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# I.—THE PRESIDENT'S ADDRESS.

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In selecting a subject for my address, it appeared to me more desirable to direct attention to some special questions more or less intimately connected with branches of science which I have perhaps studied more than most of those who are familiar with the general applications of the microscope, rather than to pass in review the many interesting communications that have been made to us during the past year. These have been of that varying character which it is so desirable for our Society to have. Several have treated on new apparatus, and on the improvement or improved use of older contrivances of different kinds, or on the methods to be employed in the examination of the microscope, and in testing its performances. We have also had a number of excellent papers on single objects of interest, both animal and vegetable, as well as others treating on more general and wider biological subjects. On the whole, I think we have good reason to congratulate ourselves on what has been brought before us. Time would not allow me to mention and discuss the various memoirs in detail, and also to lay before you a special subject which appears to me well worthy of consideration, viz. the relation between the limit of the powers of the microscope and the ultimate molecules of organic and inorganic matter. At all events, I think that this subject may lay claim to sufficient novelty; since, so far as I have been able to learn from consulting the index of the various volumes, no one during the last fifteen years has treated on this question; and until within the last few years none of the requisite data were known. Even now many of them are so imperfect, that nothing more can be done than to make the most probable assumptions. This necessarily imparts more or less of a speculative character to some parts of the subject, but I hope this will be pardoned on such an occasion as the present. It appears to me that in his annual address, the President of a society cannot do better than endeavour to point out the bearing of what is already known on some great question; and if in doing this the necessity of more accurate knowledge is made apparent, there is more hope for the future. The importance of particular classes of facts may not, and very often is not, apparent until their connection with some special question is fully appreciated. It will, I am sure, be a source of great satisfaction to me if what I shall say should lead to the more accurate study of some of the data necessary to change my suppositions into well-established conclusions, whether they agree with my own or not.

Though fully impressed with the imperfect state of our present knowledge of the ultimate constitution of organic matter, yet even now the facts are sufficiently definite to indicate, if not indeed 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 human eye. I think we may very fairly conclude that the ultimate structure, even of organic bodies, will for ever be invisible, and the only chance of obtaining some knowledge respecting it is by indirect methods of research. For my own part, I look forward with hope and confidence to a great increase in our knowledge of this question by the further study of the optical characters of both organic and inorganic substances, that is to say, by using light so that it may suffer changes easily appreciated by our organs of vision, though the ultimate molecules of the object examined may be so small in relation to the wave-length of light, that even light itself is *far too coarse a means* for transmitting to our eyes any distinct impression of actual form or magnitude. There are also other branches of physical science which serve to teach much in connection with this subject, but as yet even these fail to satisfy all the requirements of the case. The whole question is beset with the greatest difficulties, and even when we make use of the best data hitherto obtained, we see at once how very imperfect they are. One reason perhaps is that the importance of the subject has not been sufficiently appreciated, and comparatively little has been done to develop it even as far as is possible. I think I may safely say that what has been done relates exclusively to the elementary substances, or to the most simple chemical compounds. Nothing, or next to nothing, is known respecting the size and structure of the molecules of the very complex substances met with in animals and plants, and when we come to consider what may be their ultimate nature when forming a part of living tissue, we are immediately brought face to face with questions which have probably never once attracted the attention of physicists; since as a rule their studies do not lead them into the consideration of biological problems.

I propose to discuss my subject under three heads:

1. The limit of the powers of the microscope.

2. The size of the ultimate molecules of organic and inorganic matter.

3. Conclusions to be drawn from the general facts.

## 1. *Limit of the Powers of the Microscope.*

In treating this question I have no intention to enter into the consideration of the best form or arrangement of lenses to ensure the least possible amount of spherical or chromatic aberration, nor how far for the purposes of research it is desirable to make a compromise between those practical difficulties which cannot all be entirely overcome at one time. I shall assume that the instrument itself is theoretically perfect, and consider only the limit of vision due to the organization of our own eyes, and still more that due to the physical characters of light.

The visibility of a very minute object necessarily depends on a number of different circumstances. If examined by transmitted light it must either absorb sufficient to make the contrast between it and the general field great enough for the eye to recognize, or it must be of such a shape and of such a refractive power in relation to the surrounding medium as to bend the light which passes near the edges out of the general direction of the transmitted beam, so as to give rise to a sufficiently dark and definite outline. In my treatment of the question, I however assume that the character of the object examined is in every respect such as would enable us to see it, if it were not for difficulties of another kind.

