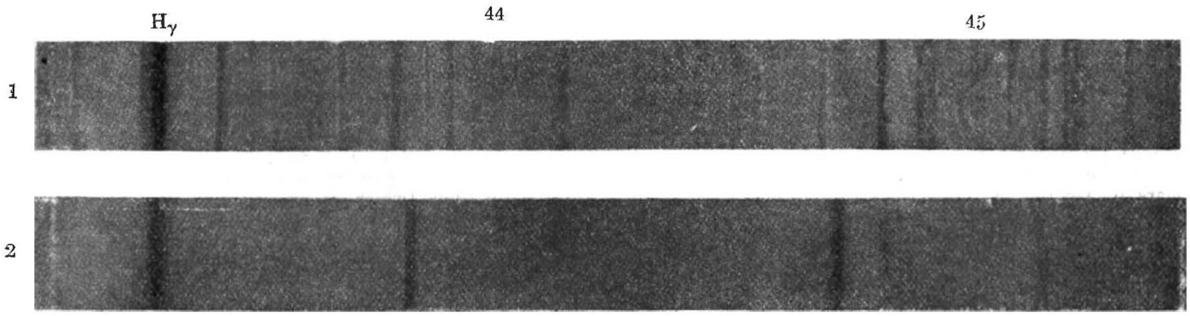
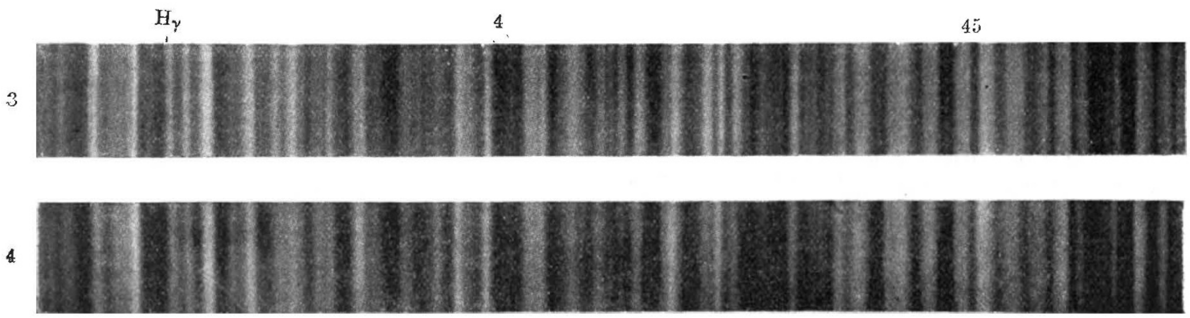
as those resources are in some instances—we have really an opportunity of doing a considerable amount of work. The particular bit of work which is represented in this diagram is an inquiry showing how the iron lines observed in the spectra are represented in these different stars; we can see whether the condition of the iron vapour is the same in a star like Arcturus as it is in a star like γ Cygni. You will see that from this point of view there is a great difference in the atmospheres of these two stars.

The definition of the negatives obtained by means of the objective prism is of such excellence that they may be almost indefinitely enlarged, and this gives them a special value when we come to investigate the smaller differences between stars which have more or less resemblance to each other. Practically, we are able to dispense with elaborate micrometric measurements, and by placing the enlargements alongside each other, to see at a glance which lines agree in position and which are different.



1. α CYGNI. 2. γ ORIONIS.



3. α TAURI. 4. α ORIONIS.

FIG. 11.—Portions of stellar spectra, greatly enlarged.

The spectra represented in Fig. 11 have been enlarged twelve times from the original negatives, and on this scale the whole of the visible spectrum would be more than a yard in length. Here we observe, in the first place, the immense difference between two groups of stars, one in which the lines are very numerous, the other in which the number of lines is relatively small. We also have an opportunity of studying the more minute differences between stars like α Tauri and those like α Orionis, and between stars like α Cygni and those like γ Orionis. In some cases, differences which might easily be overlooked altogether in an examination of the negatives alone, become apparent when the scale is enlarged in this way.

