pathologists, however, in this research have confined their attention almost exclusively to the specific forms associated, as constants, with tubercle formation, and have discarded or disregarded all other forms. It does not follow, therefore, that because nobody has followed up inquiry as to the other forms, they may not be transition-forms of the parent-forms of the bacilli, or that the bacillus entered the air-passages as a bacillus, or as the spore of a bacillus. It is not certain that the first efficient lodgment of the vegetation takes place in the epithelioid cell in which the bacillus is found. This cell may not be the actual site of the primary act of parasitism; but it would be profitless to discuss the precise position of the assumed morphism of this hypothetical mould or mildew. Suffice it to say that from all botanical, biological, pathological, and clinical data extant, it is far more probable, I conceive, that the bacillus is an anærobiotic form of a parent ærobiotic fungus, than that it is a good, autonymous species, the whole life-cycle of which is summed up in the one phase in which it is found in the organism.

By the view here taken it is easy to understand that the parent-forms may grow freely enough at ordinary temperatures, whilst their parasitic descendants in the sputum, modified but not sterilized, though morphosed to fit them for sojourn in the foreign medium of the living organism, cannot be cultivated in the laboratory (by the usual processes) except at blood-heat. The supposition furnishes us, too, with the explanation of the non-infection of the native and foreign residents, and of the non-occurrence of reinfection of patients, in the Alpine health resorts, and of the consequent condition of quiescence in the patients, so long as they remain there. (And the period of quiescence there demonstrates that not only these, but all similar periods of quiescence in phthisical patients at

all other places are due, not to an abeyance of the parasitism of the bacilli in the lung, but to the estoppal of the introduction of further efficient bacillus-vegetation into the lungs of the hosts, and the consequent death, expulsion, or encapsuling of the guests. As a sequence, the parasitic life of the bacillus is feeble and limited, and, unless a constant or frequent supply of efficient vegetation be kept up, tubercle-formation soon ceases.) The view further enables us to perceive that, as sputum-dust is practically innocuous to man, and is wholly innocuous so far as ordinary phthisis is concerned, the fear, therefore, of an outbreak of phthisis at Alpine health resorts is groundless. It is an inference that even if by any mischance or strange accident any of the true fruit of the bacillus-vegetation should find its way up these heights into some one of the houses of the residents, and should find a suitable matrix and favourable conditions for growth, and should cause phthisis, or bring about reinfection of the phthisical, as might happen, the consequences would be limited to the room or rooms of the one house. Almost the only practicable way that suggests itself by which these habitations could be contaminated with the bacillus-vegetation is the one which has been glanced at—by means of the excreta of a patient with general tuberculosis. It is, of course, possible that excreta might be left where they might become a means of starting a field of bacillus-vegetation in one of these rooms; but even so, there would not be, and could not be, any alarming outbreak. For one useful effect of altitude would then come in. Such fruit from the free vegetation in the infected house as might be carried into the air would be rapidly destroyed by the cold, etc., and the bacillus-spores in the sputum of such patients as might be infected by the local malaria would be on precisely the same footing as the spores in the sputum of all other patients—or inert. Con-

sequently there is not the slightest ground for the fear of anything like an epidemic of phthisical symptoms. And when we reflect that phthisis has not been known to occur spontaneously among indigens in the higher Alps, and that the foreign population connected with the Alpine resorts have been free from all signs of infection for several years, we see that it is far more likely that a person will be struck dead by lightning there than that he will die of phthisis contracted there.*

This subject is in the domain of pure speculation, and it must perhaps remain there for some time to come. Still, it may not have been altogether unproductive to have entered a little into it. One assumption in connection with the matter is a cardinal point in my view, and I consider the evidence in favour of its soundness so conclusive as almost to convert it into an inferential certainty. It is that a bacillus-vegetation of some description, whether in the shape of a mildew, or in the shape of the bacillus itself, or in some other shape, grows somewhere outside the body, both in the natural flora and in some of the dwellings of a country, at ordinary temperatures. The view we have been specially considering, namely, that the bacilli and their spores in the vegetative phase in which they leave the organism in the sputum are unable to reinfect the organism, is of large but secondary importance. The conception that the sporulation of the parasite in the lung is defective in the sexual element, and that the modification of its reproductive organs, though not interfering with its multiplication, lessens its parasitic power, and also inhibits, or limits, its growth outside the organism, may be unsound

* It will be understood, of course, that these remarks are strictly limited to phthisis. If the sewage systems at Davos or the Engadine are worse even than in London, and England generally, the residents on these heights must expect a larger proportionate death-rate than that of England, from typhoid, scarlatina, diphtheria, etc.

without affecting the fundamental view that the bacillus is an accidental parasite. The periodicity of intensified phthisical manifestations amongst the consumptives of a whole district, and the occurrence of the same phenomenon amongst Australian cattle, are pregnant with significance on this point; for this periodicity, coupled with the absence of a similar periodicity at Alpine health resorts, has no rational interpretation but the one. It is as plain, simple, and direct an inference as ever etiologist had to draw, that the periods of general recrudescence and quiescence correspond with and depend upon periods of activity and subsidence of growth of the bacillus-vegetation on its natural and artificial fields outside the body. Given that the bacillus in the lung is the specific agent of the morbid changes in it, there is no escape from the conclusion that a marked increase in these changes throughout a locality implies an increased quantity, or an enhanced quality or value, of an external bacillus-vegetation forced into rapid growth by seasonal conditions common to the locality. The conclusion is so obvious, so glaring indeed, that future etiologists will marvel how, in the face of such well-known phenomena, it could have been supposed for years that the bacillus is a pure parasite.

By assuming provisionally, pending the ultimate determination of the question, that the bacillus-vegetation is a mildew, or mould, or fungus, growing on its ordinary matrices in its natural localities, or on made soils in dwellings, or in the places in which cattle are confined, we are enabled to apprehend not only the cause of the seasonal variations of phthisis in a particular region, but the cause of variations in the general intensity, or in the type, of the disease within the region; and, moreover, the cause of the variations in the type of phthisis observed in regions wide apart. Confining the view now to the artificially

grown vegetation by which man is infected, we see that its virulence is governed by the local soil and by the local conditions furnished by different habitations in the same country. But over and above these variations in a given latitude, the corresponding variations in another latitude are on a higher or a lower scale—as is evidenced both by the incidence and by the type of the disease in different countries, notably in Northern and Southern Europe, and in the tropics, and even to a limited extent in the north and south of England. The variations of type in a given country might be accounted for by the contagionist by the supposition of more or less sputum-dust, but the supposition is not available for comparative variations in different countries. That the standard of the malignancy of phthisis should be higher or lower in this or that civilized race on any part of the earth's surface, is incomprehensible by the view of a bacillus-spore, which, being a pure parasite, is unaffected by conditions external to man.

The assumption that the bacillus-spores in sputum are inoperative as regards inducing phthisis, is necessarily dependent on the assumption that a bacillus-vegetation exists outside the body at ordinary temperatures, and it may be considered premature to have discussed the former whilst the soundness of the latter is in suspense. But the questions relating to both assumptions are so interlaced, both are such necessary parts of a complete survey of phthisis, and the one lends such strength to the other, that, though it may be conceded that assumption upon assumption is not always safe or sufficiently cautious etiology, I have elected to run the risk of being held to be speculative even to rashness rather than to leave important phenomena entirely out of consideration, and thus, perhaps, to create the suspicion that they are inconsistent with the leading views of accidental parasitism.

Reverting finally to the case I put of the artificial contamination of a sick-room with an enormous amount of sputum, it will be obvious that for many reasons I conclude that phthisis would not be a result of such a degree of contamination, or of any lesser degree of contamination. In fine, I infer that sputum-dust does not under any circumstances induce spontaneous ordinary phthisis, and that the theory that it is the invariable exciting cause of the disease is a widespread delusion, based on the old doctrine of contagion, and strengthened by the modern view of pure parasitism.

In closing this section of the subject, I would remark that the frequency with which phthisis supervenes upon acute or lingering debility is fully accounted for by the supposition that, as the invalids remain in their bedrooms, often purposely and over-officiously darkened by nurses or relatives, to keep out "the glare," they are, therefore, more than ordinarily exposed to the influence of the specific local malaria that occurs in such rooms. And as it would seem to be the law in parasitism that "the weaker the host the stronger the guest," a relatively small amount of bacillus-vegetation may cause rapid and serious or fatal lesions. And this tendency of the invalided to "acquire" phthisis, when no hereditary predisposition can be traced, and where sputum-dust cannot be assumed, or can be assumed only by offering violence to all probability, stamps the sick-room itself as the source of the bacilli, and points unerringly to their derivation from a growing vegetation outside the body of the patients. I say unhesitatingly that there is no inference in etiology so easily reached or that approaches nearer to a certainty than that the debilitated who "acquire" phthisis in such rooms under such conditions are the prey of an accidental parasite.

LIGHT.

THE exigencies of the argument have called for so many references to chemical light that the reader is already possessed of the main principles of the law by which I suppose it to govern the amount of phthisis in a country. But, as the sun dominates in this matter with such enormous and relentless power that it is to be inferred that ninety or ninety-five per cent. of the total mortality from consumption of the whole population of the globe has depended entirely on the modes in which different races have received his rays into their habitations; and as, consequently, a full knowledge of the precise effects of the rays which impinge upon the earth's surface is indispensable to devising the means of preventing consumption amongst civilized communities; the subject demands the most earnest consideration and the most careful study.

It would be superfluous to examine into the historical aspect of the question of light in connection with phthisis, though many of the older physicians recognized the baneful influence of darkness and the especial value of sunshine in treatment. In more recent times there is scarcely a writer of any note upon consumption who does not refer to the subject in some shape. There has been not only a consensus of opinion as to the noxiousness of damp, dark, crowded places—matters which chiefly affect the poorer classes and the dwellers in large towns—but there has been an increas-

ing tendency of late among physicians to impress upon the consumptive classes, not exposed to the evils of overcrowded and dungeon-like dwellings, the importance of, or the absolute necessity for, basking in the sun, and surrounding themselves with as much sunlight as possible. A few physicians, indeed, have made this a leading part of their treatment, and have gone beyond, and laid down rules for the aspect, size, ventilation, etc., of bedrooms. One of the exponents of this mode of treatment is Dr. Weber, who says in his Croonian Lectures (*Lancet*, April 4, 1885): "The invalid ought to have sunny rooms, sitting as well as bedroom, for though the sun does not shine at night, the vivifying influence which it exercises on the bedroom during the day does not disappear at once with the cessation of the sunshine, but lasts through the night and longer. . . . In phthisis, the long stay in bed, and particularly in the bedroom, is certainly injurious. . . . In summer, and also in winter, weather permitting, a couch or bed ought to be placed on a balcony or terrace, or in an open field or garden; the patient ought to be carried to it, and, properly covered, lay there from morning to night, or as long as possible. . . ." Speaking of hospitals for consumptives, Dr. Weber observes: "What I should like to add are terraces and balconies, on which light beds or couches could be placed for febrile patients to lie on, and thus to enjoy the open air; and, further, large verandahs, glass covered, and with movable glass doors to be opened and closed at will. . . . I should also be inclined to erect in suitable places walls with reflecting surfaces, to serve as shelter from wind and as reflectors of the sun's warming rays."

The scheme of treatment sketched in the Croonian Lectures fairly embodies modern views as regards solar radiation. Or perhaps it marks an advance beyond contemporary views, in that it distinctly lays down that the

phthisical invalid should have a sunny bedroom, and specifies means and appliances for surrounding patients with floods of light, and even "reflectors of the sun's warming rays." Most authorities lay stress rather on the open, well-ventilated or aseptic air than on the complete sunny environment; but Dr. Weber, by his method and insistence, shows that the rays of the sun are of primary importance in his plan of treatment. He does not trust to uncertain generalities, but enters into specific details. However, in one way or other, solar radiation crops up in every treatise on phthisis almost as certainly, though not so prominently, as heredity. Whether in connection with the dwellings of the poor or with the regimen for the wealthy, the subject of the sun in these days is invariably either remotely glanced at or directly referred to by etiological writers and practising physicians. Everybody, too, who has intimate relations with the phthisical know how they long for the *grateful* warmth of the sun—a warmth which cannot be indicated by the thermometer. One of the very earliest signs that the youth in a "consumptive family" has entered upon the mortal struggle is the *stone-cold* feeling which no fire, no mere increase in the temperature of a room, suffices to overcome.

One might ring the changes on this subject, and show the estimation in which the sun is held in different countries; and, indeed, one might compile an interesting and valuable work exhibiting the modes in which the inhabitants of high and low latitudes have adapted themselves to their several positions in regard to solar radiation. Such a work would be eminently useful as a means of bringing out the relations of the sun to the death-rate of a given people from consumption. But my special object here will be sufficiently attained, perhaps, by briefly referring to a large body of almost untouched facts and

views which, however distantly they may seem to be related to the question now, will be found sooner or later to be inseparably bound up with it.

