Against these advantages the process of analysis advocated by the speaker involves more trouble and more careful manipulation than are usually bestowed upon what are called "commercial" analyses, and although these drawbacks ought not to be paramount considerations, where such important issues are involved, yet if any other more simple method existed from which trustworthy quantitative information about the organic matter in water could be obtained, the more troublesome process would cease to have a *raison d'être.*

Such a simple alternative method of determining organic nitrogen, but not organic carbon, is now very extensively used by chemists. It is known as the "albuminoid ammonia" method, and depends upon the fact that, by boiling with an alkaline solution of potassic permanganate, most nitrogenous organic bodies are decomposed with evolution of ammonia. From the amount of ammonia so evolved, the proportion of organic nitrogen is calculated. A critical examination of the results obtained by this method conclusively demonstrates that it is incapable of converting into ammonia either the whole, or any definite proportion, of the organic nitrogen of potable waters. Indeed, this is shown not only by the following instances, but also by numerous others in which known quantities of nitrogenous organic matters of known composition were submitted to the process.

Results of analysis expressed in parts per 100,000 :—

Artificial Waters containing Peaty Matter.

	Organic nitrogen by combustion.	Organic nitrogen by albuminoid ammonia process.
Sample No. 1	·068 part ...	·016 part.
,, No. 2	·042 ,, ...	·016 ,,
,, No. 3	·076 ,, ...	·022 ,,
,, No. 4	1·015 ,, ...	·308 ,,
,, No. 5	1·175 ,, ...	·422 ,,
,, No. 6	·029 ,, ...	·011 ,,

Natural Waters.

Chelsea Company's water	·058 ,, ...	·011 ,,
West Middlesex Company's water	·027 ,, ...	·012 ,,
Southwark Company's water ...	·061 ,, ...	·024 ,,
Grand Junction water	·031 ,, ...	·006 ,,
Lambeth Company's water... ...	·062 ,, ...	·030 ,,
Artesian well water	·033 ,, ...	·003 ,, / ·004 ,,
Sea water, No. 1	·217 ,, ...	·006 ,,
,, No. 2	·134 ,, ...	·018 ,,

It is almost superfluous to say that any opinion as to the quality of a sample of water, based upon the albuminoid ammonia obtained, must be entirely untrustworthy.

Dr. Frankland summed up with the following conclusions, to which he had been led by the experiments of himself and others :—

1. That the "albuminoid ammonia" process of analysing water affords no evidence whatever of the absolute quantity, either of organic matter, or of organic nitrogen present in potable water.

2. That it does not indicate, even approximately, the relative quantities either of organic matter or of organic nitrogen in different samples of such water.

3. That it affords no indication, either of the presence or of the proportion, of *albuminoid* as distinguished from other nitrogenous organic compounds.

4. That the "combustion" process, though more troublesome, is the only method at present known which affords any trustworthy information respecting the organic matters present in potable waters.

5. That it is the only method which even professes to determine organic carbon in such waters.

6. That the determinations by it of organic carbon and nitrogen are fairly accurate, notwithstanding the very minute quantities of matter dealt with, and that the errors even of a comparatively inexperienced analyst fall far short of the limits which would affect a verdict upon the quality of the water submitted to investigation.

7. That it is the only process which discloses the proportion of nitrogen to carbon in the organic matter of waters, such information being often of prime importance in reference to the origin of the organic matter.

8. That since the improvements which have been made in the mode of evaporating the water to be analysed, the process can now be conducted in any laboratory and with a moderate expenditure of time and labour.

RELATION BETWEEN THE LIMIT OF THE POWERS OF THE MICROSCOPE AND THE ULTIMATE MOLECULES OF MATTER[1]

THE subject which I have selected for my address is the relation between the limit of the powers of the microscope, and the ultimate molecules of organic and inorganic matter. I think I may at all events claim for this question sufficient novelty. Until the last few years the subject could scarcely have been attempted, and even now so many necessary facts are imperfectly known, that nothing more can be done than to fill the gaps with plausible assumptions. This necessarily imparts more or less of a speculative character to some of my remarks ; but it appears to me that in his annual address the president of a society cannot do better than endeavour to point out the general bearings of what is already known on some great question, even if for no other object than to prove the need of further inquiry.

Though fully impressed with the imperfect state of our knowledge, yet, even now, the facts are sufficiently definite to indicate, if not to prove, the existence of as wide a world of structure beyond the limit of the power of the microscope, as what has been revealed to us by it is beyond the powers of the unassisted eye.

I propose to divide my subject into three heads—

1. The limits of the power of the microscope.
2. The size of the ultimate molecules of organic and inorganic matter.
3. Conclusions to be drawn from the general facts.