Thus, by means of the aids which have been placed at our disposal, by the recent improved condition of our stock in trade, and the wonderful diligence and skill of observers, chiefly in America, who have taken up the new work, we are now in a very much better position than we have ever been before to investigate this subject.

J. NORMAN LOCKYER.

(*To be continued.*)

NATHANAEL PRINGSHEIM.

BOTANISTS throughout the world will have heard, with deep regret, of the death of Prof. Pringsheim on October 6, of last year. His name is inseparably associated with the modern progress of the science, and there must be many, who, like the writer of this notice, can trace their first interest in scientific botany, in no small degree, to the fascination of Pringsheim's discoveries. Pringsheim was born in Silesia, in 1823. His career, though an active one, was unusually free from official cares. Except during four years, when he was Professor at Jena, he does not appear to have held any teaching post of importance. During the greater part of his scientific life, his work was carried on in a private laboratory, founded by himself at Berlin, and devoted entirely to the researches of original workers.

Pringsheim was founder and editor, from 1858 to the time of his death, of the famous *Jahrbücher für Wissenschaftliche Botanik*, and was President of the German Botanical Society from its first origin. He was among the many distinguished foreign men of science who attended the meeting of the British Association at Manchester, in 1887.

The scientific activity of Pringsheim extended over a period of fully forty years (1848–1888), a time which covers what was perhaps the most brilliant epoch in the history of botany. His work began in the days when the science of histology was being built up on the basis of the cell-theory of Schleiden and Schwann; he himself contributed essentially to its construction. Pringsheim's greatest services to science, however, were in the department of the morphology and life-history of the lower plants, a line of research in which he was unsurpassed. Comparatively late in life his attention became directed to physiology, but in this direction his success was less conspicuous.

Pringsheim's earliest contribution to science was his Latin dissertation, "De formâ et incremento stratorum crassiorum in plantarum cellulâ" (Linnæa, 1848), in which he discusses a question much agitated at that time, whether the increase in thickness of the cell-wall takes place from without or from within. He decides, chiefly from observations on the cells of the testa in seeds, in favour of apposition from the interior, a conclusion which no doubt holds good for the great majority of cases, though the researches of Strasburger have shown that this is not a universal rule.

Not long afterwards, Pringsheim made his first investigation of a question which continued to occupy him, at intervals, for more than thirty years, namely, the development of the Saprolegniaceæ, a family which lies on the confines of the Algæ and the Fungi. At that time the delimitation of genera and species among these plants had fallen into great confusion. Pringsheim's memoir of 1851 bears the title, "On the development of *Achlya prolifera*," but its subject was really a *Saprolegnia*. The zoospores of this family had long been known, and their resting-spores had already been observed by Schleiden and Nägeli. Pringsheim, however, was the first to prove that the two kinds of spore are produced by the same species; he watched the germination of the resting-spores, and saw that they gave rise to the ordinary form of the plant, with its zoospores. At that time he regarded it as an open question whether these plants are to be classified as Algæ or Fungi; they are now assigned to the latter class, but placed near its lower limit, where there has not been much divergence from the original algal stock.

Six years later, Pringsheim discovered the sexual reproduction of the Saprolegniaceæ, an observation of the greatest importance, though his account was not free from mistakes, most of which he corrected himself in subsequent memoirs. It is a remarkable fact that in certain species of the family some individuals are entirely without male organs, and produce their oöspores parthenogenetically. In others the antheridia are present, but there is no penetration of the oögonium. The question whether actual fertilisation takes place in *any* Saprolegniaceæ was the subject of a keen controversy between Pringsheim and De Bary; the latter, for reasons which we have not space to enter into, held that the family, as a whole, is *apogamous*, *i.e.* that the sexual organs, even when present, have lost their function. Pringsheim, on the other hand, maintained that in the forms with fully developed antheridia true fertilisation takes place by means of "spermamœbæ" The question was under discussion up to the year 1883, and cannot even now be regarded as finally settled. Pringsheim's observations of the supposed amœboid male cells are not very convincing, but neither is the negative evidence against the occurrence of fertilisation in the family decisive. The higher Fungi are characterised, as most botanists now agree, by a total loss of sexuality; in the Saprolegniaceæ we can, at any rate, trace the beginnings of this retrogressive change.