The purely physiological part of the question has not attracted much of my attention, since I did not believe that the ultimate limit of distinct vision would be found to depend on the constitution of the eye. It may, however, be well to give a short account of some experiments made by Dr. Royston-Pigott with the view to determine the physiological limit, which he has kindly communicated to me, and permitted me to employ, in order to show that the above-named conclusion is justified by experiment. He found that the smallest visual angle that he could ever distinctly appreciate was a hole $1\frac{1}{4}$ inch in diameter at a distance of 1100 yards, which corresponds to about 6″ of arc. This visual arc in a microscope magnifying 1000 linear would correspond to about the three-millionth part of an inch. Some persons, however, affirm that the smallest visible angle is 1′, or ten times the above, which would correspond to $\frac{1}{300000}$ of an inch. If such be the case, the eye could distinguish with a high magnifying power a much smaller interval than the physical properties of light will permit.

Taking into consideration merely the swelling out of a minute point of light due to diffraction, Dr. Royston-Pigott thinks that the limit of visibility must be from $\frac{1}{150000}$ to $\frac{1}{200000}$ of an inch. This, however, is not what appears to be the most important character of light in limiting the power of the microscope for separating lines so near together that they may be obscured or their number falsified by interference fringes.

This subject has been treated of in a very complete and satisfactory manner by Helmholtz,* whose authority on such a question few of us would venture to dispute. In his essay he maintains that the size of the smallest objects visible does not depend simply on their size, but very much on the susceptibility of the eye for faint differences in the intensity of light. For this reason the ultimate defining power of the microscope cannot be so well determined by the examination of single bright points or lines on a dark ground, or of single dark points or lines on a white ground, as by the use of fine gratings, which have alternate bright and dark stripes, as on Nobert's test-plate, and on the frustules of Diatomaceæ and the scales of insects. He contends that in the case of such objects the smallest distance that can be accurately defined depends upon the interference of the light passing, as it were, through the centres of the bright spaces, and that when this interference is of such a character that bright fringes are produced at the same intervals as the dark lines, and are superimposed on them, the lines can be no longer seen, and the normal limit of perfect definition has been reached. He, however, points out that by a favourable overlapping the dark portions of the fringes may occasionally so coincide with the true lines as to make it possible to see still smaller intervals, but that a certain and unequivocal perception of such lines would scarcely be possible. He then proceeds to show that this limit of true and distinct vision depends upon the angle of divergence of the light entering the object-glass of the microscope, and on the wave-length of the light, according to the following relations:

$d$ = the distance between the lines;
$\alpha$ = the angle of divergence;
$\lambda$ = the length of the wave of the light;

then we have

$$d = \frac{\lambda}{2 \sin. \alpha}.$$

This angle of divergence is equivalent to one-half of the true angle of aperture, when illuminated by an equally large pencil of light; but at the same time one cannot but think that in actual practice the results must be made somewhat more complex, owing to the presence of light having a less angle of divergence than the extreme. All the calculations are also made for true focal adjustment and correction of the lenses, and if these be not actually correct the combined effect of all the disturbing causes must necessarily give rise to many appearances not easily explained. Of course these remarks do not in any way apply to minute bright points.

The formula given by Helmholtz is entirely different from that

* Poggendorff's 'Annalen,' Jubelband, 1874, p. 573.

adopted by Nobert, which is based on the supposition that the rays of light used for illumination are parallel, and entirely ignores the question of aperture. As Dr. Woodward has shown,* the limit given by Nobert's formula is not at all borne out by observation, since lines can be distinguished at a much smaller interval than indicated by the manifestly incomplete theory. This remark will not apply in the case of Helmholtz's formula, which appears to be fully substantiated by observation.

Adopting, then, the most simple applications of Helmholtz's formula as an illustration of the general question, I have calculated what is the limit for the red and blue ends of the spectrum, and for the mean rays, according to the following wave-lengths, given for simplicity in fractions of an inch:

| | |
|---|---|
| Red end .. .. .. .. .. .. .. | $\frac{1}{37350}$ |
| Mean rays .. .. .. .. .. .. .. | $\frac{1}{46180}$ |
| Blue end .. .. .. .. .. .. .. | $\frac{1}{60470}$ |

I have also calculated the limit for a few widely different angles of divergence, giving double these in order to make the comparison more simple with the angle of aperture as usually expressed, assuming of course that the angle of divergence of the light from the condenser is equally great.