If the recorded clinical cases and the deductions and ordinances of physicians relating to the sun and phthisis be duly and carefully weighed, it will be seen that a deficiency of solar radiation is observed to be associated with the occurrence of a large proportion of cases, and that solar radiation is found to be singularly efficacious in the treatment of all cases of consumption. It will, therefore, be evident that the sun in some manner influences or governs the incidence of phthisis—that it plays a part, and an important one, in its occurrence or non-occurrence. This conclusion is easily arrived at; but when we come to inquire into the precise part played by the sun, the difficulty of conceiving in what way solar radiation and the tubercle-bacillus work together as factors in the sum of phthisis is enormous, and, in the existing state of things, insurmountable. It is simply impossible by any process of reasoning, as matters now stand, to bring out any adequate hypothesis by which an interdependence between the sun and the infective agent can be shown. In referring to the "vivifying influence" of the sun, Dr. Weber omits to explain what that influence is, or how it affects the bacilli in the lung of the patient. His whole procedure is based, apparently, on some bacillicidal property in sunshine; but the very omission to give the rationale of the treatment is a tacit admission that there is no way of accounting for the influence of the sun. It is not to be done consistently with the tenets of contagion; and the plain fact, I suspect, is that Dr. Weber could not untie this knot. And neither he nor the whole school of etiologists shall untie it so long as the inference of the pure parasitism of the bacillus is intertwisted with it. This cardinal error in the

ratiocination of Koch, if adhered to, will defy every effort to bring, not solar radiation alone, but all other now obscure and inscrutable phenomena associated with the occurrence or non-occurrence of phthisis, into accord with the dissemination of the parasite.

On the other hand, by the assumption of accidental parasitism, it is found here, as elsewhere, that all difficulties vanish at once. The influence of solar radiation on the fields of the bacillus-vegetation external to the organism and the consequent effect upon the incidence of phthisis are apprehended without effort. Whereas it is incomprehensible how the mere presence or absence of solar light should act injuriously or favourably on a bacillus-spore in a state of absolute rest outside the animal body, or on the bacillus itself inside the body, it is easily to be understood that a form of vegetation which grows in a free state, and therefore requires suitable soil and certain conditions for its independent growth, will flourish or perish as the amount of solar light to which it may be submitted shall determine. The one form is practically indestructible, it is taught, when long exposed to insolation ; the other is, inferentially, dependent for its very existence on a low degree of chemical intensity in the solar radiation that may be brought to bear on it. When once we grasp the conception that in the bacillus we have to do with an accidental in place of a pure parasite, the unknown quantity in the treatment of the physician who surrounds the phthisical patient with sunlight is disclosed. It is seen to be no longer empirical to flood the bedroom with solar radiation, for there is a definite object in using this two-edged weapon : it is wielded against the existing bacillus-vegetation and against its soil as well; and by destroying both the continuance of a bacillary malaria in the room is impossible. There need be no doubt, no hesitancy, about

employing this means on the score of uncertainty in its aim, for its object is as plain as noonday. Moreover, the measure is congruous in every respect, and squares exactly with the demonstration that the bacillus is the infective agent in phthisis. By the assumption that the bacillus is an accidental parasite, the modern practice of utilizing the sun becomes converted into treatment of a high scientific order, whilst by the opposite assumption of pure parasitism it is, and must ever remain, unmeaning and unintelligible—although successful in proportion to its approach to completeness or thoroughness. The facility with which, as has been seen all along the line, every obscure or enigmatical phenomenon connected with the occurrence of phthisis adapts itself to the theory of accidental parasitism, is again seen in this matter of the application of chemical light to the treatment of phthisis. And, in addition to the accumulated evidence tending to show the soundness of the theory, it is to be observed that it is a working theory; that it is not merely an abstract theory which may serve some useful purpose in a remote future, but a breathing, living theory, ready for immediate service in the prevention, as well as in the treatment, of phthisis. The reigning theory has failed all round; it is irreconcilable with many clinical facts, and it is admitted to have been barren of almost all immediately useful results.

But for the undetected error, as I take it to be, in Koch's conclusion, the question of enlisting the sun into the treatment of consumption would have stood upon a very different footing. It is a popular belief with most people that the sun has a beneficial effect upon the health; the faculty as a whole recognize the view, and many physicians have strong convictions on the point; but if we sum up all the knowledge we have of the influence of the sun on man and his surroundings, it amounts to little more than has been

compressed into aphorisms and proverbs. All the discoveries and observations in solar chemistry put together give us a mere glimpse of the astounding power of the sun over living things, and—partly owing to the limited number of physicists who have turned their attention to the subject, but principally because the vast importance, for good or for evil, of solar radiation has not been appreciated—medicine has not profited much. It is extremely doubtful whether any man, or any body of men, could at the present moment give a full explanation of the Italian proverb, "Where the sun does not enter, the physician enters;" or of the Indian saw, "He who plants a tree in front of his dwelling, begins to dig his own grave."

Science has not the data to enable physicists and physicians to say what is the mean chemical intensity of total daylight at given places on the earth's surface—in Italy and India and England, for instance—and what effect the difference, if any, between the means of the intensity at these places has, directly and indirectly, upon their inhabitants. This want of exact knowledge naturally induces a degree of scepticism as to the influence of the sun in some minds and a lukewarmness of belief in other minds. The bulk of the faculty in England would probably agree that the sun has its value; but the sun does not represent to them an ever-active, intensely energetic vital force, a neglect of, or a disregard for, which means death to millions annually. As we see in England none of the more striking manifestations of the sun's power that are seen in lower latitudes, and as the disastrous results which come of excluding a sufficiency of solar radiation from our houses, and of our being deprived of that large proportion of the sun's chemical work which is lost to us (through absorption by clouds, by finely divided solids in the atmosphere, etc.); as these results are effected stealthily and steadily, we

have come to regard the sun, when he appears, as a mildly benignant luminary, and to overlook his marvellous prepotency in all that relates to the nutrition and to the environment of organisms, and to the lethal consequences, slow but sure, of neglecting to cherish and utilize his rays to the utmost when they are enabled to penetrate to the surface of these islands. Though most Englishmen, and some Englishwomen, dislike sunless rooms, yet the deleterious results of excluding sunlight from a dwelling do not follow so rapidly with us as they do in Italy and warmer countries; so that, as the cause does not tread on the heels of the effect, we have missed the inference which has been forced upon Italians and others. Hence, whilst we have a few proverbs or sayings that point to the salutary nature of the sun's rays upon our bodily health, we have none that convey a lively sense of danger in shutting them out from our habitations. When a lecturer, therefore, lays down a plan for submitting phthisical patients and their bedrooms to extreme solar radiation, without assigning any more tangible reason for the practice than the "vivifying influence" of the sun on the air of the room, it is not much to be wondered at, perhaps, all things considered, if a large proportion of the profession regard the proposal somewhat in the light of a crotchet or a "fad."

But the matter would have worn another aspect had the lecturer been in a position to show that the bacilli in the lungs of patients derive from vegetation growing in their bedrooms, which vegetation is enabled or encouraged to grow there and cause a malaria by insufficiency of chemical light. This would have so altered the complexion of the case that, instead of the passive reception of the plan of active sun-treatment proposed by Dr. Weber, physicians as a body, realizing the true relation of the sun to the bacillus, would have taken up the suggestion with alacrity, and the

whole subject of phthisis might have been revolutionized by this time. But it could not be expected that the sterile doctrine of contagion mated to the equivocal pure parasite should bring forth such fruit.

Another and greater evil than has accrued in the matter of the treatment of consumptives has come of the interposition of this obstacle of pure parasitism. The delay which has occurred in the effective treatment of those who are actually phthisical is sad enough, but the indefinite postponement of energetic systematic measures for the prevention of phthisis is a far more serious calamity. If a good substantial reason could have been assigned by physicians for letting the sun stream into a patient's bedroom—such, for instance, as the preclusion of a bacillary malaria—it is absurd to suppose that the application of the principle would have stopped short at treatment. For it is palpable that if phthisis is to be cured by admitting sunlight into a bedroom, the malady is to be prevented by the same means. If phthisis is caused by a bacillaria in the bedroom, and if the admission into the room of solar radiation of a certain degree of chemical intensity precludes the bacillaria, and enables a patient to recover, it follows that the patient would not have contracted phthisis if his bedroom had from the first admitted the requisite solar radiation to keep it free from the bacillaria. Phthisis, therefore, is an unnecessary disease—to be prevented to a certainty by the very simplest of arrangements. Every man of adequate means may, at will, preserve himself and his family from consumption, while with the poorer classes it is a mere question of expenditure what the mortality of a country from this disease shall be. The death-rate of a civilized community from phthisis may be determined by any State with precision; for the whole matter resolves itself into one of cost and legislation.

All this, which is an obvious sequence from the premiss, would of course have flashed through the profession if only the correct interpretation of the cure of phthisis by sun-treatment could have been had. But how was it possible to get at the interpretation by means of a theory the fundamental doctrines of which are irreconcilable with the cure by sun-treatment? By the view of the pure parasite, and of its offshoot sputum-dust, recovery from phthisis by this mode of treatment is an anomaly, an absurdity. It ought not to occur. And it is impossible to evolve the principle underlying sun-treatment, if regard be had to the theory that the spores in sputum undergo no change outside the body. The doctrine that the vegetation does not grow outside the body, and is not, therefore, affected by the sun's presence or absence, shuts out all rational explanation of the sun's influence in effecting the cure, and leaves the advocates of sun-treatment in a dilemma; for while on the one hand the bacilli from the patient are (theoretically) neither destroyed, nor injured, nor lessened in number by the sun's rays, yet on the other hand it is seen plainly enough that by letting the sun's rays freely into the room, the bacillus-vegetation disappears from the air breathed by the patient, or its potency is in some way destroyed, or materially reduced. Some of the advocates of sun-treatment have, nevertheless, been unable to shake themselves free from the tenets of contagion. They have been so long accustomed to regard contagion as a fixed point that they have not looked at the legitimate conclusions from their treatment. They could find no orthodox reasons for utilizing the sun in curing consumption, and *à fortiori* there were none forthcoming for employing him in preventing consumption. Consequently medicine has secured but a meagre instalment of the total amount of the sun's value, and the whole question of the suppression of phthisis by

chemical light will be hung up as long as the theory of sputum-dust shall prevail.

The dictum of pure parasitism, by virtue of its brevet rank as a truth, has so overshadowed, beclouded, and befogged every question in tuberculosis, that etiologists would seem to have been unable to take independent thought. The facts that have presented themselves with regard to the sun, for instance, have led them straight to certain conclusions, but they have not essayed to push them home, because the conclusions were obviously inconsistent with the accepted views of sputum-dust. So that although, as in the case of sending patients to Alpine heights, they do not hesitate about employing and inculcating sun-treatment, inasmuch as experience has shown its rare value, yet the pure parasite theory and its corollary sputum-dust stand in the way of the inviting explanation of the sun's value in treatment; and therefore they have not thought of, or ventured upon, extending sun-treatment to sun-prevention. They have been deterred from drawing the plainest and most obvious inferences from their own pronounced and decided views upon the effect of sunlight in treatment. Many instances of this occur in medical literature, but the best illustration of our meaning, perhaps, is to be found in an article by the late Dr. B. W. Richardson in "Our Homes" (*op. cit.*), on "Health in the Home." The Hawker's Van case stated by Dr. Richardson is so interesting and important in many ways that we quote it at length, first giving extracts from the article to show the admirable views he held as regards the properties of light.

Dr. Richardson says: "Pure light is as essential to health as pure food and drink. We are but just beginning to understand its vital value. Still, we now do know that those who are immured in dark places become etiolated, or blanched, anæmic, feeble. We are beginning to know more

than this. We are learning that by the action of light the poisonous organic products which produce disease are decomposed, or rendered inactive. I found this to be the fact in respect to the poison of the cobra di capello, which poison retained its active properties in the dark, but lost them on exposure to the rays of the sun." In another portion of the article occurs: "A fourth principle in house-sanitation is to ensure the full admission of light. There ought not to be a dark room in any human habitation. It is almost impossible to let in too much of the sun. As a rule a south-western light is most desirable, inasmuch as that light feeds the dwelling for the greater portion of the day. . . . It should be kept steadily in mind that the light not only makes all clear that should be clear, but that it purifies, and that when it cannot be directly admitted into a room, it should be admitted by being reflected from a good reflector. In planning windows the utmost care ought to be taken that they are of sufficient size, and that they are not over-shadowed. . . ."

In dealing with the investigation into the mortality from phthisis in the army, Dr. Richardson observes: "A single agent, *vitiated air*, acted with such intensity—especially when superadded to a certain degree of exposure—as not only to produce in the Foot Guards an amount of chest-disease, and especially of pulmonary consumption, greater than was produced in civil life by all the other causes united, but actually to carry off annually a number of men nearly equalling in the infantry and actually exceeding in the Guards, the number of civilians of the same age who died from all classes of disease. . . . The record of these observations is the best and most forcible, because most extended and accurate, that has ever been supplied respecting the influence of confined air in the living and sleeping apartments of men. . . . They" (the

Commissioners) "recommended that an entirely new system should be introduced into barrack-life; that air, fresh and pure, should at all times circulate through the buildings, and especially through the dormitories. . . . Since these regulations have been in force, the English soldier at home has no longer the unenviable position of being first in the ranks of those who fall victims to pulmonary consumption. . . . In many of our best houses—I mean best in relation to their appearance and cost, not in respect to their construction—the errors that were common in the barrack are still present, and rooms are used as sleeping-rooms which stand in the eyes of the sanitarian like so many experimental boxes for the synthetical development of pulmonary disease."

With regard to the Hawker's Van case, Dr. Richardson says: "I have myself shown that consumptive persons who have lived in the same apartment have successively become affected by the disease, the following being perhaps the most striking of these facts:—A man, by business a hawker, a 'Cheap Jack,' who was accustomed to live, with some other members of his family, in the van in which he travelled from fair to fair, and from which he sold his wares, was brought to me in the third stage of pulmonary consumption. He soon succumbed to the disease, and was succeeded in business by his brother, who followed precisely the same line of life, and came to live in the same van. His brother soon afterwards became consumptive, and died. He, in turn, was succeeded by his sister's husband, who shared the same fate; and, not to extend the narrative to an undue length, in the course of seven years I had before me no fewer than nine victims of the fatal pulmonary disease, in every one of whom it seemed to originate in that particular travelling-van. It was in vain I protested to those affected against continuing to live under conditions

so favourable to disease. They argued that they were constantly in the open air by day, that they got regular change of air, that they were not exposed to wet, and that at night they were very snug; in short, they would not believe that the sleeping in the van-house had anything whatever to do with the disease.