It was, however, among the Algæ themselves that Pringsheim's chief triumphs were won. His earliest purely Algological paper was on the germination of the resting-spores of *Spirogyra*, in which he first demonstrated the interesting fact that the embryonic plant presents a distinction of apex and base, which is quite lost in the later stages of its growth. The supposed motile spores of *Spirogyra* (described in the same paper, 1852) really belonged to a Chytridiaceous parasite. These inconvenient intruders had already misled Pringsheim in some of his earlier observations on Saprolegniaceæ. In the same year, he made out the reproduction in an interesting little Alga (*Cœlastrum*) allied to the "Water-net."

Pringsheim's really fundamental investigations of the Algæ begin, however, with the year 1855, when he communicated to the Berlin Academy a memoir on the fertilisation and germination of the Algæ, and on the nature of sexuality. The chief point of this paper is the demonstration of the sexual reproduction of *Vaucheria*. Vaucher himself had discovered the "anthers" at the beginning of the century; it was reserved for Pringsheim to find the spermatozoids, and to observe the act of fertilisation. The proof of such an advanced mode of reproduction in an unicellular (or rather non-cellular) plant, was a startling discovery, and provoked some opposition among those who had not the skill to repeat Pringsheim's observations. This paper further contains researches on the fertilisation of *Fucus*, and on the sexual organs of Florideæ (which of course was only fully understood at a later time), and preliminary observations on the remarkable life-history of *Œdogonium* and *Bulbochæte*.

These latter discoveries were more fully announced in the next year (1856), and two years later the whole history was told in a magnificent memoir, published in the first number of the *Jahrbücher für Wissenschaftliche Botanik*. This work is a model for all such investigations, and has left very little for subsequent observers to do, as regards these plants, which in some respects present unique peculiarities. The Œdogoniaceæ possess (in addition to the asexual zoospores) highly differentiated ova and spermatozoids. Many species are monœcious, and a few diœcious; most, however, present a remarkable form of dimorphism. Certain cells produce ova; others (usually in the same plant) give rise to small zoospores, which become free, attach themselves to the oögonia, or to adjacent cells, and there germinate into dwarf male plants. Each of these dwarfs consists of a single vegetative cell and an antheridium, in which two or more spermatozoids are produced, by which fertilisation is effected.

Another interesting point discovered by Pringsheim, is the germination of the sexually produced resting-spores, which do not grow directly into new plants, but give rise, each, to four zoospores; we thus have a case here, either of polyembryony, or of alternation of generation, according to our interpretation of the phenomena.

Two years later (in 1860), Pringsheim published an equally important memoir on another group of freshwater Algæ—the Coleochæteæ. These curious little plants, which grow on the larger water-weeds, go through one cycle of development in each year. During the earlier part of the season, only asexual individuals, reproduced by zoospores, are formed. At the close of summer the last generation produces sexual organs also. After fertilisation (which is effected by spermatozoids) the oöspore passes into its winter rest, and then germinates in the spring, giving rise, not to a normal plant, but to a parenchymatous, fruit-like body, in each cell of which a zoospore is formed. There is thus an evident analogy with the life history of the Mosses, which Pringsheim at once recognised. Of all Thallophytes, the Coleochæteæ show the nearest approach to that form of alternation of generations which is so general among the Bryophyta. Whether this is anything more than an analogy, is still an open question.

A controversial Algological paper of the same date (1860) is of interest, because the author opposes the view (held at one time by Thuret) that only a *dynamical* reaction takes place between spermatozoid and ovum. Pringsheim showed that the two sexual cells undergo an actual material fusion, a fact which lies at the root of all sound views as to the nature of the sexual process.

A year later, Pringsheim added to our knowledge of the remarkable course of development of the "Water-net" (*Hydrodictyon*), by tracing the history of those minute zoospores which become free from the parent-plant, and give rise to resting-spores. In this case, however, he overlooked the occurrence of conjugation, which, according to a subsequent observer, takes place between these small motile cells.