60°, which gives the wave-length as the limit.
97°, which gives three-fourths of the wave-length as limit.
120°.
150°.
180°, or an angle so great that its sine is near unity, whether practicably possible or not. This gives for the limit half the wave-length of the light.

The results are expressed in the following table, in which I give the nearest round numbers:

| | 60° | 97° | 120° | 150° | 180° |
|---|---|---|---|---|---|
| Red end .. .. | $\frac{1}{37000}$ | $\frac{1}{55000}$ | $\frac{1}{64000}$ | $\frac{1}{71000}$ | $\frac{1}{74000}$ |
| Mean rays .. .. | $\frac{1}{46000}$ | $\frac{1}{69000}$ | $\frac{1}{80000}$ | $\frac{1}{89000}$ | $\frac{1}{92000}$ |
| Blue end .. .. | $\frac{1}{60000}$ | $\frac{1}{90000}$ | $\frac{1}{104000}$ | $\frac{1}{116000}$ | $\frac{1}{120000}$ |

All these limits are calculated for dry lenses. For immersion lenses *of equal aperture* the limits would in all cases be about three-fourths of the various magnitudes here given. In order to see such minute intervals, of course a high magnifying power is necessary, but when the interval is less an increased power would magnify the defects and the object equally.

* 'Monthly Microscopical Journal,' vol. ii., 1869, p. 289.

An examination of the table will clearly show the value of a large aperture in defining lines at very small intervals on flat objects like Diatomaceæ, though in practice this advantage may be entirely counterbalanced by other disadvantages in the case of a different class of objects. The largest possible aperture would define lines at half the distance apart that could be defined with an aperture of only 60°. It follows from the law of the sine that there would be a rapid increase in defining power on increasing the aperture when small, but when large a similar increase would have no such corresponding advantage. Mr. Jabez Hogg informs me that by a comparison of different object-glasses he has been led to conclude that the defining power varies as the chord of the aperture, which of course is in absolute agreement with this theory of Helmholtz. It is the same, only expressed in different words. Of course the defining power of different object-glasses depends on several other circumstances; but since we find that many of the facts may be explained by the action of the interference fringes, depending on the essential characters of light itself, no matter how perfect the manufacture of the instrument or the capabilities of the eye, it appears to me that they deserve far more consideration than has been given to them. Their influence has been entirely overlooked by many who have treated on this question. At all events, since they are altogether independent of the mechanical construction of the instrument, it appears to me that we cannot do better than adopt these principles in forming some conclusion as to the size of the smallest object that could be distinctly seen with a *theoretically perfect microscope.* Looked at from this point of view alone, *with a dry lens* this could not be less than $\frac{1}{80000}$ of an inch. Even when $\frac{1}{74000}$ the fringes due to the extreme red rays would begin to produce partial obscurity, and at $\frac{1}{92000}$ the brightest part of the spectrum would make the obscurity more or less complete. If it were possible to make use of the blue end alone, lines of $\frac{1}{110000}$ could still be seen, since their shorter waves would not produce obscurity until the size was reduced to $\frac{1}{120000}$ of an inch. The size of the smallest bright point that could be seen depends on entirely different considerations, and might be considerably less, as far as the physical constitution of light is concerned.

The question now arises, Are these general conclusions borne out by actual observation? As far as I am able to judge from such evidence as I have been able to collect, they are very strongly confirmed, if not actually established. Thus, according to Helmholtz, Dippel* found that the limit of the true resolution of Nobert's lines was about $\frac{1}{90000}$ of an inch, which is just within the limit for the mean rays, with a very wide aperture. By theory this limit might be considerably exceeded by the use of blue light; and, since the

* 'Das Mikroscop und seine Anwendung,' 1867.

rays at the blue end of the spectrum are those which are active in photographing, it might be possible to obtain a good photograph of lines not distinctly visible when mixed light is employed. This, Helmholtz thinks, explains why Stinde was able to photograph lines on *Surirella gemma* which were $\frac{1}{100000}$ of an inch apart, and therefore considerably within the possible limit. Helmholtz does not appear to have seen the papers on Nobert's bands by Stodder * and by Dr. Woodward,† which contain many facts of great interest in connection with this subject.