"The observation of a series of facts such as these may at first sight seem to convey the notion that the poison of the disease was conveyed in the van, and was communicated from one series of its occupants to the next. The evidence is not conclusive. The first sufferer had bought the van new, and first occupied it; he, therefore, did not get the disease by a process of continuation. The van was afterwards regularly emptied, cleaned, newly painted, and exposed to the air; so that the probabilities of the continuance of specific communicable poison in it are most remote. The practical truth is, however, none the less valuable that the disease originated so many times under the same conditions, and truths of this kind cannot be too often related."

But for the deadening effect produced upon the mind by the theory of sputum-dust, it would be a marvel that Dr. Richardson, considering the certainty of his conviction that the van was the source of the disease, and the knowledge he had of the action of the sun in destroying the vitiated air of bedrooms, should have omitted to apply his knowledge in this instance. For of all the "experimental boxes for the synthetical development of pulmonary disease" one more favourable for the experiment can hardly be conceived than this van—assuming it to have been constructed on the deadly principle on which so many of them are, namely, with one window at the gable end about a foot square, and placed high above the floor. If we further suppose the van to have been largely taken up with the goods of a hawker,

it is obvious that whilst no portion of the interior could have been sufficiently lighted, parts of it will have been for long periods thrown into deep shade. By converting this "experimental box" into a dormitory, all the conditions for the development of a bacillary malaria were brought together; and on the data furnished by Dr. Richardson, it is almost an inferential certainty that the hawker's van had been made a pestiferous den simply and solely by excluding an adequate proportion of the sun's rays from its interior. If this inference is sound, it follows plainly enough that all that was required to transform the van into a comparatively wholesome dormitory, was to have admitted into it a certain amount of chemical light either through the roof or sides. How was it that Dr. Richardson omitted to draw a conclusion to which his argument led him? And that his contemporaries failed to point it out to him? Other physicians and etiologists have cited cases similar in principle to the Hawker's Van case, and have held similar views as to the effects of light to Dr. Richardson's, but like him they have been deterred from giving practical effect to them.

The position is a curious study. It is out of the question that a body of men comprising dialecticians of high order had not the capacity to work out that which presents itself as a very simple problem. That they have not done so is only to be accounted for by some controlling and overpowering *ultima ratio*—some accepted fact or admitted truth, or some doctrine or dogma that has for them all the force or impress of truth or fact—which bars certain lines of thought and shuts out particular conclusions. It will be evident that I refer to the unquestioned assumption as to the botanical nature of the bacillus. This, I take it, explains how it is that the relation of the sun to phthisis has not been seen, and that the question of the prevention, as well as of the cure, of consumption by means of the sun

has not been, and could not well have been, entertained. For by the view of pure parasitism—the stepping-stone to sputum-dust—the etiologist cannot move towards prevention without being confronted with anomalies and absurdities. The recognized effect of the sun in the treatment of phthisis being an inconsequence, he would have to start with the paradox that the sun influences that which is not affected by it; and it is no great wonder, therefore, that the task of reconciling the action of the sun with the theory of sputum-dust has not been seriously attempted even by the advocates of sun-treatment. And until the etiologist shall come to see the bacillus in its true light, and shall thereby apprehend the fallacies in the long-enduring doctrines of contagion, heredity, favourable soil, "acquired" phthisis, etc., he will not understand how it is that the sun determines the sum and value of the bacillus-vegetation in a given locality, and that, as a consequence, it remains with man himself to control with certainty the amount of consumption in the world.

This is the brief of the case against the pure parasitism of the bacillus. It is unnecessary to recapitulate the large body of facts brought forward, or to sum up the argument. It will be sufficient to observe that, as all the facts are in perfect accord with the view of accidental parasitism, at least a *primâ facie* case for the main propositions has been made out. If from lack of material, from faulty arrangement, or from feebleness in statement, I have failed to make the most of the indictment against the botanical position assigned to the bacillus, I trust that the case will fall into abler hands. It is not to be supposed that I consider the indictment complete—that all has been said that could have been said. Whole groups of phenomena have been left untouched, but they have been carefully examined

The large field of tuberculosis, for instance, of which phthisis is but a patch, has barely been alluded to; yet I have explored it, nevertheless, with the most earnest care. For it is manifest that the part must agree with the whole. And the result of the exploration may be shortly summed up by saying that every fact connected with tuberculosis has appeared to be in perfect harmony with the central view of the bacillus here submitted. It may be repeated that if any one fact in the whole history of tuberculosis shall be found to clash with that view, so as to be perfectly irreconcilable with it, the theory that phthisis is caused by a local bacillary malaria falls to the ground.

Setting aside the multitude of questions relating to tuberculosis generally, a great many etiological points connected with phthisis specially have not been gone into. It is not necessary to indicate these wide gaps in the argument, which no doubt give a ragged look to it and may furnish occasion for criticism. All I need say is that the enormous bulk of material to be handled in going through the history of phthisis left no choice between compiling an exhaustive treatise and selecting a few of the most prominent subjects and of the most salient points. I elected to take the latter course, because of the necessarily speculative nature of the inquiry and of the ephemeral life of all contributions to etiology. Moreover, if the reasons advanced in support of the novel views here propounded are held to be insufficient to shake or affect the supremacy of Koch's view, they will not acquire force by iteration. Their validity does not depend on cumulative effect; and although the conscientious accumulation of inferential proof undoubtedly lends weight to a proposition, the weight may become so oppressive as to justify the canon that the part is sometimes greater than the whole. Wherefore I have preferred to understate rather than to overcrowd, and to

reserve for a future occasion any supplementary remarks that may be called for on account of serious omissions, or to repair such damages as may accrue from attacks on the score of shortcomings, instead of attempting vainly to anticipate and meet every objection that may be raised.

Whatever defects may be found in the conception of the botanical position of the bacillus, or in the method or want of method in bringing it forth, or in pointing out its immediate consequences, I have been deeply impressed with the immense importance of the question to mankind. All things considered, the cause of phthisis is one of the grandest questions now before the world. It is indeed a noble study; one that may worthily fill the thought; and the work here has been labour of love. Let the result be what it may, I have striven hard to get at the abstract truth, and have taken but few things for granted. Whether I have succeeded in showing that it has been Koch's own extrinsic inference which has nullified the intrinsic value of his great discovery, I must leave. One proposition, however, that I have already submitted, I can again submit without a shadow of a doubt as to its soundness. It is that if it be granted that phthisis is caused by a local bacillary malaria, not only does it follow that the bacillus is an accidental parasite, but that the prevention of phthisis is to be effected with absolute certainty to an extent proportionate to the means employed. The premiss being conceded, the conclusion is inevitable and demonstrable; and the premiss is well-nigh forced.

PREVENTION.

THE question of the prevention of phthisis has now to be considered. As the conclusions here depend upon the soundness of the views propounded as to the bacillus, it may be thought that their consideration should be delayed until the question concerning the nature of the bacillus shall be determined. Having weighed the objections to entering into the matter at this stage, however, I find that they are of no great force, and are all to be outweighed by the least of the many reasons for giving the outlines of a scheme of prevention based upon the view that phthisis is caused by a local bacillary malaria governed by chemical light. The scheme itself is merely an extension, or amplification, of a means which is at present employed in the cure of consumption, and has been indirectly, but successfully, used for its prevention. It is an attempt to reduce to definite principles a plan of treatment which is now advocated by some physicians, and to enlarge the plan so as to take in prevention as well as treatment. It is impossible that it should be injurious, for it does not preclude any of the observances and ordinances which have been imposed upon the "consumptive families" for greater safety—though at the same time I conceive that the sooner some of them are annulled, the sooner a stigma upon rational medicine will be removed.

Whilst Governments or States would not be warranted in acting upon the views of prevention here given without

the completest and strongest evidence as to their validity, yet before the question is matured for concerted or national efforts, I believe that a vast amount of suffering may be alleviated and many lives may be saved, if private individuals, whose position may enable them to do so, will carry out the practical measures suggested without waiting for the approval of the general voice. Public steps will not be taken, and certainly public expenditure will not be incurred, in this country, until the clearest possible case is presented and the most urgent need is shown for legislative intervention. The faculty in England, we may be sure, will not move as a body until the whole question is threshed out, and not before a preponderating majority, including the recognized heads, of the profession are assured of the fact—if it be a fact—that Koch's inference as to the pure parasitism of the bacillus is erroneous, and that the opposite inference (there is no *tertium quid*) is sound, and leads to the important consequences here supposed. How slow they are to move whilst any doubt clings to a subject has been seen, fortunately, in this very matter of phthisis; for though in Germany and elsewhere no time was lost in issuing stringent and vexatious (and now admittedly inefficacious) regulations, founded on the discovery of the bacillus and on the inference as to its botanical nature, yet here it was detected that "there was a something," as Dr. Pollock put it in his Croonian Lectures, "as yet unexplained;" and notwithstanding that the discovery of the bacillus was soon recognized, and the inference attached to it was unquestioned, the shrewdness, or "insular stolidity," of the British schools stepped in to counterpoise the "logical sequences" of the theory which was the inevitable outcome of the discovery (when coupled with the inference). The "something unexplained," which, I take it, is quite explicable by reversing Koch's inference, was no insurmountable obstacle

to some schools; but in this country it intervened so effectively that nothing has been done, and nothing has been formally proposed to be done even, on the lines of Continental hygiene, as regards phthisis or tuberculosis. Therefore, so long as the antagonistic view to Koch's shall remain undetermined by actual experiment, or by certain inference, it is not to be supposed that the faculty will take any formal notice of it. Common sense alone forbids.

But in the mean time, whilst the question is ripening, some practising medical men may see fit to apply the principle of sun-prevention, tentatively, to "consumptive families" of the wealthier classes, if only for the reason that in any case it can do no harm to flood a bedroom with sunlight; and when consumptives cannot go to Davos, or take a long sea-voyage, a physician here and there may be disposed to advise a change from a sunless bedroom to a chamber admitting a full supply of chemical light—a change which, if I do not greatly err, is equivalent to sending a patient to sea or to the Alps. Even though the happiest results from such changes as are suggested will be but negative proofs of the action of sunlight, and though instances of actual prevention may be open to the suspicion that they are merely coincidental with the changes, yet repeated instances of the arrest, and of the non-occurrence, of phthisis, observed in the practice of family physicians of the wealthier classes, will in time amount to cumulative evidence of very high value. Conjoined with the other facts which may be elicited, with the experiments which may be made, and with the arguments which may be struck out in the discussion on this subject during the next two or three years, such evidence, though negative, will give immense weight to the view of the bacillarial origin of phthisis, and to the proposition that the disease is to be suppressed at will—to say nothing of the numbers of persons whose lives may

be saved, or whose freedom from phthisis may be assured, by submitting them to conditions which are at once health-giving and agreeable.

All this, of course, depends upon the soundness or otherwise of the principal conclusion in this argument. If that is right—if the bacillus is an accidental parasite—it follows, almost to an inferential certainty, that it is to be destroyed and prohibited by chemical light. Therefore, pending the settlement of the botanical question, it is to be hoped on every account—from the highest scientific reason down to the low intellectual ground that "it's best to be on the safe side"—that many of the unfortunates who are now believed to be doomed through hereditary predisposition, or who are in the earlier stages of "acquired" phthisis, and whose means and circumstances will admit of it, may be submitted to the most thorough processes of what, for brevity, is here called sun-prevention. It will be observed that it is pre-supposed that the consumptive, and they who are sickening with consumption, are, or have recently been, exposed to a bacillary malaria fostered by an insufficiency of solar radiation; and this is one of the inferences which urgently requires to be verified. From the data furnished, it is to be inferred that in every instance of established phthisis, as well as in every case in which premonitory signs of the malady are present, it will be found on careful investigation that continuous or frequent exposure for some weeks to such a malaria has preceded infection. (I conceive, indeed, that this exposure will be brought out with such unfailing certainty, that absolute proof of non-exposure may hereafter come to be a means of nice diagnosis in doubtful cases.) It will be seen further on that this test of a sufficiency or an insufficiency of chemical light is a delicate one, and might fail occasionally in the hands of those who should apply it carelessly or loosely. There is the preliminary difficulty of

determining with precision the proper size for the windows of dormitories of given shape and dimensions in certain places; but let it be assumed that inspection or description of a particular bedroom satisfies the physician that the window or windows are large enough and so placed as to admit abundance of chemical light. If he conclude from this fact that sufficient chemical light is actually admitted daily into the room, he may easily be deceived. He has still to look to any permanent obstructions there may be to the passage of the direct rays of the sun, and to the customs observed as regards the admission of these rays into the room. Some housewives, for instance, and most housemaids, regularly exclude them by blinds, curtains, or shutters during the period of their highest chemical intensity; so that although a window may be far larger than is really required, still the requisite amount of chemical light may not find entrance into the room. All these points have to be considered before it can be concluded whether or not a person has been exposed to a bacillary malaria, engendered and maintained in consequence of a deficiency of proper solar radiation in a bedroom. In a large proportion of cases amongst the wealthier orders there will be no occasion to go into these details, for the insufficiency of solar radiation in the bedrooms of consumptives, or of those in the commencing stages of consumption, will be seen at a glance; and in the other more doubtful cases in the same class of habitations, if due regard be had to the environments of the patients, I submit, from the facts before us, that every instance of phthisical infection may be connected to a certainty with deficient chemical light.