In 1862, Pringsheim turned his attention to marine Algæ, and published some observations on the Red Seaweeds, in which he rightly described the structure of the sexual organs. Their function, however, was only demonstrated, five years later, by Thuret and Bornet.

To the year 1863 belongs an important memoir on the Characeæ, a group of plants, which, though so popular as text-book types, is still a mystery as regards its relationships. Pringsheim's observations were chiefly in the proembryo and allied structures. He established the existence of a striking correspondence between these organs and the protonema of the Mosses.

After an interval of six years, he returned, in 1869, to the investigation of the Algæ, and made known perhaps the most important of all his Algological discoveries, that of the conjugation of zoospores, a process which he regarded as the primitive form of sexual reproduction in plants. This striking discovery was first made in *Pandorina*, one of the Volvocineæ, a family which is often claimed by the zoologists, but which, in its reproductive phenomena, betrays the closest affinity to undoubted plants. The motile cells, which conjugate in pairs, sometimes differ considerably in size, while in other cases they are almost exactly similar. The product of their union is a resting-spore, which on germination ultimately, though not directly, gives rise to a new colony. Pringsheim observed the details of the fusion of the sexual cells, and found that they first become united by their anterior ciliated ends, which consist of clear protoplasm. He compared this part of the conjugating cell to the "receptive spot" of the ovum in the higher Algæ, to the canal-cells of archegoniate plants, and to the synergidæ of Angiosperms.

The conjugation of motile cells has since been observed in many other families of Algæ, of the most diverse affinities, and in several of these families we can now trace the advance from this primitive fusion of like cells, to the union of a differentiated ovum and spermatozoid. Pringsheim's discovery has thus proved to have all the far-reaching significance which his scientific insight attributed to it, from the first. It has now become customary to speak of active conjugating cells as "planogametæ," a word which is useful, as indicating their sexual function, but which must not be allowed to disguise their complete homology with the asexual zoospores.

In 1871, he discovered the male plants, and the presumable sexual cells of *Bryopsis*, a marine Alga, allied to *Vaucheria;* the process of fertilisation however, has not yet been observed.

Pringsheim's latest contribution to Algology was his interesting work on the course of morphological differentiation in the Sphacelariaceæ, a group of Brown Algæ, in which the progress from filamentous to cormophytic structure can be traced through a very complete series. His views on evolution were akin to those of Nägeli (*cf.* NATURE, vol. xliv. p. 582). He saw clearly the evidence of descent, but could not admit that natural selection is a sufficient explanation. It seemed to him that the highly differentiated *Sphacelaria* has no advantages, in the struggle for existence, over the simple *Ectocarpus*. He held that the first deviations from primitive simplicity of structure, are of a "purely morphological nature," and have no demonstrable relations to any physiological functions.

Views of this kind are widely spread among German naturalists. The greater, however, our knowledge of the conditions of life in plants becomes, the less room is left for these supposed "morphological characters," which are probably, at most, nothing more than adaptive characters which became fixed a long time ago.

We have endeavoured to give a connected account of Pringsheim's Algological work. During the same period, however, he had also made important contributions to science, in other directions; one or two of these must now be noticed.

A work bearing the date 1854, on the structure and formation of the vegetable cell, belongs to the period when the doctrine of the multiplication of cells by division had just gained the victory over the erroneous theory of free-cell-formation, so pertinaciously defended by Schleiden. Pringsheim's researches afforded valuable support to the new views. He observed cell-division in both vegetative and reproductive cells of many Algæ, as well as in the pollen-mother-cells of Phanerogams. His view of the formation of the cell-wall comes wonderfully near to the conception of this process, which we have attained at the present day.

Pringsheim's work on the morphology and development of *Salvinia* (1863) is his most important contribution to our knowledge of the higher Cryptogams. He completely worked out the entire life-history of this interesting plant, and a more perfect monograph has never appeared. His observations will always form the basis of our knowledge of *Salvinia*, which is one of the most highly modified forms of Pteridophyta; they also throw great light on the embryology and development of the class as a whole.