In considering the limits of the power of the microscope, I shall assume that the instrument itself is perfect, and that the invisibility of the objects examined is in no way dependent on the want of the necessary characters. The point to which I particularly wish to direct attention is the limit of visibility depending on the constitution of light, beyond which light itself is of too coarse a nature to allow of our seeing objects distinctly defined. This question has been treated of in an admirable manner by Helmholtz in the jubilee volume of *Poggendorff's Annalen* (1874, p. 573). The conclusion to which he arrives is that the limit depends on the confusion in the image due to the bright interference fringes overlapping the dark outlines of the object. This limit varies directly as the wave-length of the light, and inversely as the sine of half the angle of the aperture of the object-glass when illuminated by means of a condenser of equal aperture. According to this principle the limit for dry object glasses of 60° aperture is, roughly speaking, about equal to the wavelength of the light, and for the largest possible aperture equal to $\frac{1}{2}$ the wave-length. In the case of immersion object glasses of the same aperture, the limit is about $\frac{3}{4}$ of that for dry.

On comparing this theory with the results of observation, the agreement is very striking. It indicates exactly the same law for the increased defining powers of lenses of large aperture, as has been determined by experiment, and gives for the theoretical limit of distinct visibility $\frac{1}{80000}$th part of an inch, which is exactly the same as the mean of the experimental determination of a number of the most skilful microscopists. It also shows why in the case

[1] Anniversary Address of the President of the Royal Microscopical Society, H. C. Sorby, F.R.S., &c. Abstract by the Author.

of lines at equal intervals, like Nobert's bands or the markings on Diatomaceæ, it is possible so to manage the illumination that the dark fringes of interference may coincide with the true lines of structure, and give rise to good definition, even beyond the normal limit, and also agrees with the fact that lines less than $\frac{1}{100000}$th of an inch apart can be *photographed*, though *seen* with extreme difficulty, if indeed truly resolved, except under very peculiar and exceptional conditions; since the waves of light at the blue end of the spectrum, which are concerned in photography, are short enough to give good definition of lines so near together that the interference fringes due to the longer waves at the red end would give an indistinct image. Taking everything into consideration, the agreement between observation and the theory is so close as to make it extremely probable, and to justify the conclusion that the normal limit of distinct visibility with the most perfect microscope is $\frac{1}{2}$ of the wave-length of the light. If so, we must conclude that, even with the very best lenses, except under special conditions, light itself is of too coarse a nature to enable us to define objects less than $\frac{1}{80000}$th or $\frac{1}{100000}$th of an inch apart, according as a dry or an immersion lens is used. We must also conclude that, *as far as this question is concerned*, our microscopes have already reached this limit, whatever improvements may remain to be made in other respects.

2. *The Size of the Ultimate Atoms of Matter.*

After discussing the results obtained by Stoney, Thompson, and Clerk-Maxwell, mainly derived from the properties of gases, I come to the conclusion that in the present state of our knowledge the best approximation that we can make to the size of the ultimate atoms of matter is the mean of their determinations. I adopt for simplicity $\frac{1}{1000}$th of an inch as the unit of length, and $\frac{1}{1000}$th of an inch cube, or $\frac{1}{10000000000}$th of a cubic inch, as the unit of volume. In the case of a true gas the number of atoms in the length of $\frac{1}{1000}$th of an inch at 0° C., and a pressure of one atmosphere, would be 21,770, and hence, in $\frac{1}{1000}$th of an inch cube, about 10,320,000,000,000.

If this gas were a mixture of two volumes of hydrogen and one of oxygen, on combining to form water there would be a contraction to $\frac{2}{3}$, and on condensing into liquid water a contraction to $\frac{1}{770}$ of this; but since the molecules of the resulting liquid would contain three atoms of the gases, the total number of molecules of liquid water in a given volume would be $\frac{2}{3} \times 770 \times \frac{1}{3} =$ 385 times the number of atoms of a gas. This gives for the number of molecules of liquid water in $\frac{1}{1000}$th of an inch cube about 3,900,000,000,000,000.

As an illustration of a far more complex substance I take albumen, and calculating as well as one can from its chemical composition, and from the specific gravity of horn, I come to the conclusion that the diameter of the ultimate molecule of dry albumen is about 3·82 that of the molecules of water, and that $\frac{1}{1000}$th of an inch cube would contain about 71,000,000,000,000.