If only a few physicians whose practice lies amongst the upper and middle classes, especially in the country, will be at the pains to investigate this matter searchingly, and to

publish the results, they may confirm or upset the inference that the bacillary malaria in the bedroom is controlled by the sun. If it should transpire that clear, well-authenticated cases of ordinary phthisis have occurred where the patients have not breathed at night the air of a bedroom insufficiently supplied with chemical light, the inference is unsound. If, on the contrary, it shall come out plainly, as I foresee it will, that breathing an atmosphere of this description is an invariable condition precedent to the occurrence of phthisis, it will go a long way towards establishing that it is a necessary condition precedent. I trust, therefore, that inquiry into this matter will be made; and, further, that when the inquirer has satisfied himself of the fact that the infected members of "consumptive families" have occupied, or are occupying, the bedrooms in the house which admit an insufficiency of chemical light, he will bring about the requisite change in their behalf whenever practicable. And this reminds me that he will probably meet with unexpected difficulties in many shapes. The room is large, airy, well ventilated, and with a good fire; it cannot be unwholesome. The windows are thrown wide open daily; but to keep the blinds up from early morning would destroy furniture, prevent sleep, and so forth. The worst of it is that a short and complete answer to all these objections is not yet ready. And as the means proposed are so simple and apparently so disproportionate to the end proposed, and as the reasons for employing them are so complex, it is almost hopeless to expect, in the present state of our knowledge of the effects of light, to convince English parents that any of the rooms in their houses have been and are the sepulchres of their children, or that the premature death of the occupant of any one of such rooms may be averted to a certainty by putting him or her into another room in the same house, similar in all respects to the pro-

scribed room, except aspect. Such propositions are too startling to the unprepared mind. It is easy enough nowadays to show that a son or daughter should go to the Riviera, or to Davos, or should undertake a long voyage, or travel in the Andes; but the notion that the invalid would derive as much, or more certain, benefit by putting another window in his bedroom, or by enlarging the existing window, or by insisting on the free admission of light through it, or by removal to another room, even from a commodious and spacious chamber to a small and inconvenient but well-lighted attic or garret, as by going abroad, would probably appear absurd or preposterous at first blush, even to cultivated Englishmen.

Everything, however, must have a beginning, and if the views enunciated here are sound, the sooner we begin the higher culture which shall include the art of living wholesomely, or, at all events, without contracting consumption, in our own country, the better. The trusted family physician who shall assure himself that there is solid stuff in this contention, will see that the task of enlightening parents in the matter of prevention devolves to a large extent upon him. It will come within his province, and it will be within his power, to indoctrinate the principles of sun-prevention. Then, if the results answer to the promises; if it be shown in process of time that numbers of persons in easy circumstances have apparently been saved from untimely death by means denied to poorer people; if, while this experience is being gained, the evidence collected from all other sources go to establish that the bacillus is an accidental parasite; if it come to be a forced conclusion that the persons who have recovered from consumption, and from incipient consumption, after the changes here suggested in respect of their bedrooms, have recovered as a consequence of such changes; and if the proposition that

all may be saved from consumption by the like or by similar means, or by other means to the same end, shall be clearly apprehended; the wide sympathy of the British nation will be moved. Once let this proposition be fairly grasped by the laity as well as the faculty, and there would go up one universal cry for the suppression of the disease. At whatever national cost—and it may be great—such organic changes as are seen to be necessary for the safe housing of the labouring classes spread throughout the country districts, and of the masses of people of all classes crowded together in the towns and cities, will be pressed upon the Legislature with resistless force. Much excellent work has been done for the poor in the way of sanitation, but the work contemplated here aims at poor and rich—or, at all events, at the comparatively wealthy classes—men with large incomes earned in cities, merchants, tradesmen, manufacturers, etc., etc. I conceive that if the profession were in a position to lay down authoritatively that consumption has been prevented in some grades, and may be suppressed altogether in every grade, of society, by the expenditure of an adequate sum of money, such effective measures would be taken that in one or two decades this fell disease would be utterly stamped out of these islands, and tuberculosis would be almost unknown. Therefore I have ventured to anticipate the verdict of micro-pathologists, botanists, and etiologists on the issue raised as to the pure or accidental parasitism of the bacillus, and to discuss questions of prevention on the basis of the assumption of a local bacillary malaria.

There is yet another reason for entering upon the question of prevention before the final decision regarding the bacillus shall be given. New ground will have to be broken, and collateral evidence given in support of the view that a local bacillary malaria is the sole cause of

phthisis. Indeed, the problem, when reduced to the form it may be made to assume, is seen to be neither so complex nor so difficult that one of fair capacity may not solve it; and I do not feel under the necessity of awaiting the sanction of authority before indicating the consequences of the views arrived at. All things considered, therefore, I shall venture to touch upon a few of the leading principles of sun-prevention, and leave details for future consideration.

Obviously the first thing to be dealt with here is the question as to the amount of chemical light required to preclude a bacillary malaria in a room of a given size, shape, and aspect. The subject of lighting apartments of all kinds for various purposes has been looked at from many points by physicians, architects, and others, but it has yet to be examined from the standpoint of sun-prevention. A deal of useful knowledge has been acquired, but some years must elapse before the application, for specific sanitary purposes, of chemical light to dwellings in all parts of the world can be placed upon a thoroughly scientific footing. Even in our own country, a deal of work remains to be done before we can emerge from the present state of nescience in all that concerns the distribution of solar energy. We have, it is true, some rough but serviceable data, which will enable us to effect highly useful and important changes in a large proportion of our domiciles as soon as the crying necessity for sun-prevention shall be recognized; but very nice calculations will be required to determine the precise amount of chemical light that will suffice for dormitories in certain positions in crowded cities and in large country houses, as well as in the dwellings of the inhabitants of all latitudes. It will be found impossible for a long time to fix a safe minimum of chemical light for dormitories; and standards for the purposes of legal definition will be out of the question until physicists

shall have made substantial additions to the small store of knowledge of solar chemistry. These matters, however, will resolve themselves; and possibly by the time the etiologist and the sanitarian are in a position to put into a concrete form all their requirements for a system of sun-prevention, the physicist may have made such advances in actinometry as to supply them with all the information necessary for their purposes. In the mean time, let us see, as well as we can, where we are. And I have to confess that I have acquired but a smattering knowledge of the subject through being forced to look into it by the exigencies of the conception as to the action of solar light on the bacillus-vegetation in bedrooms. It goes, therefore, without saying that I may display lamentable ignorance in the use of terms, but this will not matter if I can but convey my meaning.

The chemical intensity of total daylight from sunrise to sunset on any given day, or during any given hour before or after noon of the same day, in all habitable latitudes, is unknown. The whole chemical intensity of total daylight of any one day, at any one place in England, has not, I believe, been ascertained. Physicists have not been able, therefore, to lay down iso-chemical, or iso-actinic, lines on the principle of iso-thermal lines. What is called the mean chemical intensity of total daylight for certain periods at a few places has been taken: but as this mean was calculated from results obtained by the exposure of a chemical compound from one second to sixty seconds every hour or half-hour of daylight; and as the accuracy of these results depends to some extent upon the eye which daily compared the tints produced on the sensitive paper with the units or normal tints; and as the elimination for absorption by dust, smoke, cloud, vapour, etc., during the periods of the hours or half-hours intervening between

the moments of special exposure, must be more or less defective, by reason of its being based on assumption or approximation; it is evident that the recorded mean at any one place is, as a matter of fact, an arbitrary mean, though it may have answered the purposes of the physicist. The principle of most self-recording actinometers, indeed, precludes the possibility of obtaining a mean; so that it may be assumed that a mean is not a desideratum with physicists. It is, however, or will be, of vital importance to the hygienist to know what the mean chemical action of total daylight is at particular places during all hours of every day in the year.

From the description of the actinometer invented by Professor Sir H. E. Roscoe (*Proceedings of the Royal Society*, 1874), it is seen that the insolation apparatus, stocked with chloride of silver paper of constant sensitiveness, is worked by means of an electric communication with a properly arranged clock, so that discs of the sensitive paper "are exposed each hour for successive known intervals of time varying from two to thirty seconds." Ingenious arrangements are made to adapt the instrument "to the great variations which occur in the chemical intensity of total daylight in different places, at different times of the day, and in different periods of the year." Sir H. E. Roscoe observes that "in order to be able to estimate the chemical intensity, the colouration acquired by the paper must reach but not much exceed a given tint. . . . On unrolling at the end of the day the strip of sensitive paper which has served for the exposures, black discs showing where the paper has been stationary for the hours are seen, and between each of these are found ten circles variously tinted, from that, probably, scarcely visible, which was exposed for two seconds, to that, perhaps too dark to read off, which was insolated for thirty seconds. Amongst

these, some one at least will be found of such a shade as to enable it to be read off by the monochromatic soda-flame, on a graduated fixed strip." Comparisons were made at Manchester "with the hand-insolator and with the self-recording instrument," and there was found a "close correspondence of both sets of observations. . . . The integrals of total chemistry . . . exhibits as close an agreement as, from the nature of the experiments, can be expected."

It is evident that all actinometers designed to record the chemical intensity of total daylight for a few seconds, every hour or half-hour, brings us no nearer to a mean than would an intermittent register of the rainfall of a place assist us to the mean of its rainfall. The ten measurable periods of exposure Sir H. E. Roscoe's apparatus provides for, leaves the chemistry of the remainder of the hour a matter of uncertainty. We require to know not only the chemical action of daylight as it is expressed in these ten discs, but also the amount of decomposition represented by the "black discs showing where the paper has been stationary." We shall not have a mean for any place unless and until the results shall be obtained of the continuous decomposition there of a chemical compound (or perhaps of several chemical compounds) by total daylight from sunrise to sunset for a series of years.

As we do not know the mean, even for a year, of the terrestrial chemical energy of total daylight at any of the large towns in this kingdom, or of the comparatively clear country places in their vicinity, we cannot make the elimination for absorption by smoke, dust, etc., in these towns. The late Dr. R. Angus Smith, in a paper on this subject (*Proceedings of the Royal Society*, 1880), foresaw the difficulties here, and foreshadowed the future lines of investigation. He said: "When examining the air of towns and the effect of smoke and fogs, I have often wished for a very

simple method of measuring the total light absorbed by these gases and floating solids;" and then detailed some ingenious experiments made with this object with iodide of potassium. But whatever agents may be found suitable for the proposed measurement, it is palpable that, as the absorption of light in each town will vary hourly, daily, and monthly, correspondingly with the seasons, continuous work is required, perhaps for years, to get at definite, tangible, useful results. It is the amount of absorption that is so essential to the hygienist to ascertain. Physicists, on the contrary, regard the intervention of floating solids rather as an interruption to their observations. Thus Mr. J. B. N. Hennessy, in a communication on Actinometrical Observations made in India at Mussoree (*Proceedings of the Royal Society*, 1882), observes: "What can be the use of measuring solar radiation through a visible varying atmospheric umbrella, such as is represented by cloud, mist, haze, dust, and smoke?" Again, he says: "It is astonishing how considerably one's notions of the number of days suitable for actinometrical work undergoes modification after careful watching. I speak, of course, only of *visible* vitiating causes; of the invisible I know nought, if ought be known. . . . At the best, a number of days will occur annually when observations if made are worse than useless, for they are misleading. Hence in any project for continuous actinometrical work the stations should be in pairs, so that the omissions at one of the two may be supplemented at the other." The suggestion is admirable, but what precedes would seem to indicate that the physicist was so intent on some special object, that he overlooked the possibility that it might be of extreme importance to mankind, not only to find the amount of chemical intensity of unimpeded solar radiation at a place, but to measure the sum of the energy just as it comes to the earth—with or without the umbrella.

Yet Mr. Hennessy, having made his protest, nevertheless did measure the effect of the radiation through the umbrella of a "dry (dust ?) haze," and found that the mean results of five clear and three hazy days showed "about five and a half per cent." of absorption by the haze; whereupon he says pertinently, and from our point of view opportunely: "If this may happen at a height of 6700 feet, and the haze was by no means of maximum density, what may be expected at lower or less favoured localities?" No better illustration could be given of the necessity for getting at the co-efficient for absorption by the umbrella which nature interposes, as well as the co-efficient for the umbrella which man thrusts between earth and sun. For what prospect is there, otherwise, of counteracting with precision the injurious effects caused in large towns through their being deprived of their normal amount of solar action by reason of the foreign matter suspended over them? The most that can rationally be expected is that some good may be had from decent approximation—from some interim calculations that must, perforce, suffice until we get scientific data to work upon. To give some notion of the complexity of the questions involved here, and to show plainly whither we are tending, let it be assumed that light precludes a certain noxious vegetation in dormitories. We will then suppose that the light admitted freely by a window of a given size and aspect into a bedroom of a given size and shape in a house standing by itself five miles away from a manufacturing town (having an atmosphere of a known absorptive power), is sufficient to preclude the vegetation all the year round; and we require to know the difference that would have to be made in the size (specifying width and length and height) of the window of a bedroom of the same size and shape and aspect in the manufacturing town, to compensate for the (given) position of the house in the town,

and for the absorption of light by smoke, etc., in order to equalize, if it be possible, the two rooms in respect of the vegetation. By varying the terms of the problem—by altering the aspect of the window of the house in the town, or the position of the house relatively to other houses, by shifting the town a hundred miles north or south, by changing the amount of absorptive matter in the atmosphere of the town, etc.—the calculations become more nice and intricate. Yet they will have to be gone into some day, and it may not be many years before they come before the Law Courts.