In reading these papers it is easy to perceive that the true resolution of one of Nobert's bands, which according to Dr. Woodward contains lines at a distance of about $\frac{1}{112000}$ of an English inch, is a matter of such extreme difficulty, even with the best object-glasses, that, if the exact nature of the object and the number of lines were not known, it would be almost impossible to decide how many lines there were to an inch. The lines due to interference are often as distinct as the true lines on the glass, and Dr. Woodward believes that such spurious lines, and not the actual, were seen and counted by Stodder; since the number was not correct. The black lines due to interference do occur beyond the limit of the true, and at closely the same intervals as the real, as should be the case according to Helmholtz's theory. Now it is quite manifest that the distinctness of definition depends on how these spurious bands occur in relation to the true. If they exactly overlap, the definition would be good, and the lines distinct; but, if they occurred at the half intervals, the dark part of one series occurring at the bright part of the other would more or less completely obliterate both. It appears to me very probable that these facts will in great measure explain the phenomena seen when the light is thrown on the lines at a varying angle, since in one position the lines cannot be defined, on increasing the obliquity false lines are visible, and with still more oblique light the true may be seen. An alteration in the angle of aperture of the condenser would also alter the distance of the diffraction bands; and therefore, taking all these facts into consideration, we may easily explain why, as Helmholtz says, it is possible under such favourable conditions, with lines at equal intervals, to distinguish them when closer together than what is the normal limit of the distance at which they can be seen without any special difficulty, even when not at equal intervals, that is to say, when the intervals are greater than that of the bands due to diffraction. Even then, however, they do occur in varying numbers and position between the true lines, as may be seen in photographed diffraction gratings.

* 'Quart. Journ. of Micros. Science,' 1868, vol. viii., p. 133.

† Ibid., p. 225; 'Monthly Microscopical Journal,' 1871, vol. vi., p. 26; and 1872, vol. viii., p. 227.

Still, even the above-named Nobert's band is quite within the limits of what might be resolved by the use of blue light, and thus there is no difficulty in understanding how it might be photographed as done by Dr. Woodward.

Similar principles would of course apply in the case of the very close and uniform markings on the frustules of Diatomaceæ. Dr. Woodward's paper and admirable photographs of *Frustulia Saxonica*, brought before our Society at our meeting last November,* fully bear out all Helmholtz's conclusions, and show the difficulty of distinguishing true structure from interference fringes when the intervals between the real markings are of the same order of magnitude as half the length of the waves of light. This effect is of course altogether independent of the quality of the lenses. It depends on the physical constitution of light itself, and would only be the more perfectly seen with more perfect object-glasses.

There is also another fact mentioned by Dr. Woodward which merits attention.† He says that for resolving very close lines or linear markings it is a decided advantage to have the lenses somewhat under-corrected for colour. As he suggests, this may be partly due to the possibility of making such lenses more correct for spherical aberration, but at the same time it appears to me quite possible that it may also to some extent be due to the fact that with such a correction it is possible so to have the lines in focus for the blue rays as to take advantage of their shorter wave-length, whilst the interference fringes due to the longer waves are sufficiently modified by being out of focus as to obscure the vision less than they otherwise would.

Taking then all these facts into consideration, it appears to me extremely probable that for object-glasses not made on the immersion principle the limit of perfectly satisfactory definition of lines not exactly the same distance apart must be somewhere about $\frac{1}{80000}$ of an inch. With a dry lens having an aperture of 140°, or an immersion of 100°, both illuminated by a condenser of equal angle, only the extreme red rays would then serve to produce a very slight indistinctness. Under very favourable circumstances by varying the angle of divergence of the light passing from the condenser, or by throwing it more from one side than from the opposite, it would be possible to make the dark interference fringes so coincide with dark structural lines that a considerably smaller interval might be distinguished. This, however, would be extremely difficult if not impossible, if the lines were at unequal intervals, since any adjustment of the illumination that gave interference fringes at the proper interval and situation for one part of

* 'Monthly Microscopical Journal,' 1875, vol. xiv., p. 274.