If such be the case, we must conclude that in the length of $\frac{1}{80000}$th of an inch (the smallest interval that could be distinctly seen with the microscope) there would be about 2,000 molecules of liquid water lying end to end, or about 520 of albumen. Hence, in order to see the ultimate constitution of organic bodies, it would be necessary to use a magnifying power of from 500 to 2,000 times greater than those we now possess. These, however, for reasons already given, would be of no use unless the waves of light were some $\frac{1}{2000}$th part of the length they are, and our eyes and instruments correspondingly perfect. It will thus be seen that, even with our highest and best powers, we are about as far from seeing the ultimate structure of organic bodies as the naked eye is from seeing the smallest objects which our microscopes now reveal to us. As an illustration, I have calculated that with our highest powers we are as far from seeing the ultimate molecules of organic substances as we should be from seeing the contents of a newspaper with the naked eye at the distance of a third of a mile; the larger and smaller types corresponding to the larger and smaller molecules of the organic and inorganic constituents.

3. *General Conclusions to be deduced from the above facts.*

When we come to the application of these principles to the study of living matter we are immediately led to feel how very little we know respecting some of the most important questions that could occupy our attention. As illustrations I do not think I can select better than the facts bearing on the size and character of minute germs, and on Darwin's theory of ultimate organised gemmules.

For the sake of simplicity I will take into consideration only the albuminous constituent of animals, using the term *albumen* in a sort of generic sense. Whatever be the special variety of this substance, it is so associated with water in living tissues, that in most, if not all, cases they would cease to live, if thoroughly dried. Much of the water is no doubt present simply as water mechanically mixed with the living particles; but it appears to me that we ought to look upon some portion as being in a state of *molecular combination*. The existence of such a state of union is clearly proved by the optical characters of various solutions of the same coloured substance. These are by no means such as would result from the mere mechanical diffusion of very minute particles of the solid substance in the liquid, and cannot be explained unless we suppose that the coloured substance is to some extent in the state of molecular combination with the solvent. This molecular affinity is also in some cases manifested by a swelling up of a solid substance when placed in certain liquids, even when perfect solution occurs to a very limited extent. Such a condition appears to be very characteristic of the living tissues of animals, and makes it very probable that the ultimate living particles are molecular compounds with water, and not molecules of free dry albuminous substances. So little is known of the true proportion of water thus combined, that the only course now open is to suppose, for illustration, that living albuminous matter contains half its volume of water mechanically mixed, and one-fourth of free albumen, united with one-fourth of molecularly combined water. On this supposition a sphere of such living matter $\frac{1}{1000}$th of an inch in diameter would contain the following number of molecules:—

Albumen	10,000,000,000,000
Water in molecular combination .	520,000,000,000,000
	530,000,000,000,000

The very small relative amount of dry matter in some living animals makes it probable that the molecularly combined water plays such an important part in their ultimate structure, that we may base our provisional conclusions on this total number of molecules.

Theory of Invisible Germs.

Calculating then from the various data given above, we may conclude that a spherical particle one-tenth the diameter of the smallest speck that could be clearly defined with our best and highest powers, might nevertheless contain no less than one million structural molecules. Variations in number, chemical characters, and arrangement would in such a case admit of an almost boundless variety of structural characters. The final velocity with which such particles would subside in air must be so slow that they could penetrate into almost every place to which the atmosphere has access.

Darwin's Theory of Pangenesis.

Full particulars of this theory will be found at p. 374 of vol. ii. of his work on the variation of animals. He nowhere gives any opinion as to the actual size of gemmules, or the number present in particular cases, but it appears to me interesting to consider how far the theory will hold good when examined from this more physical point of view.

For the sake of argument, I assume that gemmules on an average contain one million structural molecules of albumen and molecularly combined water. Variations in number, composition, and arrangement would then admit of an almost infinite variety of characters. On this supposition it would require a thousand gemmules to be massed together into a sphere, in order to form a speck just distinctly visible with our highest and best magnifying powers. By calculation I find that a single mammalian spermatozoa might contain so many of such gemmules, that, if one were lost, destroyed, or fully developed in each second, they would not be completely exhausted until after the period of one month. Hence, since probably a number are concerned in producing perfect fertilisation, we can readily understand why the influence of the male parent may be very marked, even after having been, as regards particular characters, apparently dormant for many years.

In a similar manner I calculate that the germinal vesicle of a mammalian ovum might contain enough gemmules for one to be destroyed, lost, or fully developed in each second, and yet the entire number not be exhausted until after a period of seventeen years, and the entire ovum might contain enough to last at the same rate for no less than 5,600 years.

These calculations are made on the supposition that the entire mass is composed of gemmules. Of this there is little probability; but still, even if a considerable portion of the ovum consists of completely formed material and of mere nutritive matter, it might yet contain a sufficient number of gemmules to explain all the facts contemplated by the theory of pangenesis. The presence of any considerable amount of such passive matter in spermatozoa would, however, be a serious difficulty in the way of the theory, unless indeed very many spermatozoa are invariably concerned in producing fertilisation.