Preventive Medicine does not appear to be in a fair way of getting substantial aid from any extant photometric process. It seems to me, if I might hint as much, that the discontinuous principle that has obtained in actinometrical apparatus is singularly open to the "personal error" objection, and that the results hitherto, few as they are, are not altogether trustworthy. In fact, nothing short of a continuous decomposition of some agent which should give a ponderable result by which the whole of the chemical action of total daylight could be measured—provision being made for separate hourly results, or half-hourly, if need be—would answer the purpose thoroughly and completely. Some such photometric plan for measuring the intensity of the ultra-violet rays as that of M. Eden, by a mixture of oxalate of ammonia and corrosive sublimate in water, from which mixture a black precipitate is obtained from the action of light, the weight of the precipitate depending on the intensity of the light—some such process as this, if it will admit of adaptation to continuous work by means of automatic arrangements, would serve. In any case it would appear unlikely that we shall learn the actual amount of absorption at a place by the use of the actinometrical processes now in vogue.

What, however, is of still greater importance than the determination of the quantity of the absorptive matter, or of the precise effect of the absorptive conditions, in the air, is that we know literally nothing of an exact nature as to the relative chemical intensities of the solar beams at places where the atmosphere is not artificially charged by man with solids. We have no exact knowledge of the solar chemistry of any of the comparatively clear parts even of Great Britain. It seems that physicists are not yet in a position to tell us the difference between the chemical effects of total daylight for the same hours before or after noon of the same cloudless day in any place in the south of England and in the north of Scotland; or the difference, if any, in the sum of the chemical effects for the whole of the same cloudless day at both places. It is not known whether the sum of the chemical energy of light distributed throughout the year is, or is not, the same at all places in both hemispheres; or if it is, or is not, the same in high as in low latitudes, what the law of its distribution is for the shorter or longer periods of daylight in each latitude in the same hemisphere on the same day; or whether the intensity increases or decreases equably (other things being equal) before and after noon of the same day through all the hours of daylight, or whether the increment or decrement is greater or less for some of the corresponding hours before and after noon of the same day; or whether more of the chemically active rays impinge upon one portion of the earth's surface than upon another throughout the whole year—that is, whether one place receives a larger amount of chemical energy for the year than another place, or whether the increase at one season at a place is compensated for by a decrease at another season, so that, taking one season or one year with another, the chemical action of light is equalized all over the globe; or, if there is inequality in this

respect, what portions of the earth receive the most and what the least chemical action, and how the distribution takes place, and at which periods of the year the greatest chemical activity prevails.

Some of the questions here raised were answered provisionally many years ago, and no doubt somewhat hastily, on the strength of inferences from certain phenomena observed in photography; but these inferences have been disturbed, and the observations from which they were reached are considered, apparently, to have been hallucinations which have been disposed of by the experiments made by Professor Thorpe. And, as physicists have not educed any general laws of the distribution, terrestrially, of chemical light, the questions must wait for full and final answers. In the mean time, it may be asked whether it is held to be safe to accept Professor Thorpe's experiments before they shall have been confirmed by at least one or two competent observers—the more especially seeing the startling results he obtained. These experiments were made at Pará, lat. 1° 28′ south, during the month of April, 1866, and they are thus referred to by Sir H. E. Roscoe in the *Proceedings of the Royal Society*, 1868: "According to their" (photographers') "observations, it appears that the difficulty of obtaining a good photograph increases as we approach the equator; and more time is said to be needed to produce the same effect upon a sensitive plate under the full blaze of a tropical sun than is required in the gloomier atmosphere of London. Hence the existence of a peculiar retarding influence has been suggested which the heating and luminous rays are supposed to exert upon the more refrangible portions of the spectrum. The fallacy of these statements has been fully proved by a series of direct measurements of the chemical intensity of sunlight under the equator, made at Pará by Mr. T. E. Thorpe. The curves

of daily chemical intensity given in the paper show that the activity of the chemical rays in the tropics is very much greater—on one day fifty-five times as great as in our latitudes; and these measurements prove that the reported failures of photographers cannot, at any rate, be ascribed to a diminution in the chemical intensity of sunlight." We learn, further, that the measurements at Pará were made "in the middle of the rainy season, and at very frequent intervals during the day; the curves show the enormous and rapid variation in intensity from hour to hour which the chemically active rays undergo under a tropical sun during the rainy season." In a subsequent paper by Professor Sir H. E. Roscoe and Professor Thorpe * occurs the following:—"Curves are given showing the daily march of chemical intensity at Lisbon in August, compared with that at Kew for the preceding August, and at Pará for the preceding April. The value of the mean chemical intensity at Kew is represented by the number 94·5, that at Lisbon by 110, and that at Pará by 313·3, light of the intensity 1 acting for twenty-four hours being taken as 1000."

Passing over the amusing superciliousness of the physicist to the artist in these quotations, it will be observed that the Pará results, arrived at in one month of the rainy season by one observer, are taken to have shown conclusively that the concurrent testimony of the large body of photographers spread over the world was altogether wrong, and must be set aside as valueless. When, however, we come to reflect that keen observers and good chemists were in the ranks of the photographers, and that some of these had verified the fact that the chemical intensity of light lessened for a given period on the same day, other

* "On the Relation between the Sun's Altitude and the Chemical Intensity of Total Daylight in a Cloudless Sky," *Proceedings of the Royal Society*, 1870.

things being equal, from higher to lower latitudes—Dr. Draper, for instance, always found a marked difference between New York and Virginia—are we to trust implicitly to the measurements of one observer whose results are so opposed to the practical experience of a whole body of men? Should we not rather incline to the view that some error may possibly have crept into and vitiated Professor Thorpe's experiments—something wrong with the sensitive paper, or with the "unit," or in the reading off the colouration-tints, or with the length of exposure, or with something else? It is at least as rational to suppose that a physicist, however eminent, or his assistants, may have committed some error of omission or commission, where so much nice work is involved, as to suppose that some hundreds of photographers have all made the same mistake in all quarters of the globe about one particular thing, occurring repeatedly in their ordinary work, and that their combined observations are all delusional. But if the possibility of error on the part of the physicist be eliminated as an impossibility, so far as the measurements are concerned, and if we thus put the photographers out of court, the question then arises whether the interpretation of the phenomena observed by Professor Thorpe is the correct one, and whether all the inferences drawn from them are sustained by the data.

In the first place, the so-called mean of chemical activity at Pará is open to the "personal error" objections heretofore taken to discontinuous decomposition. I conceive that a mean arrived at by the eye and by assumption (for intermediate periods between the measurable moments of exposure) is an arbitrary mean. Letting this go, the next reflection that occurs is that it is not apparent why the observations at Pará were made during the rainy season. The results appear, no doubt, to accentuate the "fallacy" of the statements of photographers about the "full blaze of

a tropical sun," but then it does not appear to have suggested itself that the cases are not parallel as between the sun-pictures taken by Professor Thorpe and those taken by the photographers. As may readily be supposed, artists did not select the rainy season of a country for taking views of scenery, etc. Therefore, who shall say with certainty that, assuming the observations at Pará to have been correct in every particular, the statements of the photographers must of necessity be wrong? Who shall say that after the rainy season at Pará the rays of the tropical sun there may not become to a large extent chemically inactive from atmospheric opalescence, or from some intangible, invisible, unknown cause of absorption, or, to borrow Mr. Hennessy's figure, from some unseen natural umbrella, and may remain so until the rains return and wash the skies clear again? Or is it to be supposed that the month's experiments at Pará exhausted actinometrical observation there and elsewhere, and left nothing to be learnt of the phenomena of light in tropical and subtropical regions? Upon this point it needs only to be observed that Mr. Hennessy's results at Mussoree, obtained since those at Pará (*Proceedings of the Royal Society*, 1882) are found to be opposed to the latter. Mr. Hennessy found that the summer results, though there were great and violent changes, were less than the autumn results at Mussoree. In short, it is palpable that the few observations at Pará are insufficient to generalize from, and that the conclusions drawn from them were not warranted by the nature of the facts elicited.

Wherever we turn in this branch of actinometry we find nothing but incertitude. Not one law has been educed from all the observations that have hitherto been made, relative to the greater or less chemical intensity of light at different times and places. Sir H. E. Roscoe's investigations

appear to show that the daily maximum of chemical intensity corresponds to the maximum of the sun's altitude; but this can hardly be held to be established as a fact at present, with regard to all periods before and after noon, and if it were established it would merely be a fact. Although it may be true that the chemical intensity of total daylight is equal, or nearly so, at corresponding periods before and after noon of the same day and at the same place for the two hours immediately before and immediately after noon, yet there are many strong reasons for concluding that the chemical action is not the same for the corresponding periods before ten and after two o'clock in the day. For, to say nothing of the wide experience of photographers before the present more sensitive processes came into use—an experience which, however rudely gained, is not wisely or safely to be ignored or disregarded by the physicist—there are the rough tests of the bodily sensations caused respectively by the morning and evening sun, and the well-known greater bleaching properties of early than of late sunlight on the same day. This property of sunlight is as familiar to the chemist and to those employed in the arts as it is to the washerwoman and the housewife. It is matter of common observation, especially in tropical and sub-tropical regions, that the air is far more oppressive at three or four o'clock in the afternoon than at eight or nine in the morning of the same day, on days when the thermometer indicates the same temperature at both periods. In siroccos or hot-wind days in Egypt, Australia, etc., it is found that, thermometrical conditions being the same, the morning air is sometimes comparatively fresh and invigorating, whilst that of the afternoon is invariably like the blast of a furnace, without any redeeming quality. This marked difference, *longo intervallo*, is to be perceived on some summer and autumn days in England, and, as a general rule, it may be

said that whatever the temperature of a place it is rarely unbearably hot up to noon on days when the heat becomes almost stifling in the afternoon and evening. Certainly it has not been shown that the difference is due to the chemical constituents of the solar beam, but it has not been shown to be connected with, or rather to be dependent upon, moisture, or on ozonic, telluric, electric, or other conditions; and as the photographer and the bleacher have found that the power of the sun diminishes rapidly, so far as their processes are concerned, after two o'clock, more rapidly than the converse increase of power in the corresponding periods before ten in the morning; and as it is known that the work of the photographer and the bleacher depends on chemical intensity,—it is an inference that the difference in the sensations of man from exposure to the morning and afternoon or evening temperature is to be attributed to the chemical effects of light. The fresh exhilarating feeling from the morning, as compared with the evening, sun is to be easily understood, if the tanning of the skin be regarded as a photo-chemical effect by which depurative changes are brought about in the blood. If, as is supposed, the decomposition of some of the constituents of the blood and the removal of such elements as are in excess of the normal amount, and their deposition in the skin, are the results of exposure to chemical light, the pleasure of being in the morning sun is to some extent explained. The lightness and elasticity come of being relieved of surplus or waste material. But the afternoon or evening sun, being weak in chemical activity, is incapable of inducing these changes to the same extent; hence heat is the predominating sensation. It may be that the sun's chemical rays may be intercepted to a larger extent, and that there may be more absorption from unknown atmospheric conditions obtaining during the afternoon; but, whatever the precise explanation of the

phenomenon, it is abundantly clear that the terrestrial chemical intensity of total daylight is not the same at all corresponding periods before and after noon of the same day.

If the sensitive papers used in actinometrical observations invariably give uniform results for hours equidistant from the noon of a given day, then there is something as yet undiscovered—something that leaves the marked difference between the chemical effects of morning and evening sun unexplained. Perhaps the compounds employed to test the chemical intensity of the ultra-violet rays may not be acted on by all of these rays, and consequently their potency in nature is unrecorded by the actinometer. The ordinary sensitive paper may not detect, and therefore cannot register, the relative morning and evening variations in the sun's chemistry. Be this as it may, it is palpable that finality in this matter has not been arrived at, and that something more has to be brought out in relation to the daily changes in the intensity of solar radiation. For the failure of the physicist, with his present means, to determine the nature, extent, and causes of these changes cannot be accepted as evidence that there is no difference in the chemical power of the sun at periods equi-distant from noon. The difference is too pronounced, even in this country, to admit of its being glossed over as imaginary; and in lower latitudes it is so glaring that there is no possibility of explaining it away as a popular delusion.

If our knowledge of the daily variations of chemical intensity in our own country is meagre, it is eminently unsatisfactory as regards the distribution of solar force in other countries. The limited observations of the few observers who have entered this field are utterly insufficient for the purposes of generalization. One of these few, Mr. Hennessy, with a frankness that does him infinite honour,

confesses that he knows "nought, if ought be known." Another, Professor Thorpe, has drawn weighty conclusions from slender premisses; and until substantial confirmatory evidence of the correctness of his work at Pará be forthcoming in the first place, and in the next place some grounds for his interpretation, or rather, perhaps, Sir H. E. Roscoe's interpretation, of the phenomena he observed, the work, I conceive, will be of small value. As it stands, I shall hazard the opinion that it by no means suffices to upset the observations and experience of the host of earlier photographers. Making every allowance, though there is no occasion for it in a number of instances, for the unscientific modes in which they may have worked, there is no getting over the plain fact that they one and all found, or said they found, that their salts of silver required a longer exposure to produce a given effect in the lower than in the higher latitudes, and that the lower the latitude the longer the exposure required; and, moreover, that in all latitudes they found they could take a photograph between 9 and 10 o'clock a.m. in less time than between 2 and 3 p.m. of the same day. Whether the law which was formulated on the strength of the photographer's work—viz. that the chemical intensity of light decreases in a certain ratio, for equal proportions of time, in each latitude nearer to the equator—is correct or not, or whether there is any approximation to a natural law in the formula, are distinct and separate matters which remain to be seen into; but the question as to what photographers found, or said they found, is a question which turns on the nature and weight of the evidence.