† 'Quart. Journ. of Micros. Science,' viii., p. 229.

the object would give them at such an interval and situation as would obscure the structural lines in another part, and by no single adjustment could the whole be seen correctly, but in all cases true and spurious lines would be mixed up together. The only chance of arriving at a true knowledge of the real structure would be by a careful induction from the facts observed when the illumination is made to vary; and even when a satisfactory conclusion could thus be drawn it would only be by acting on the principle that the limits of simple and distinct visibility had been passed, when light has to be treated as an agent scarcely fitted for the requirements of the case.

When we come to the examination of single detached particles the conditions are materially changed, but if the bright part of the interference fringes fall on the dark boundary line of a transparent particle or the bright part of a fringe on the centre of an opaque particle, it could not be distinctly seen though its presence might be recognized.

The limit of $\frac{1}{80000}$ of an inch deduced on Helmholtz's principle from the physical characters of light agrees admirably with the estimate formed independently by various great authorities on the microscope. The mean of the estimate thus formed by Quekett, Ross, De la Rue, and Carpenter, as quoted by Stodder, is in fact exactly the same ($\frac{1}{80000}$ of an inch), so that we cannot, I think, be far from the truth, if we take that as the base on which to build further conclusions. With an immersion object-glass of very large aperture it might be possible to define an interval of somewhat less than $\frac{1}{100000}$ of an inch, but probably the above-named determinations were made with dry lenses. At all events, since the limit of visibility as determined by the use of the best modern microscopes agrees so completely with what appears to be the limit due to the physical constitution of light, we must, I think, conclude that our instruments do now enable us to see intervals so small in relation to the wave-length of light, that we can scarcely hope for improvement *as far as the mere visibility of minute objects is concerned*, whatever may remain to be done to improve their performances in other respects.

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

Having then come to the conclusion that the limit of distinct and unequivocal definition is somewhere about from $\frac{1}{80000}$ to $\frac{1}{100000}$ of an inch, it appears to me very desirable to consider what relation such a magnitude bears to the size of the ultimate atoms of organic and inorganic matter. From the very nature of the case the microscope altogether fails to throw any light on this question, and the only course as yet open to us is to draw the best conclusions we can from the various properties of gases. This problem has been attacked by Stoney,* Thomson,† and Clerk-Maxwell,‡ who, from various data, and by various methods of reasoning, have endeavoured to determine the number of ultimate atoms in a given volume of any permanent and perfect gas. In order to avoid inconveniently long rows of figures, I have reduced all their results to the number of ultimate atoms contained in a space of $\frac{1}{1000}$ of an inch cube, that is to say, in $\frac{1}{1000000000}$ of a cubic inch, at 0° C. and a pressure of one atmosphere. These numbers are as follows:

| | |
|---|---|
| Stoney .. .. .. .. .. .. .. | 1,901,000,000,000 |
| Thomson .. .. .. .. .. .. | 98,320,000,000,000 |
| Clerk-Maxwell .. .. .. .. .. | 311,000,000,000 |
| Mean .. .. .. .. .. .. .. | 50,260,000,000,000 |

As will be seen, there is a very great discrepancy between the numbers given by Thomson and Clerk-Maxwell. This is in part due to the fact that Thomson gives the greatest probable number, whilst Clerk-Maxwell has endeavoured to express the true number indicated by the phenomena of inter-diffusion of gases. The determinations do to a great extent depend on the measurements of length, and any differences are of course greatly increased when the number of atoms in a given volume is calculated, since that varies as the cube of the linear dimensions. Extracting the cube root of each of the above numbers, we obtain the number of atoms that would lie end to end in the space of $\frac{1}{1000}$ of an inch in length. These are as follows:

| | |
|---|---|
| Stoney .. .. .. .. .. .. | 12,390 |
| Thomson .. .. .. .. .. .. | 46,160 |
| Clerk-Maxwell .. .. .. .. .. | 6,770 |
| Mean .. .. .. .. .. .. .. | 21,770 |

The cube of this mean is about 10,317,000,000,000, and, taking into consideration the various circumstances named above, it appears to me a far more probable approximation to the truth than the mean of the numbers in a cubic $\frac{1}{1000}$ of an inch as given by the authors. As will be apparent from the wide differences, even this mean result can be looked upon in no other light than a very rough approximation; but still, when we bear in mind that Thomson's result is given as a limit, it must be admitted that the numbers belong sufficiently to one general order of magnitude to justify our looking upon the mean as a tolerably satisfactory ground on which to form some provisional conclusions.