Pringsheim, in 1876, crowned his long series of morphological investigations by a remarkable essay on the alternation of generations in the Thallophytes, and its relation to that in the Mosses. In the opinion of the writer of this article, this is, from a theoretical point of view, his most important work.

The essay was suggested by the experiments of its author on the sprouting of Moss-fruits. These experiments were undertaken by Pringsheim with the express object of determining whether the Moss-seta could be induced to subserve vegetative propagation in the same way as the Moss-stem, which it so closely resembles anatomically. He found that the seta of *Bryum* and *Hypnum*, when divided, and kept in moist air, developed a normal protonema, from which true Moss-plants arose. In other words, he succeeded in producing the sexual from the asexual generation, without the intervention of spores, thus establishing the first case of *apospory*, our knowledge of which has since been so much extended. In the meantime the converse phenomenon of *apogamy* had been demonstrated by De Bary and Farlow, who showed that the asexual generation, in certain Ferns, may arise from the sexual prothallus, without the intervention of the sexual organs. These facts really formed the groundwork of Pringsheim's theory of alternation.

His leading idea is that the successive generations among the Thallophytes are to be sought in the free sexual and asexual forms of the plant, and not in the plant itself on the one hand, and its fruit on the other. To take only one example: in the case of the Florideæ, Pringsheim regarded the tetrasporic and the sexual individuals as constituting the alternate generations; the sexually produced crystocarp he interpreted, not as a distinct generation, but as a case of polyembryony. Having expanded this view, he proceeds to face the difficulty, how it is to be reconciled with the life-history of the Mosses (in the widest sense), in which the alternation is between vegetative plant and fruit.

His reply is very ingenious: in the germination of the sexually produced spore of many Thallophytes the young plant suppresses more or less completely its vegetative stage, and proceeds at once to the formation of asexual spores; this is the case, for example, in *Œdogonium*, *Sphæroplea*, *Hydrodictyon*, and more especially in the Phycomycetous Fungi, in which every transition can be traced between "sporangial" and "mycelial" germination of the sexually produced spore. Pringsheim goes on to say: "It may therefore hold good, as a general experience among Thallophytes, that the *first* neutral generation hurries by a short road to spore-formation, suppressing more or less the vegetative part of the plant." This change is most marked where germination takes place within the oögonium, as in *Coleochæte*. "Alternation of generations in the Mosses thus appears as a contracted form of that in Thallophytes; in the former the neutral generations are reduced to a single one, which remains in unbroken connection with the sexual plant. There is therefore no reason for comparing the sporogonium of a Moss with the fruits of the Thallophytes," and so on.

On this view it follows that all alternation of generations must be regarded as "homologous." Sporophyte and oöphyte, however differently modified, are homologous one with another, both having been derived from sexual and asexual individuals, which at one time presumably differed as little from each other as do the cystocarpic and tetrasporic plants in Red Seaweeds. It is therefore not surprising that in cases of apogamy and apospory, the one generation may still pass over directly into the other.

These views have been much criticised, and the case is still *sub judice*. In England the opposite theory—that the alternation in the higher Cryptogams is "antithetic," the sporophyte being an intercalated generation, not homologous with the oöphyte, is predominant, and appears to receive some support from certain minute histological differences between the two generations. Pringsheim's interpretation of the facts has, however, some advantages, perhaps the most conspicuous among which is that it would enable us to understand the existence of the immense and unbridged gulf which separates the sporophyte of the Muscineæ from that of the Vascular Cryptogams. The latter might well have been derived from ancestors, in which the "first neutral generation" had never suffered the extreme reduction which characterise it in the Moss series, but had always retained its vegetative organs. It would then no longer be necessary to endeavour to force the plant of a Fern or a Horsetail to fit into the Procrustean limits of a Moss-sporogonium.

We have endeavoured to give some idea of Pringsheim's varied activity in the field of morphology, which he cultivated with unsurpassed success. It remains to say a few words regarding his physiological investigations, though these are far from possessing the same importance.