From all the facts known or produced, from the facts relating to the luxuriant growth of certain low vegetable forms in slight shade in low latitudes—forms which require very dull diffused light to enable them to subsist in high latitudes—and from many phenomena connected with the

incidence of phthisis and with the relative virulence of the disease in tropical, sub-tropical, temperate, and cold regions, I conceive it to be highly probable that the sun's chemical power varies in high and low latitudes, somewhat after the manner described in the law laid down by some physicists before the Pará experiments. An immense amount of work has to be done, however, before the subject can be lifted out of the elementary stage in which it now rests, and it is obvious that physicists are on the threshold merely of this important study. For as yet not one law or general principle has been educed from this branch of actinometry and established upon a solid basis. We are in complete ignorance of the delivery, terrestrially, of the solar chemical rays in different latitudes; and what is deplorable is that, with the limited number of physicists engaged in actinometrical work, it will take many years to collect sufficient data for sound generalization. In the mean time, all the questions as to the principles of the distribution of the chemical force of light in every part of the globe must be abandoned to the speculative inquirer; and all applications of this force for sanitary purposes must be more or less uncertain in their results.

This being the position of the subject, and on the understanding that my views are based upon data which, among the conflicting data before us, would appear to be most in consonance with certain phenomena and the inferences from them, I shall submit my conclusions touching the chemical energy of light in different countries. They embody the pith of what I apprehend to be the relation between chemical light and the phenomena of phthisis in different latitudes. As it is premised that I have selected the data upon which the conclusions are founded, and as the correctness of some of the data has

been challenged by eminent physicists, it will be understood that these conclusions are provisional merely. In plain language, the views are speculative, and are to be regarded accordingly.

With regard to the question whether the altitude of the sun determines the degree of the chemical intensity of total daylight, it has already been suggested that, whatever may be the relation between the sun's altitude and terrestrial actinism, there are what appear to be cogent reasons for inferring that the photo-chemical rays are more energetic from sunrise to noon than from noon to sunset—excepting, perhaps, during the four hours of midday. Whilst there may be equal potency, or nearly so, in the sun for the two hours before and the two hours after noon, the earlier and later hours of daylight do not correspond in like manner, but there is an appreciable difference between the chemical effects produced during the other hours that are equidistant from noon. As the altitude of the sun is practically the same at these corresponding hours—the daily declination being insignificant—it is an inference that, whatever the effect of altitude, it alone does not govern the sun's chemical effect on the earth. We need not, however, cast about for the precise explanation of the phenomenon of the greater intensity of the morning than of the evening rays. It is sufficient to observe that, though there are no actinometrical measurements in support of it, we may infer from the other data mentioned that it is a law that, from some cause, the morning sun has, in effect, more chemical power than the afternoon sun at hours equally distant from noon of the same day at the same place.

The point is most material in connection with the prevention of consumption, and its practical bearing will be seen when an east or a west window for a bedroom

or other apartment is in question. In such a case it becomes, perhaps, a matter of vital concern. If the physicist is right, an east or a west window, all other things being equal, will admit the same quantity of light of exactly the same chemical intensity into a room; and it is, therefore, unimportant which of the two aspects are chosen for a dormitory. If I am right, an eastern or a western aspect may be matter of life and death; for though the east window, all other things being equal, will admit the same quantity of light as the west window, yet it will be light of greater chemical intensity, and might just turn the scale as regards permitting or prohibiting the growth of the bacillus-vegetation in a room. An east window might preclude the vegetation where a west window of equal size, etc., owing to the comparative feebleness of the chemical light admitted through it, could not prevent it from establishing itself. Into all calculations, therefore, touching the aspect of windows, this element of morning and afternoon sun must enter—assuming, of course, there is, as I suppose, the difference in the chemical intensity of light referred to.

A still more important point in connection with the lighting of dormitories is the question of latitude; and our nescience here is deplorable. It has been seen that a mean of chemical intensity has not been properly obtained for any one place, and cannot be had until the whole sum of the chemical action of the sun at a place has been legitimately measured for a series of years. Experimental data are so few and so imperfect that but little reliance can be placed upon them. It is useless, therefore, to attempt to get at any leading principles from them as to the chemical intensities for the same periods of time, at given distances from noon of the same days, in the different latitudes in either hemisphere. From numerous

phenomena, however, unconnected with the experimental data supplied by the actinometer, or with the work of the photographer (although this work is in perfect accord with the phenomena), we may infer that there is a steady decrease in the chemical action of light for a given period of time in each degree of latitude nearer the equator. I conceive, indeed, that it is not improbable that an equal, or about an equal, amount of chemical action is induced during the whole year at all parts of the earth where the transmission of solar light is not interfered with by man. That is to say, I suppose that the yearly mean of the intensity of the rays delivered at Calcutta, for instance, may be about the same for a series of years as that of the rays which impinge upon any part of Lapland. The two regions may be equalized in this matter by the longer time required to produce a given effect in the lower than in the higher latitude; so that although there is a far larger proportion of sunshine at Calcutta, yet the greater chemical action of the sun and of total daylight in Lapland restores the balance. To bring the point nearer, I suppose that the yearly sum of the chemical work of the sun in a village in the north of England is the same as that at a village in the south of England—assuming them both to be out of the region of manufactures. What the village in the north loses in sunshine or daylight is compensated for by its greater chemical intensity for the time being.

This view that all parts of the globe are placed under nearly equalized conditions as regards chemical light may or may not hold water. Certainly it is not in harmony with the experimental data contributed hitherto by some physicists, but it squares with the observations of other physicists, and with the experience of all the earlier photographers. Possibly the conception, though crudely pre-

sented here, may embody in it a law of some importance in connection with nutrition and with the existence of animal and vegetable life. Confining ourselves to our special subject, however, it enables us to get a complete and intelligent survey of pulmonary consumption amongst all races, and furnishes us with a full explanation of the peculiarities in the incidence of phthisis throughout the world. If phthisis be regarded as a disease caused by a local bacillary malaria, the existence of which malaria depends on the amount of chemical light brought to bear on the locality in which it occurs, it may be seen at a glance how a small difference in latitude, other things being equal, may make a vast difference to the malaria. If we assume, for example, that two houses, similar in all respects, are placed one in the north, the other in the south, of England, and both with the same aspect, it is obvious that whilst a room in the house in the north shall be free from the bacillus-vegetation, the corresponding room in the fellow house in the south may be contaminated with a bacillary malaria. For, although the windows of the two rooms are of equal size, and both face the same way, the value of the chemical intensity of total daylight admitted into the room in the north will exceed that received into the room in the south. The difference in the intensities may be slight, but it may determine the existence or non-existence of the bacillus-vegetation. Hence it follows that to produce an equal chemical effect in each of these two rooms, the windows of the room in the south must be larger than those in the room in the north, or differently placed. The exact alteration in the dimensions, or the precise variation in the position of the windows, required to compensate for the difference in latitude and the consequent difference in the position of the locality to the sun, involve nice calculations which must be left. It has to be borne in mind, however,

that heat has to be taken into the account as well as chemical light. From the comparative rapidity of efficient infection, and from the greater virulence of phthisis, in warmer countries, it is an inference that the bacillus-vegetation grows more vigorously in them than in colder regions even when the conditions are equalized in respect to light. This may introduce hereafter an element of uncertainty into computations as to the amount of chemical light required to preclude the vegetation in a given low latitude; but the inhabitants of the countries most interested in the matter will soon be able to fix their own standards and to work out the problem for themselves, when once the relation of the sun to the bacillus shall be recognized. In Great Britain the range of latitude does not include regions in which the (assumed) increased luxuriance of the vegetation is so pronounced as in the southern parts of Europe, yet even in this country it has been observed that not only is there a larger proportionate number of cases of consumption in the southern and western than in the northern and eastern counties, but that they run their course more rapidly in the former. If this is partly due to the more favourable conditions for the vegetation as regards warmth and moisture, the calculations as to the size, position, and aspect of the windows required for the suppression of this vegetation in dormitories must provide for additional chemical light on this account, as well as for a larger proportion of chemical light for each degree of lower latitude.

This view of the effects of climate runs counter to the views of those physicians who send their patients to soft, balmy regions; but it helps to explain some of the unfortunate results. The object of sending them is clear enough, and a proportion of consumptives are no doubt vastly benefited, or restored to health, or to such a modified state of health as may be possible; and the explanation is obvious.

They have undoubtedly gained by the substitution of a warm, moist, calm atmosphere for keen, dry, cutting winds, but they have owed their recoveries entirely to the accident that they have escaped further infection from the bacillus-vegetation. Their good fortune has preserved them, after having been rescued from the local bacillary malaria in their own country, from being subjected to another and more rapidly fatal local bacillaria in a foreign country; and we may read the history of many once favourite and fashionable health resorts by this light.

The inhabitants of a district, an island, a village, or a town having always been singularly free from consumption, it was no doubt argued that the soft and balmy air of the locality did not favour the occurrence of the disease, and that therefore it must be an excellent place for those with incipient or even advanced phthisis. At first there were some happy results from this view, and soon there was an influx into the place of young consumptives of both sexes. A few years' experience, however, showed that to send even mildly affected patients to the place was to send most of them to early death. Some died who were but slightly tuberculous, and those more seriously involved went into galloping consumption at once. So the place came to get a bad name, and other rising places were resorted to, many of which have since gone through the phases of high reputation and final abandonment. The explanation of the rarity of phthisis among the natives of these places is that these primitive folk lived in rude habitations, under such conditions as regards the sun that they were seldom or never brought within the sphere of a local bacillary malaria—or not long enough to be efficiently infected. When the few consumptives who first resorted to the place arrived, they had of necessity to submit themselves to the same conditions as the residents, and, as a consequence, they whose lesions

were not grave got an excellent chance to recover. Taking in no more bacillus-vegetation in the air they breathed, the bacilli in their lungs died out or became encapsuled, and tuberculosis was stayed; so that small, and sometimes even large, cavities closed up, and the whilome consumptives became stout and returned home living instances of the salutary effects of the climate. But they who followed them in two or three years mostly went to their graves; for their numbers and wealth demanded that special provision should be made for them. The local residents, finding their profit in these sick visitors, laid themselves out for their reception; and thus it was that, whereas the pioneers had to content themselves with such quarters as they could find, the later arrivals had what was considered better accommodation. They got more comfortable or luxurious rooms, carefully protected against the glaring sun; and in the cool shade of these rooms the fatal bacillaria was soon engendered.

Climate undoubtedly influences the sum of phthisis, but, as I infer, in the opposite direction to that generally supposed. Analysis of the large groups of facts bearing on this question will bring out that, assuming other things to be equal, climatic conditions (and chemical light) determine the incidence of phthisis in different latitudes. But as, in point of fact, no two regions present similar conditions in regard of the number, occupation, dwellings, etc., of the population, it happens that a region which is not naturally so favourable to the occurrence of consumption as some other regions, may nevertheless have a larger proportionate death-rate from consumption than those regions, by reason of the artificial environment of its people. Thus England is naturally less favourable to the development of the bacillus-vegetation than the south of Europe, and yet, owing to the modes in which the population of England is housed, the mortality from phthisis in England is, or perhaps was, larger

relatively than that of Southern Europe. If it be supposed that London, with its four millions of inhabitants, were transported as it stands to the south of Europe, and that the aspects of the houses and all other conditions, save latitude, remained precisely as they are, the mortality from consumption in the transported city would be enormously increased. If, in like manner, we suppose London to be shifted as far north, the mortality would be considerably, though not correspondingly, diminished.

This important inference is to be reached by means of the assumption that the chemical action of light is less in a given period on the same day, and at the same time of the day, in a low than in a high latitude. As, therefore, on this assumption, a given amount of total clear daylight let into a room in England on a given day and hour will produce a greater chemical effect than the same amount of total clear daylight let into a precisely similar room on the same day and hour in Southern Europe, it follows that low forms of vegetation that require, as a condition of their being, that they shall be subject to no more than a certain amount of chemical light, may grow freely in rooms in low latitudes from which they would be altogether excluded in higher latitudes (the co-efficient for absorption by solids in the atmosphere being allowed for). If this view of the relation of latitude to consumption be sound, we have at once the explanation of the many conspicuous failures of changes of climate in the case of English consumptives. It has depended on the merest chance whether the removal of a patient to the warmer latitude has resulted in taking him out of the sphere of a bacillary malaria or not; and the chances on the whole have been decidedly against this result, and in the majority of cases the patients would have been far better, or would have lived longer, in their own country, and even in the contaminated family house.

Physicians have gone further afield than Europe for resorts for consumptives, but as the fundamental principle upon which the value of change of air depends, namely, removal to an atmosphere free from bacillus-vegetation, has not been apprehended, the results have not varied much from those obtained in the warmer climes nearer home. Setting aside the Americas, the Nile, the Cape, etc., we will just glance at Australia, a large but neglected field of etiology. This country is noted for its dry as well as warm climate. Its interior is sparsely populated by a pastoral people, but there are some inland towns—mining and agricultural centres—of large size here and there. The most populous colonies of the group are Victoria and New South Wales, each of the capitals of which now contains over three hundred thousand inhabitants. The favourable accounts of the climate having attracted attention, many invalids in various stages of consumption were sent thither, and a proportion of them recovered. It was very soon observed, however, that those who went into the bush, instead of remaining in the capitals or large towns, showed by far the greater proportion of cures. In process of time it was found that the native or resident youth and middle-aged and old persons, more especially in the populous places, fell into consumption, which generally ran a more rapid course than is usual in England, and the mortality from it in Melbourne and Sydney became extremely large. And many younger members of the families of the wealthier settlers in the country districts, and of the rich merchants and others in the suburbs of Melbourne and Sydney, contracted phthisis and died—some of them in the colony, and others in countries to which they had been sent for change of climate !