* 'Philosophical Magazine,' 1868, vol. xxxvi., p. 132.
† 'Nature,' March 31, 1870, vol. i., p. 551.
‡ Ibid., August 11, 1873, vol. viii., p. 298.

Now, if the gas containing the above-named number of atoms consisted of two volumes of hydrogen to one volume of oxygen, when combined to form vapour of water there would be a condensation of volume from three to two, and on condensing into a liquid a further contraction to $\frac{1}{770}$ of the bulk of the vapour. Each molecule of water would however consist of three atoms of gas, and hence in order to determine the number of molecules of liquid water in $\frac{1}{1000}$ of an inch cube, it is necessary to multiply the number in a gas by $\frac{3}{2} \times 770 \times \frac{1}{3} = 385$. This gives for the number of molecules of water in $\frac{1}{1000}$ inch cube about 3,972,000,000,000,000. In this and all other cases I give round numbers, since any nearer approximation is impossible.

Though living organisms contain much water, yet far more complex substances enter into their composition. As an example of one of these, we may take albumen. According to Lieberkühn its composition is expressed by the formula $C_{72}H_{112}N_{18}SO_{22}$. It therefore contains seventy-one times as many ultimate atoms as water, and its atomic weight is about eighty-two times that of water. In the condition of horn I find that its specific gravity is about 1·31. Calculating from these data, I conclude that when the various constituents combine they contract to $\frac{9}{10}$ of the total volume, and not as water to $\frac{2}{3}$; and that the volume of a single molecule of albumen is about 55·6 that of a molecule of liquid water. If their form be similar, their diameter must therefore be 3·82 times that of a molecule of water. This would lead us to conclude that in a cube of $\frac{1}{1000}$ of an inch of horn there are about 71,000,000,000,000 molecules of albumen.

According then to these principles there would be in the length of $\frac{1}{80000}$ of an inch about 2000 molecules of water, or 520 of albumen, and hence, in order to see the ultimate constitution of organic bodies, it would be necessary to use a magnifying power of from 500 to 2000 times greater than those we now possess. These, however, for the reasons already given, would be of no use unless the waves of light were some $\frac{1}{2000}$ 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 constitution of organic matter as the naked eye is from seeing the smallest objects which they now reveal to us. Nor does there appear to be much hope that we ever shall see the ultimate constituents, since light itself is manifestly of too coarse a nature, even if it were possible to still further develop our optical resources. As matters now stand we are about as far from a knowledge of the ultimate structure of organic bodies as we should be of the contents of a newspaper seen with the naked eye at a distance of a third of a mile, under which circumstances the letters of various sizes would correspond to the smaller and larger ultimate molecules. This being the case, we may feel persuaded that particles of organic matter, like the spores of many living organisms scarcely visible with the highest magnifying powers, and, if seen, quite undistinguishable from one another, might yet differ in an almost infinite number of structural characters, just as any number of different newspapers in various languages or with varying contents would look alike at the distance of a third of a mile.

### 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—questions which certainly never presented themselves to *me*, until I looked upon them from this point of view, and which perhaps have not occurred to anyone before. As illustrations of the subject now under consideration, 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 organized gemmules, as described in Part ii. chapter xxvii. of his work on the variation of animals and plants under domestication. So far as I have been able to learn, he has nowhere given any opinion as to the probable *size* of such gemmules, nor discussed the probability of some of his speculations when examined from a *numerical* point of view, and in connection with the probable size of the *ultimate molecules* of organized matter. I therefore propose to do so; since, though not actually a microscopical question, it is most intimately connected with our studies, and as microscopists I think we have a good claim to investigate objects that are just beyond our magnifying powers.