So far as we have been able to find, Pringsheim's first entry upon the physiological domain dates from the year 1875, when he published papers on the spectrum of chlorophyll, and on the modifications of that pigment. His *début* as a physiologist thus almost exactly coincides with the climax of his brilliant career as a morphologist. It was not till 1879, however, that he became prominent in physiological questions. In that year he produced his first paper on the action of light and the function of chlorophyll. He investigated the action of light of great intensity on vegetable tissues, and observed the consequent destruction of the chlorophyll, in green tissues, and the disorganisation of the protoplasmic structures, both of which he attributed to excessive oxidation. The great conclusion at which he arrived, is that "the function of chlorophyll, by means of its powerful absorption, especially of the so-called chemical rays, is to limit the intensity of respiration, and to serve as a regulator of that process." He then proceeds further, and states that "the fact that only *green* parts of plants evolve oxygen, finds its sufficient explanation in the diminution of the amount of respiration, owing to the presence of the chlorophyll screen." Pringsheim thus regarded assimilation as a function of the protoplasm alone, with which the chlorophyll has nothing to do, except in a purely negative way, by keeping off rays which would induce the opposite process of oxidation.

In the same year, Pringsheim announced his discovery of a body which he called Hypochlorin. This he was able to demonstrate in the chlorophyll corpuscles by the aid of certain reagents, notably dilute hydrochloric acid. He regarded hypochlorin as the first and most constant product of assimilation.

Both of Pringsheim's conclusions were sharply attacked, and an extensive controversial literature soon grew up. As regards the hypochlorin, it seems certain that Pringsheim was mistaken in the importance he attributed to this body. It has been clearly shown by Arthur Mayer, and others, that hypochlorin is not a product of assimilation, but is derived from the chlorophyll itself, by the action of the reagents employed.

The other theory, that of the screen-action of chlorophyll, has not met with much favour from physiologists, and it is clear that this is, at any rate, not its only function. The investigations of Timiriazeff and Engelmann have proved that certain of the rays absorbed by the chlorophyll (principally those in the red) are just the rays most active in the decomposition of carbon dioxide. The chlorophyll, in fact, is not merely a screen, but is also a light-trap, which catches and detains those rays which are most effective in the assimilative work of the plasted. On the other hand, recent investigations as to the action of light on protoplasm have shown that many pigments (such as those of spores and pollen-grains) are useful as screens, and have rendered it probable that this may be a function (though not the most important one) of chlorophyll also. The discovery, by Winogradsky, that certain Bacteria can assimilate carbon from inorganic carbonates, without chlorophyll, and in the absence of light, is a striking confirmation of the view that the seat of assimilation is the protoplasm itself, and to this extent Pringsheim's opinion is completely justified.

It would be unfair to deny considerable credit to Pringsheim for the boldness and originality of his physiological theories, and the energy with which he supported them. Yet it must be admitted that his views on these subjects were one-sided, and not characterised by the same sober judgment which distinguishes his morphological researches. Entering, in mature life, upon an unaccustomed field of investigation, he failed to add greatly to his previons reputation; and though much of his physiological work is suggestive, it has not given us, as he intended it should, a consistent theory of assimilation.

Passing over some of the physiological papers of secondary importance, we come to Pringsheim's last work (1888), which treats of the origin of calcareous incrustations on water-plants. He explains this process by the removal of carbon dioxide from calcium bicarbonate during assimilation, causing the precipitation of the insoluble calcium carbonate. This theory is supported by the interesting observation that the incrustation only takes place in the light; but it may be doubted whether the explanation given is sufficient.

We have not attempted to record all Pringsheim's researches, but enough has perhaps been said to give some idea of his life's work.

In addition to his original investigations, Pringsheim rendered a great service to science by means of his magnificent Year-book for Scientific Botany, which shares, with the botanical portion of the French *Annales des Sciences Naturelles*, the honour of constituting the finest serial record of morphological and physiological botany. It is satisfactory to hear that, since the death of the founder, the editorship of this great publication has been undertaken by two such distinguished botanists as Profs. Strasburger and Pfeffer.