Here we have a series of instructive facts which are simply incomprehensible and utterly irreconcilable one with another without the aid of the assumption of a local

bacillary malaria, governed by chemical light; but with that aid they may be readily apprehended and brought into accord, not only with each other, but with every fact connected with consumption. The immunity of the pioneers of Australia from consumption is easily accounted for. They lived at first in the open air, or in tents, or slab huts roofed with bark. These conditions in so dry and sunny a climate of course precluded a local bacillary malaria in their sleeping-quarters. When these rougher stages of bush life passed, and the slab hut was replaced by the weather-board building with the wide verandah and the shingle roof, still the intense heat and dryness of the air kept the wooden structures free from moisture and prevented the growth of this vegetation. This being the case, neither the settlers nor the few consumptives who found their way as visitors to this class of habitations were subjected to the specific malaria. But as their incomes from sheep or cattle increased exceedingly, and their properties became enormously valuable, some of the colonists abandoned the old homesteads, built residences of stone or brick, and surrounded them with verandahs and balconies to shut out the sun on all sides. By these means the conditions of the interior of their dwellings were radically changed. Thick walls and constant shade soon permitted some of the errant spores from the bacillus-vegetation in the natural flora (from which their cattle became tuberculous) to establish themselves in their bedrooms, and the local bacillaria was engendered. The wealthy citizens of Melbourne and Sydney also had their solidly built mansions in the suburbs; and as shade and coolness in the bedrooms were desiderata, the like result followed. The conditions in these two cities themselves were not to all outward appearance so conducive to a bacillary malaria as those obtaining in London. There were neither so much want and overcrowding nor so great an absence of light. The build-

ings were not so high, many of them were of wood, and there were no underground sleeping-places. Yet the mortality from phthisis became excessive, and increased steadily year after year, and is still increasing.

The interpretation of all this lies on the surface for those who have assimilated what has been advanced in respect to chemical light. Although these towns are flooded with sunshine, compared with London, yet the chemical effects of light in Melbourne are, in a ratio with the latitude, weaker, and in Sydney still weaker, than in London, and the volumes of smoke and clouds of dust sent into the atmospheres of Melbourne and Sydney furnish a large amount of absorptive material. As a consequence, the bacillus-vegetation will grow in a bed-chamber in Sydney or Melbourne in which it could not get a footing (other things being equal except latitude) in London. Less shade being required for the vegetation in the lower latitudes, the practical result has been that, owing to the style and arrangements of the buildings in Melbourne and Sydney, the consumption-plant has invaded and established itself in a large proportion of the bedrooms. For instead of placing these so as to admit the morning and midday sun freely through large northern or north-eastern windows (which would have compensated for the less intensity by introducing a larger number of the chemical rays), they placed them, when they could, on the south or west side of the house, or where the morning or midday sun could not enter from any cause; and when this was impracticable, they kept the sun out by verandahs, blinds, shutters, and other devices. Hence in a few years more, unless sounder knowledge shall prevail, these cities threaten to have a very high death-rate from phthisis and tuberculosis. As their increase in population is rapid and involves higher structures and deeper shade, the outcome in phthisis alone is likely to become a serious plague.

In addition to the inferences reached with respect to the latitudinal and daily chemical power of the sun, there are the seasonal variations in its intensity to be taken into consideration. The epidemical exacerbations of phthisis furnish, as we have seen, one of the most cogent arguments in support of the view of accidental parasitism and local bacillary malaria, and against the theory of contagion. But the only point now is whether the increased growth of the bacillus-vegetation, which is indicated by the general recrudescence of phthisical symptoms in a district, is associated with, or dependent upon, any diminution of the chemical activity of daylight at the period of recrudescence. And after looking at the matter carefully, I fail to find any interdependence between the two things. Certainly the mean chemical action of total daylight for all the seasons has not been measured for any part of Great Britain, so far as I can ascertain; but, assuming that the observations hitherto made represent a fair approximation to a true mean, it does not appear that these periods of recrudescence occur at times when the chemical intensity of solar radiation is relatively weak. These periods would seem rather to correspond with warm or unusually moist conditions of the air, than to be connected with special variations in the chemical action of the sun—though, as prolonged cloudy weather undoubtedly causes a deal of absorption, the lessened chemical intensity of light may be one of the factors in the result. And it may be observed that at such periods when, as it is inferred, the bacillus-vegetation takes on an almost tropical luxuriance in its growth, it may happen that in a dimly lighted chamber, where the vegetation had not previously gained a footing, it may, under the exceptionally favouring conditions, establish itself.

The foregoing views upon chemical light will be found to be in accord with, and are indeed complementary to,

the views connected with the accidental parasitism of the bacillus, the incidence of phthisis, and all the phenomena surrounding the disease. No two views clash, and, as far as I am able to see, there is not one fact in the whole history of tuberculosis that is irreconcileable with my main propositions. On the contrary, every fact admits readily of an interpretation which is in perfect consonance with them. This general agreement, I submit, is strong presumptive proof of the soundness of the inference that the tubercle bacillus is not a pure parasite, but finds entrance to the lung in a local miasm caused by the growth of its parent-form in habitations where it is fostered by a deficiency of chemical action in daylight. And when regard is had to the enormous difficulties pointed out, to the incongruities, the anomalies, the inexplicable phenomena standing in the way of pure parasitism, it is almost an inferential certainty that the bacillus is an accidental parasite. If the inference is right, it is a sequence that the successful treatment of consumptives by insolation of their bedrooms owes its efficacy to a destructive influence on the bacillus-vegetation, and to the consequent suppression of the local bacillary malaria in the rooms. From this it is but a step to the utter preclusion of the bacillaria in all bedrooms, and to the consequent certain prevention of phthisis amongst all classes, by making provision for the admission into dormitories, etc., of a sufficiency of chemical light. The chain here is not to be broken, if only the primary assumption for which I have contended all through shall hold good. If that stands, the prevention of consumption is assured. I have no doubt—I can have no doubt—whatever, of the soundness of the assumption. It appears to me to be a forced conclusion. If this be affirmed by etiologists, the period of the final disappearance of phthisis from any one, or from all, of the several nations will

depend solely upon their internal organization, or upon their higher or lower civilization.

My task is nearly done. The practical steps to be taken to prevent phthisis have been sufficiently indicated perhaps. Nothing can be plainer than the broad general principle that every bedroom should receive abundance of morning and midday sunlight or daylight, in order to destroy and preclude the growth of the bacillus-vegetation. Yet it will not do to overlook the obstacles in the way of getting the requisite amount of chemical light into all dormitories. In the first place, I infer that the amount of chemical light necessary for rooms of a given size and shape varies in different latitudes, and with our present knowledge it is impossible to lay down anything with precision in regard to the relative size of rooms and windows for any one latitude. Inasmuch as the importance of chemical light in this connection has not yet been recognized, there are no standards for the dimensions, shape, position, aspect, etc., of windows to admit a competent amount of chemical light into apartments of certain dimensions, shape, position, aspect, etc. But when the true relation of the sun to the bacillus shall be seized, sufficiently accurate provisional standards for much or most of the preventive work of a community will soon be forthcoming. Arrangements can at once be made to meet the requirements of a proportion of cases, because, fortunately, excess in the amount of solar radiation supplied to a dormitory can do no harm. Extreme nicety of calculation will not be required in a large number of instances—especially in those in which houses are isolated, or those in which the bedrooms of a house are so numerous that a selection can be made of those required for permanent occupation by the members of the family. The wealthy everywhere, and a large number of the residents in country districts, can

very soon provide themselves with sufficiently sunned bedrooms to bid defiance to consumption and "scrophula" and tuberculosis in nearly all its shapes. Most of the "consumptive families"—all the rich ones—may break the entail, or what has been supposed to be the entail, of a score of generations, by simply changing the bedrooms of the family, or by enlarging present windows, or by putting in other windows to catch the rays of the morning and midday sun. These classes need have no difficulty in placing themselves and their families forthwith in a position of complete immunity from phthisis. They have no abstruse calculations to make as to what amount of chemical light they can cause to be thrown into a room, and as to whether that amount is sufficient, or insufficient, or barely sufficient, for the purpose. All that they have to do to ward off consumption is to see that their bedrooms are lighted by large south-east or somewhat larger south-west, windows; that no interruptions to the free passage of the light (through clear, or unstained, or uncoloured panes) take place; and to bear in mind that a surplusage of light for the specific purpose in view is, to say the least, innocuous. By the due observance of these plain rules, and by a rigid insistence on the actual admission of the chemical light rays far into the room, either through the open windows or through uncurtained or unobstructed panes or sashes, for three or four hours daily, at least, they cannot go wrong. Inmates of such rooms will be as free from a bacillary malaria, and as secure from consumption, as when in the Alps.

It is when we come to consider the question of supplying the dormitories, workshops, offices, etc., of the denizens of towns with a sufficiency of chemical light, that we are confronted with serious difficulties and endless complications. It is here that the want of exact knowledge of the terrestrial action of the sun will be severely felt, and it is

here where such hindrances will be met with as will have the effect of delaying the suppression of phthisis, even in the most enlightened countries, for many years to come. It is a painful reflection that some thousands of lives must, in the mean time, inevitably be sacrificed in crowded cities; and the reflection is all the sadder that, knowing full well the cause, and perceiving clearly the means of the prevention, of all these premature deaths, we are nevertheless powerless to save. The truest sympathy, however, is expressed in work; and there will be plenty of it for the etiologist, the hygienist, the philanthropist, and the legislator, before consumption shall be stamped out of Great Britain. Such a result might well seem hopeless of attainment at this moment when we reflect on what has to be done—when we see that it must be preceded by a revolution in etiological thought; by the reversal of the existing theory of pure parasitism; by the general recognition and acceptance of all the consequences entailed by this organic change, including the abandonment of the ancient doctrines of contagion and heredity; and, finally, by an organized movement of such wide extent and influence as to induce the nation to grapple with the suppression of the disease. Yet I do not concern myself overmuch, for, if there is a solid foundation to these views, the end I contemplate will not be so far off. If it can be made clear to the people of England that consumption is to be eradicated from the closely packed city as well as from the open country, it will be done.

The problem that will be before the practical sanitarian is how to destroy the fields of bacillus-vegetation in cities in the readiest way at the least cost. It has been seen that the sun has been and is our most potent known ally in this work, and that his influence may be utilized with facility in the case of isolated houses of the wealthier classes, and,

indeed, in the cottages of the agricultural and other labourers in the country at but little outlay; but it is palpable that there are many and apparently insuperable obstacles in the way of introducing chemical light into houses in narrow lanes, or even in fairly wide streets when the houses are run up several stories high. It is, of course, simply impossible to get direct sunlight into a large proportion of the rooms used as dormitories in such houses; and the question is by what artificial means the end may be reached. The question opens up a wide field, and will offer opportunities for the display of rare ingenuity. It naturally divides itself into two questions—the one relating to existing, the other to future, houses in cities.

Taking habitations of all kinds as they stand to-day in London, what is to be done to shut out a bacillary malaria from such rooms as do not now receive a sufficiency of chemical light? I have to admit that I am not prepared with a definite answer. For, in the first place, we cannot determine from the data before us what is a sufficiency of chemical light in a room of known size and shape in a given house in London. Latitude and absorption enter into the question, and dryness of the air in the house, and the presence of sulphur compounds from the combustion of coal, may play an important part by way of compensation, owing to their injurious and retarding effect on the bacillus-vegetation. (All these conditions vary more or less in the large towns.) But, in the next place, even supposing that the precise amount of chemical light required to keep down the vegetation in these rooms in London could be determined, how is that amount to be introduced into all the rooms of dwellings with northern aspects or overshadowed by tall buildings? Though we may know what is wanted, how are we to get it?

Reflected light offers the only means of overcoming the

difficulty, but in what shape or form we need not stay to discuss. It is enough to say I conceive that some scheme for transmitting sufficient chemical light into dark dormitories, by a skilful arrangement of reflectors, may be devised and found as practicable as it is to send solar rays far into the interior of some of the caves in Egypt for the purposes of photography. Methods of adjustment of the mirrors or reflecting surfaces to meet the changes in the earth's position relatively to the sun might be designed, and with the adjuncts of whitened walls, and of Sir David Brewster's plan of ground-glass windows, it is not improbable that an adequate quantity of chemical light might be thrown for three or four hours daily into the darkest rooms of some of the darkest habitations of London. The question of supplying the dormitories of the dwellers in towns with chemical light of sufficient intensity resolves itself, in short, into one of expenditure; and though that would necessarily be large, yet, considering the end in view, it is not so large as to be outside the domain of practical hygiene. It will come to a calculation whether the price to be paid for such or such a number of lives is more than the country can afford, or is within its means.

The question of dealing with the houses to be erected hereafter in towns need not detain us. Regulations might be framed to minimize the danger of letting in an insufficiency of light, although, until some exact knowledge be had, it will be extremely difficult to draft enactments that shall express the precise meaning to be conveyed.

It need hardly be suggested that solar chemical light, or the chemical light of total daylight, are not the only means by which the bacillus-vegetation and its soil may be destroyed in habitations. They are the most natural and agreeable and the most easily and cheaply applied means in isolated houses, but there are many other means

by which the deadly vegetation may be reached, some of which may yet prove to be of the highest value in places where sufficient chemical light of the required intensity cannot well be introduced. Heat, electricity, and chemical compounds furnish us with potent agents for the destruction of the vegetation, or of its soil, or both. Some of these agents have been shown to be of great efficacy in the prevention of consumption—the forced conclusion from which is that the prevention has been due to their injurious effects on the local bacillary fields.