For the sake of simplicity I will take into consideration only the albuminous constituents of animals, using the term albumen in a sort of generic sense, to include many compounds, which differ in many particulars, and yet have many in common. With slight modifications the same principles would apply in the case of other substances. Whatever be the special variety of this constituent, it is so associated with water in living tissues that in most, if not in all, cases they would cease to live if thoroughly dried. This is exemplified by the case of hair and horn, which must contain much water at the growing end, but are dead where hard and dry. In living tissues much of the water is no doubt present simply as a liquid 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*. So little attention has been directed to this kind of weak affinity, that its very existence is almost or quite ignored in many large and important chemical works, and yet probably many of the phenomena of life are manifested only by such compounds. Very much light is thrown on this question by the study of the spectra and other optical characters of coloured substances. These clearly prove that when dissolved in any liquid the optical properties of the solution depend in part on the nature of the solvent, and are by no means the same as they would be if minute particles of the solid substance were diffused in the liquid. These facts cannot, I think, be explained unless we conclude that the solvent is to some extent in the state of molecular combination with the substance dissolved. This molecular affinity is also in some cases manifested by a swelling up of a solid substance when placed in some 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 sufficiently probable that the ultimate living particles are molecular compounds with water, and not molecules of free dry albuminous substances.

Unfortunately, nothing definite is known respecting this question, and all that can now be done by way of illustration is to make some sort of a probable supposition. Taking everything into consideration, it appears to me that, as a reasonable example, we may assume that living albuminous tissue contains one-half of its volume of water mechanically mixed, and one-fourth its volume of free albumen united molecularly with an equal volume of water. On this supposition the number of molecules in $\frac{1}{1000}$ of an inch cube would be about

| | |
|---|---|
| Albumen .. .. .. .. .. .. .. | 18,000,000,000,000 |
| Water in molecular combination .. .. | 992,000,000,000,000 |
| | 1,010,000,000,000,000 |

Since, however, the form of minute living organisms more nearly approximates to spheres than to cubes, it will be more convenient to give the numbers in a sphere of $\frac{1}{1000}$ of an inch in diameter. For this there would be about as follows:

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

In the present state of our knowledge it is perhaps impossible to say whether or not the essential characters of living particles are due to the structural arrangement of the molecules of this combined water as well as of those of the albumen, and whether or not in considering the possible variations in structure the total number of molecules should be taken into account. The very small relative amount of dry matter in some living animals does, however, make it very probable that molecularly combined water really plays a part in their structure; and on the whole we may, I think, base our provisional calculations on the total number of molecules given above.

### *The Theory of Invisible Germs.*

The relation between the size of the smallest object that can be seen, and that of the ultimate molecules of living matter, is manifestly a question of great importance in connection with the theory of germs. If the ultimate molecules were much larger than they appear to be, there would be serious objections to the theory; but, as far as we can judge, they are sufficiently small to make it possible for an almost endless variety of germs to exist, each having a distinct structural character, and yet each so small that there is no probability of our ever being able to see them, even as indefinite points. Thus, according to the principles described above, a sphere of organized matter one-tenth of the diameter of the smallest particle that could be clearly defined with our highest powers, might contain a million molecules of albumen and molecularly combined water. Variations in number, chemical character, and arrangement, would in such a case admit of an almost boundless variety of structural characters. The final velocity with which such extremely minute 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.*

Darwin's theory of pangenesis is an attempt to give something like a reasonable explanation of the phenomena of inheritance, and is not necessarily connected with the question of the evolution of new species. A full account of the theory will be found in his work on the variation of animals. At p. 374 of vol. ii. he says that "he assumes that cells before their conversion into completely passive or formed material, throw off minute granules or atoms, which circulate freely throughout the system, and when supplied with proper nutriment multiply by self-division, subsequently becoming developed into cells like those from which they were derived. These granules for the sake of distinctness may be called cell-gemmules, or, as the cellular theory is not fully established, simply gemmules. They are supposed to be transmitted from the parents to their offspring, and are generally developed in the generation which immediately succeeds, but are often transmitted in a dormant state during many generations, and are then developed. Their development is supposed to depend on their union with other partially developed cells or gemmules which precede them in the regular course of growth.

Gemmules are supposed to be thrown off by every cell or unit, not only during the adult state, but during all the stages of development. He assumes that the gemmules in their dormant state have a mutual affinity for each other, leading to their aggregation into buds or into the sexual elements. These assumptions constitute the provisional hypothesis which he calls Pangenesis."