Taking everything into consideration, it does not appear to me that any serious objection can be raised against pangenesis when examined from a purely physical point of view, as far as relates to the inheritance of a very complex variety of characters by the first generation, though there would have been may serious difficulties to contend with, if the ultimate atoms of matter had been very much larger than is indicated by the properties of gases.

When we come to apply similar reasonings to the second or following generations, we are compelled, along with Darwin, to conclude that gemmules have the power of producing other gemmules more or less closely resembling themselves, and of being collected together in the sexual elements, since otherwise the number that could be transmitted in a dormant state for several generations would be far too small to meet the requirements of the case.

Conclusion.

In my remarks I have made no endeavour to conceal our present ignorance of many very important questions connected with my subject. Want of the requisite data necessarily imparts a speculative character to many of my conclusions; but perhaps there is no more fruitful source of knowledge than to see and feel how little is accurately known, and how much remains to be learned.

THE TUFTED DEER OF CHINA

AMONG the many most valuable additions which Mr. R. Swinhoe has made to our knowledge of Chinese zoology, there are none more important than his discoveries in the deer-tribe. The Water Deer of Shanghai (*Hydropotes inermis*), first described in 1870, is one of the most interesting of these. It is of small size, without horns of any kind, and with long canine teeth present in the males only. In outward appearance it in these respects closely resembles the Musk Deer. Its colour is light chestnut, and the hairy coat is harsh. It is called the *Ke* and the *Chang* by the Chinese. Sir Victor Brooke has demonstrated that its skull differs in important points from that of *Moschus*.

Still more recently, in 1874, Mr. Swinhoe has described another small deer from the mountains near Ningpo, of much the same size as *Hydropotes;* it also resembles that genus in being hornless and possessing large canine tusks in the males. Mr. Swinhoe, in the "Proceedings" of the Zoological Society (1874, p. 452), writes as follows:—"My friend and correspondent, Mr. A. Michie, wrote me a letter dated Shanghai, December 19, 1873, as follows:—'I send another note to overtake the mail, to tell you I have just found a new deer from the Ningpo country. It is a dark iron-grey or pepper-and-salt colour, like some Scotch terriers, with white tips to its ears, square-built (that is, straight back and pointed hip), with very short tail. On its forehead is a thick black mane, like the bristles of a boar. . . . It has the lachrymal sinus, but not so large as the Muntjac; in size the beast about equals the Muntjac.'" An excellent figure accompanies this description. It was drawn from a skin sent by Mr. Michie to Mr. Swinhoe, who has named the animal *Lophotragus michianus*.

A living example of this species, the first ever brought to this country, has just reached the Zoological Gardens in Regent's Park. From this male specimen it can be seen that the drawing above referred to, made from the flat skin, excellently represents the figure of the animal, and is truthful in that it shows the canine teeth and the absence of horns. In the living specimen there is a pair of hair-covered tuberosities on the frontal regions, at the postero-lateral angles of the hairy head-tuft, but, as in the Giraffes, these have no horns upon them. Comparing this condition with that of *Elaphodus cephalophus*, also from China, described by M. A. Milne-Edwards, the intimate relation of *Lophotragus* to the Muntjacs (*Cervulus*) is evident; the series of gradual antler-reduction being in the following order:—*Cervulus*, *Elaphodus*, *Lophotragus*. Whether *Hydropotes*, or *Moschus*, or both are extremes of this series, remains to be proved; and it must be mentioned that it is not perfectly certain, though highly probable, that the above-described individual specimen of *Lophotragus* exhibits its highest degree of antler-development.

NOTES

SOME weeks since it was stated that the collection of fishes made by Mr. Francis Day, Inspector-General of Indian Fisheries, would be deposited in the New Indian Museum at South Kensington. It was offered to and accepted by the Secretary of State for India, but it was subsequently considered that neither the expense of bottles in which to exhibit them, nor of spirit for their preservation, could be rightly debited to the resources of India. Mr. Wood, the well-known artist, very liberally proposed, in exchange for the type collection, numbering about 1,200 species, to increase Mr. Day's plates in his work, the "Fishes of India," from 160 to 190, or to 1,140 figures. The Director of the Indian Museum in Calcutta hearing of this arrangement, proposed to the trustees that he should secure it at once on these terms, and we understand that he has been instructed to do so. It will doubtless render the Museum in Calcutta the most complete in Indian fishes in the world; but whether this collection finding a place in the British Museum might not have proved more beneficial to science we leave for the decision of our readers.

MR. WILLETT has just issued a report on the Sub-Wealden boring, stating that the bore-hole has been widened and lined to