Although the heat from ordinary fires in isolated houses does not destroy or keep down the bacillus-vegetation, yet dry air heated up to a certain temperature, either naturally or artificially, is inimical to the forms; and perhaps the extraordinary number of fires kept up in London may have some effect in drying the air in the bedrooms. Hot winds soon desiccate the consumption-plant and its soil also. The immunity of cooks from phthisis, especially in London, as compared with other servants, has been supposed to depend upon the great drying heat from the kitchen-range; and in cases where their sleeping-rooms adjoin the kitchen the warm anhydrous air may undoubtedly prevent the growth of the vegetation: but another element may come into play here as well—the greasy nature of the cook's occupation, which, like that of the butcher or the soap-boiler, may afford protection. There is no reason, however, why heat should not be found to be a most valuable auxiliary in keeping down a local bacillary malaria. In the *Medical Times and Gazette* (December 29, 1883) is a notice of a paper by M. H. Martin and M. Parrot, on the application of hot air to rooms for the express purpose of destroying the infective germs of tuberculosis. The *Gazette* quotes the writers, and we take the following extract:—"The only agent which germs, be

they what they may, cannot resist for a single moment is *fire*. Now, we know that at 100° the tubercle loses its infectious property in a very little while; and a temperature, even dry, of 120° to 125° destroys this power almost instantly. We must conclude that a heated stove is the only certain and practical purifier of instruments for surgical or experimental operations, for clothing, linen, and dressings; in short, for all objects whose shape and size admit of their being introduced into it. As regards the walls, floors, etc., of private rooms or hospital wards, it would be possible, it seems to us, from time to time to submit them to the action of a current of air heated to about 125°, which, being brought by means of suitable portable tubes, and distributed just as one directs a jet of water from a pump, would dry and calcine the infectious germs hanging about the plaster, pictures, or woodwork, without sensibly damaging them."

This ingenious and admirable suggestion has not been carried out apparently, but I conceive that the scorching effect of the direct application of a stream of hot air, for a few seconds only, to the dark nooks and corners, and even under the flooring boards, of dormitories would to a certainty kill off all low forms of vegetation over which it passed. Although it might not destroy all the spores of some forms, no growing forms could survive such treatment; and a periodical application of hot dry air after this fashion would effectively get rid of all existing bacillus-vegetation that came under its influence, and would largely diminish future crops, and, if thoroughly done, might eventually so far affect the soil as materially to reduce, or even to destroy, the field; and it might take many months before sufficient organic matter to form another efficient field would be deposited. Perhaps the plan might be found inconvenient or impracticable in private houses, but modifi-

cations of it suitable for every kind of dwelling would soon be made, if it were determined that consumption is caused by a local bacillary malaria. For all public, or quasi-public, institutions, however, where numbers of persons are domiciled, and where the means of heating air may be found, and the appliances for conveying it by pipes to the sleeping rooms may be supplied at small expense, we consider that this hot-air system is not only practicable, but excellently suited to the purpose. In those lunatic asylums, workhouses, alms-houses, colleges, schools, barracks, prisons, etc., where the mortality from phthisis is excessive, and in which the aspect, construction, or position of the sleeping-rooms may make it extremely costly, or almost impossible, to supply them with sufficient chemical light, it may prove a great aid to the sanitarian.

From what is known of the chemical properties of electric light, it would seem that they are closely akin to those of solar light. Siemen's well-known experiments show this clearly.* He says: "The effect upon the flowering plants is very striking, electric light being apparently more efficacious to bring them on than daylight. . . . Experience alone can determine absolutely the effect of electric light upon the ripening of delicate fruit, but, considering its evident power to form chlorophyl, there seems no reason to suppose that its action would not also in this case resemble that of the sun, and that saccharine matter, and more especially the aromatic constituents, would be produced." He concludes: "1. That electric light is efficacious in producing chlorophyl in the leaves of plants and in promoting growth. 2. That an electric centre of light, equal to fourteen hundred candles, placed at a distance of two metres from growing plants, appeared to be equal in

* "Influence of Electric Light upon Vegetation," *Proceedings of the Royal Society*, March 4, 1880.

effect to average daylight at this season of the year, but that more economical effects can be attained by more powerful light-centres. . . . 4. That plants do not appear to require a period of rest during the twenty-four hours of the day, but make increase and vigorous progress if subjected during daytime to sunlight and during the night to electric light." Taking his investigations, therefore, in conjunction with the photographical effects of electric light, the inference is that the influence of this light is *ejusdem generis* with that of the sun; and as the electric light is seen to have a similar effect to that of the sun upon flowering plants and fruit and in the production of chlorophyl, it is an inference that the electric light is as injurious to certain low forms of vegetation as the sun is. If, therefore, an electric centre of light equal to fourteen hundred candles at a distance of two metres from growing plants has an effect upon them equal to that of average daylight, it may be assumed that the effect upon fungi would also be equal to that of average daylight; or, in other words, the effect of this amount of electric light would be to destroy those vegetal forms which require shade. Consequently the electric light, when the cost of its production, storage, and distribution shall be considerably reduced, may come to be utilized for its chemical effects upon the mould and mildew fields in habitations, as well as for its illuminating qualities. And as the bacillus-vegetation comes, as I suppose, somewhere among these low forms, it is by no means improbable that the electric light may become a strong arm of the sanitarian for the destruction of this vegetation in places where the chemical light of the sun cannot well be introduced. It is not unlikely, indeed, that in many cases the continuous presence of small centres of electric light in bed-chambers insufficiently lighted by the sun, may so far supplement the influence of what daylight is admitted into them as to

compensate for the deficiency in the supply, and thus shut out the bacillus-vegetation. In fine, the electric light promises to be of great assistance in counteracting the baneful effects of keeping out the sun in the dense quarters of large cities; and it will hereafter, perhaps, come to a calculation as to the relative merits and relative cost of a system of reflectors of solar radiation and of electric light centres. Possibly some cheaper forms than any yet known of causing incandescence having the chemical action of the ultra-violet rays may be discovered, and may be turned to account in the destruction of the bacillus-vegetation.

Certain chemical compounds, too, are obnoxious to this vegetation, as may be inferred from the non-occurrence of phthisis amongst the workers at particular trades—tanners, copper-smelters, and others. The hygienist might well have taken a hint long ago from these instances of immunity, but the etiological world has been so impressed with the doctrines of contagion and heredity, which have received such a fillip from the inference of Koch that the bacillus cannot grow outside the body at ordinary temperatures, that efforts for the prevention of phthisis by means external to the organism have probably appeared futile. However, the use of chemical substances is not unlikely to be superseded now by the employment of solar radiation, of the electric light, of hot-air processes, or of some other physical agency destructive to vegetable life. The affirmation of the view here taken that the bacillus-vegetation grows outside the body at ordinary temperatures would at once be followed up by a plethora of schemes for the ready extirpation and permanent preclusion of the vegetation. The precise object being recognized, there would be no lack of practical and economical methods of attaining it. There is no cause for apprehension lest the final result that phthisis will disappear from British and other civilized communities should mis-

carry, but it is matter of deep concern that the work will be so long delayed.

It may be years before some of the questions raised in the issues submitted in this paper will be settled. The cardinal point may not admit of experimental proof or disproof; and probably it will have to be determined on inferential grounds alone, whether Koch's view of the botanical position of the bacillus, or the view here taken, is the correct one. For though one of these two antagonistic views must be right and the other must be wrong, yet the scientific affirmation of the correctness of the one or of the other may not be possible, or may not come, for a decade or two. And as the pressing questions depending on the determination are not to be hung up all that time, the etiologist must have recourse to inference, which, after all, is a safer guide, as has so frequently been seen, than some of the most elaborate and long-trusted observations of the micro-pathologist. But there is no royal road to a sound inference from complex, or questionable, or disputed data. To reach the truth in such a case involves hard work and painstaking sifting. No fact which bears upon it can be left out of consideration, unless at the risk of vitiating the conclusion. The important inference that Koch reached in the matter of the pure parasitism of the bacillus, although it has been so long accepted that it would seem to have passed into the stage of inferential certainty, is rather to be regarded as an authoritative dictum, based upon, or jumped at from, the one fact which came out incidentally in his research, without reference to whole groups of facts and phenomena that point clearly to the opposite inference. It is more of the nature of a decree than an inference. The time is approaching, however, when the intrinsic value of the one fact upon which this momentous conclusion was founded will be submitted to the test;

and when, indeed, the reasons for and against each one of the two views of the bacillus will be tried by every known process—including inference in its legitimate sense.

In the absence of positive or experimental proof of the correctness of either of these views, all phenomena connected with the occurrence and non-occurrence of phthisis must be of some value, for it is absurd to suppose that their clinical nature detracts from their significance; and amongst those of high value will come authentic and trustworthy data relating to the effects of the sun upon the incidence of the disease. If it could be shown beyond doubt, by special investigation directed to that end, that the sun is not only beneficial in treatment, but that it may be so utilized as to prevent consumption, the only rational inference would be that chemical light destroys and precludes the bacillus-vegetation. No more conclusive inferential proof of the accidental parasitism of the bacillus could well be conceived, than a series of well-designed and carefully conducted experiments that should demonstrate that the employment of chemical light, in the manner suggested, is invariably followed by the disappearance of phthisis from habitations which had long been infested by it. For, as we know that the bacillus-spore is not injured by direct insolation, and as, therefore, the suppression of the disease could not be attributed to the destructive influence of the sun on the spores in sputum-dust, we should be driven to infer the existence of a local bacillus-vegetation in a growing state. Man has undesignedly made the experiment in thousands of instances, always with the one result, but it might be held to be more conclusive if the experiment were made of set purpose. The hot-air and some other methods for getting rid of the vegetation, though perhaps they may hereafter prove to be equally effective with chemical light, would not be so conclusive as regards

the question of accidental parasitism, inasmuch as it might still be a question whether the favourable result was not due solely to spore-destruction.

On the other hand, if a thorough trial of solar radiation failed to prevent phthisis, the fact would militate strongly against the view of a free bacillus-vegetation, and would be fatal to the view that this vegetation is governed by the sun. Although it would not be absolutely repugnant to the theory of accidental parasitism, which I take to stand on too firm ground to be upset even if this view of the sun's influence were proved to be erroneous, yet it would undoubtedly nullify a deal of the evidence in support of a free vegetation, if it could be shown that they who have slept for years in bedrooms which have been perfectly sunned, in the sense indicated here, have nevertheless contracted consumption. My proposition based upon that evidence is that a person placed under such conditions has not been, and cannot by any possibility be, invaded by a sufficient number of bacillus-vegetation forms to cause ordinary pulmonary phthisis. If this proposition be proved to be unsound, the inferences drawn from the phenomena relating to the apparent relation of the sun to the incidence of phthisis are, admittedly, likewise unsound. I have, however, examined them so rigidly and tested them so carefully by every phenomenon remotely or nearly connected with the occurrence of phthisis, and have found them so perfectly in accord with every known fact, that I have no room for doubt, either that the bacillus is an accidental parasite, or that the life of the vegetation in its natural or artificial phases external to the body is principally dependent on the chemical action of the sun.

But I do not conceal from myself that the processes of reasoning that have satisfied me on these points will not satisfy others, and that much remains to be done before it

will be determined whether the novel views of the bacillus submitted here will be generally accepted or rejected. It is unlikely that all etiologists will form their judgment upon the facts and arguments at present before them. They will require further and perhaps stronger proofs. They may distrust a new and disturbing interpretation of familiar phenomena, and may insist upon direct experiment in the laboratory, or special observations made expressly by recognized authorities, before they will seriously entertain views which strike at the root of all they have learnt and of all they have taught. They must have "grounds more relative" than they can find in my contention, which, not being supported by original research of any kind, or even by statistics, will doubtless appear to some minds as the wildest speculation.

For these and other reasons, therefore, I trust that such members of the profession as may be moved to put the views here submitted to the proof, will, if they find themselves in a position to do so, set themselves systematically to work. Unfortunately, there are numerous instances where the mortality from phthisis in public institutions is not only far in excess of that of the general population, but greatly exceeds that of kindred institutions in which a similar class of people is housed. It is well-known matter of fact that some barracks, prisons, poor-houses, lunatic asylums, etc., have for many years past had a much larger death-rate from consumption than others; and where this cannot be set down to any appreciable difference in the age, sex, constitution, occupation, etc., of the inmates of two similar institutions, or cannot be otherwise sufficiently accounted for, there is the opportunity for determining whether the incidence of the disease has depended on purely local conditions. If the occurrence of the cases of phthisis has been governed solely by the intensity of the

chemical action of daylight in the dormitories of these institutions, it will, in the first place, be a very easy matter to ascertain whether the phthisical inmates have slept, or worked, or remained all day in rooms insufficiently lighted. Next, if this be found to have been a constant condition precedent to their infection, which I take to be a certainty, it will not be difficult to determine whether or not the phthisis has been a consequence of a local bacillary malaria encouraged by a deficiency of chemical light. All that has to be done is to let the sun into the infected rooms in the manner and to the extent here indicated, or to remove the occupants of these rooms into well-sunned rooms, leaving the unsunned rooms unoccupied. A year or less will tell its tale in the death-rate from consumption. If from overcrowding of the institution, or from any other cause, such as a paucity or an absence of unsunned rooms (a condition which obtains in many of these places), it be found impracticable, or utterly impossible, to place all or any of the inmates under the full and sufficient influence of chemical light, there remain the hot-air and other processes to be resorted to. And although these expedients, even if successful, would not have the same value, etiologically considered, as the plan of sun-prevention, still they would have their etiological value, and be of immense hygienic value. Of course, another aspect of such experiments naturally suggests itself, but it need not be urged upon medical men.

Much remains to be said, but it may remain unsaid; for if what has been said is not enough for the purpose, more would be waste of time.

THE END.

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