In order to form some opinion as to whether the ultimate molecules of organic matter are of such a size as to make this theory possible or probable, it is necessary to form some idea as to the number of such molecules that may be united to make one gemmule. It must be very considerable, or else it seems difficult to understand how they could vary enough to explain the inheritance of many characters. Perhaps, for the sake of argument, we may assume that on an average each contains something like a million. Varying numbers, chemical constitution, and arrangement, would in such a case allow of an almost infinite variety; but of course we are so profoundly ignorant of many necessary details that this number can be looked upon only as a rough illustration of the application of a general method of study. On this supposition one thousand such gemmules massed together would form a sphere just distinctly visible with our highest and best magnifying powers. If the gemmules were of much greater or of much less magnitude, it appears to me very probable that Darwin's theory would break down from two opposite causes, or would need very considerable modification, because, if much greater, their number would be too few to transmit sufficiently varied characters, and, if much less, they could scarcely contain enough of the ultimate atoms of matter to have a sufficiently varied individual character to transmit, since of the assumed million ultimate molecules only eighteen thousand would be of a true protoplasmic nature, the rest being of water in molecular combination.

Adopting, then, this size as a basis for calculation, it is easy to form some opinion as to the number of gemmules that might be present in spermatozoa or in ova, assuming them to be their sole or chief constituent. Thus, for example, if we take $\frac{1}{6000}$ of an inch as the mean diameter of a single mammalian spermatozoon, it might contain two and a half millions of such gemmules. If these were lost, destroyed, or fully developed at the rate of one in each second, this number would be exhausted in about one month; but, since a number of spermatozoa appears to be necessary to produce perfect fertilization, it is quite easy to understand that the number of gemmules introduced into the ovum may be so great that the influence of the male parent may be very marked, even after having been, as regards particular characters, apparently dormant for many years.

Then, again, adopting $\frac{1}{1000}$ of an inch as the mean diameter of the germinal vesicle of a mammalian ovum, it might contain above five hundred millions of gemmules. If these were lost or fully developed at the rate of one in each second, this number would not be exhausted until after a period of seventeen years. There would thus be no difficulty in understanding why the characters of the female parent might remain during life, even though apparently dormant for many years. This is still more the case if we take into consideration the entire ovum, since calculating on the supposition of its being a sphere $\frac{1}{150}$ of an inch in diameter it might contain so many gemmules that if one were lost or developed in each second they might not all be exhausted until after 5600 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 may 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 the spermatozoa would certainly be a serious difficulty in the way of the theory, unless indeed a very considerable number are invariably concerned in producing fertilization.

When, however, we come to apply similar reasoning to the inheritance by the second or following generations of characters which have remained apparently dormant in one or more previous generations, it appears to me that the gemmule theory would fail, unless gemmules have the power of reproducing others more or less closely resembling themselves, and of collecting together more especially in the sexual elements. This will, I think, be apparent from the following considerations.

An animal weighing 8 stones would contain about 3000 cubic inches, and thus its entire volume would be about six millions of millions times that of the germinal vesicle of an ovum. Hence, if the number of gemmules in a vesicle as given above were present in the grown-up animal and equally distributed over the whole body, there would only be enough to allow one for each thousand ova, or only one for a much greater number of spermatozoa.

I have treated this question entirely in its *physical* aspect, and made no reference to any other class of facts. The conclusions to which I have been thus led agree remarkably well with those of Darwin, though drawn from entirely different data. As will be seen, the probable size of the ultimate molecules of living matter is sufficiently minute to make the gemmule theory possible when examined from a purely physical point of view. If there had been good evidence to prove that the ultimate atoms of matter are very much larger than indicated by the properties of gases, the gemmule theory could scarcely have been maintained, since the possible number of gemmules that could have been present in the germinal vesicle or spermatozoa would not have been adequate to explain the various facts of inheritance.

### *Conclusion.*

As I have pointed out in the course of my remarks, there is still unfortunately very much doubt respecting many most important questions connected with this subject, and therefore my conclusions can be looked upon only as a first attempt to apply a physical kind of argument to various biological speculations. Even if our present knowledge is inadequate to make this attempt satisfactory, I trust that what I have said will be sufficient to show the need of a more complete study of the various questions to which I have directed attention. I hope myself to study them much more fully as soon as circumstances will permit. Such an inquiry at all events serves to show how very little is yet known respecting some of the most important facts connected with the phenomena of life, and 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.