By the death of Pringsheim we have lost another of the leaders who guided scientific botany through the period of its most vigorous growth. Very few of his generation now remain; they have left worthy successors behind them, but the work of Pringsheim, in the field of morphology, will not soon be rivalled.

D. H. SCOTT.

THE ANTITOXIC SERUM TREATMENT OF DIPHTHERIA.[1]

THE subject with which we shall deal to-night, though at first sight of interest to the physician only, has been so fully discussed in the public prints, and has been so bitterly and irrationally opposed on the one hand (perhaps also unreasonably beplauded on the other), that those who take even a general interest in the public health, or who are wishful to obtain some insight into the practical and scientific aspects of a new system of treatment, may well be interested to know something of what is being so freely discussed in the columns of our daily newspapers. Beyond this, however, many take a more personal interest in a method of treatment which holds out promise of help in the cure or amelioration of the symptoms and conditions met with in diphtheria, a disease which, very justly, is looked upon as one of the most dangerous with which the physician has to deal. To begin with, I should like to make a frank confession. With that conservatism which is met with even in the most radical of natures, many, of whom I was one, felt disposed to treat antitoxic serum as belonging to the same group of substances as tuberculin, around which was constructed a theory of which the laboratory experimental basis, though apparently fair and firm, was as yet insufficient for the support of the structure of therapeutic treatment that was afterwards raised upon it. I followed the earlier experiments on this new method with great attention; I carefully analysed the principles on which the method was founded, and then with some misgivings watched the gradual development of the treatment as applied to actual cases of diphtheria. I was inclined to receive the statistics with great reserve, as I felt that this new method, like all new methods of treatment, might be making cures in the minds of the observer, and not on the bodies of the patients. Now, however, I am convinced that whatever justification my incredulity may have had from the consideration of previous experiments, none could be claimed in connection with the experiments that were carried out in the investigation of this special subject, and I am thoroughly satisfied that, although the antitoxic serum treatment may not come up to the expectations of all the rash writers on the subject—for many people seem to think that it should be a specific against diphtheria in all its stages—it promises, and this promise has in part been redeemed, to diminish the diphtheria case mortality in a very remarkable manner.

What is Diphtheria?

It is primarily an inflammation of the mucous membrane (the moist skin) of the tonsils, of the soft palate, of the upper part of the gullet, and of the upper part of the windpipe. During the course of this inflammation, which appears to be set up by the action of a special bacillus, there are usually thrown out some of the fluid elements of the blood and some of the white cells that float in the blood; these coagulate and form a soft toughish layer or film which offers an excellent food and resting place for this bacillus, which under such favourable conditions secretes or manufactures a most virulent poison. This poison is rapidly absorbed into the blood and is carried to various parts of the body; its effects are evident at first only on the nervous system, but afterwards on the muscles.

The Bacillus of Diphtheria.

First as to the bacillus. In 1875 Klebs described a short bacillus which he found on the surface of the greyish leather-like diphtheritic false membrane or film. Following up these observations, Loeffler traced a definite etiological relationship between this bacillus and diphtheria. First he obtained pure cultures of the bacillus by growing it on solidified blood serum, or on a mixture of three parts of blood serum and one part of neutralised beef bouillon containing extract of beef, 1 per cent. of peptone, 0·5 per cent. of common salt, and 1 per cent. of grape sugar. This organism may be readily detached from the surface of the false membrane by pressing firmly but gently with a little bit of cotton wadding twisted round the end of an iron wire or an ordinary penholder. When stained and examined under the microscope the diphtheria bacilli are found to be small rods from 3 to 6 μ (1 $\mu = \frac{1}{25000}$th of an inch) in length, fairly plump, straight, or slightly curved, sometimes wedge-shaped or pointed, but usually somewhat enlarged and rounded at the ends, where also in stained

[1] A lecture delivered at the Royal Institution, on Friday, February 8, by Dr. G. Sims Woodhead.