



# XXIV. Additional remarks on active molecules

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S E P T E M B E R 1829.

XXIV. *Additional Remarks on Active Molecules.* By ROBERT BROWN, F.R.S., Hon. M.R.S.E. & R.I. Acad., V.P.L.S., Corresponding Member of the Royal Institutes of France and of the Netherlands, &c. &c.\*

ABOUT twelve months ago I printed an account of Microscopical Observations made in the summer of 1827, on the Particles contained in the Pollen of Plants; and on the general Existence of active Molecules in Organic and Inorganic Bodies.

In the present Supplement to that account, my objects are, to explain and modify a few of its statements, to advert to some of the remarks that have been made, either on the correctness or originality of the observations, and to the causes that have been considered sufficient for the explanation of the phænomena.

In the first place, I have to notice an erroneous assertion of more than one writer, namely, that I have stated the active Molecules to be animated. This mistake has probably arisen from my having communicated the facts in the same order in which they occurred, accompanied by the views which presented themselves in the different stages of the investigation; and in one case, from my having adopted the language, in referring to the opinion, of another inquirer into the first branch of the subject.

Although I endeavoured strictly to confine myself to the statement of the facts observed, yet in speaking of the active Molecules I have not been able, in all cases, to avoid the introduction of hypothesis; for such is the supposition, that the equally active particles of greater size, and frequently of very different form, are primary compounds of these Mole-

\* Communicated by the Author:—Mr. Brown's former paper on this subject, will be found in *Phil. Mag. and Annals*, N.S. vol. iv. p. 161.

cules,—a supposition which, though professedly conjectural, I regret having so much insisted on, especially as it may seem connected with the opinion of the absolute identity of the Molecules, from whatever source derived.

On this latter subject, the only two points that I endeavoured to ascertain, were their size and figure: and although I was, upon the whole, inclined to think that in these respects the Molecules were similar from whatever substances obtained, yet the evidence then adduced in support of the supposition was far from satisfactory; and I may add, that I am still less satisfied now that such is the fact. But even had the uniformity of the Molecules in those two points been absolutely established, it did not necessarily follow, nor have I any where stated, as has been imputed to me, that they also agreed in all their other properties and functions.

I have remarked, that certain substances, namely, sulphur, resin, and wax, did not yield active particles, which, however, proceeded merely from defective manipulation; for I have since readily obtained them from all these bodies: at the same time I ought to notice that their existence in sulphur was previously mentioned to me by my friend Mr. Lister.

In prosecuting the inquiry subsequent to the publication of my Observations, I have chiefly employed the simple microscope mentioned in the Pamphlet, as having been made for me by Mr. Dollond, and of which the three lenses that I have generally used, are of a 40th, 60th, and 70th of an inch focus.

Many of the observations have been repeated and confirmed with other simple microscopes having lenses of similar powers, and also with the best achromatic compound microscopes, either in my own possession or belonging to my friends.

The result of the inquiry at present essentially agrees with that which may be collected from my printed account, and may be here briefly stated in the following terms: namely,

That extremely minute particles of solid matter, whether obtained from organic or inorganic substances, when suspended in pure water, or in some other aqueous fluids, exhibit motions for which I am unable to account, and which from their irregularity and seeming independence resemble in a remarkable degree the less rapid motions of some of the simplest animalcules of infusions. That the smallest moving particles observed, and which I have termed Active Molecules, appear to be spherical, or nearly so, and to be between 1-20,000th and 1-30,000th of an inch in diameter; and that other particles of considerably greater and various size, and either of similar or of very different figure, also present analogous motions in like circumstances.

I have

I have formerly stated my belief that these motions of the particles neither arose from currents in the fluid containing them, nor depended on that intestine motion which may be supposed to accompany its evaporation.

These causes of motion, however, either singly or combined with others,—as, the attractions and repulsions among the particles themselves, their unstable equilibrium in the fluid in which they are suspended, their hygrometrical or capillary action, and in some cases the disengagement of volatile matter, or of minute air bubbles,—have been considered by several writers as sufficiently accounting for the appearances. Some of the alleged causes here stated, with others which I have considered it unnecessary to mention, are not likely to be overlooked or to deceive observers of any experience in microscopical researches: and the insufficiency of the most important of those enumerated, may, I think, be satisfactorily shown by means of a very simple experiment.

This experiment consists in reducing the drop of water containing the particles to microscopic minuteness, and prolonging its existence by immersing it in a transparent fluid of inferior specific gravity, with which it is not miscible, and in which evaporation is extremely slow. If to almond-oil, which is a fluid having these properties, a considerably smaller proportion of water, duly impregnated with particles, be added, and the two fluids shaken or triturated together, drops of water of various sizes, from 1-50th to 1-2000th of an inch in diameter, will be immediately produced. Of these, the most minute necessarily contain but few particles, and some may be occasionally observed with one particle only. In this manner minute drops, which if exposed to the air would be dissipated in less than a minute, may be retained for more than an hour. But in all the drops thus formed and protected, the motion of the particles takes place with undiminished activity, while the principal causes assigned for that motion, namely, evaporation, and their mutual attraction and repulsion, are either materially reduced or absolutely null.

It may here be remarked, that those currents from centre to circumference, at first hardly perceptible, then more obvious, and at last very rapid, which constantly exist in drops exposed to the air, and disturb or entirely overcome the proper motion of the particles, are wholly prevented in drops of small size immersed in oil,—a fact which, however, is only apparent in those drops that are flattened, in consequence of being nearly or absolutely in contact with the stage of the microscope.

That the motion of the particles is not produced by any cause acting on the surface of the drop, may be proved by an

inversion of the experiment; for by mixing a very small proportion of oil with the water containing the particles, microscopic drops of oil of extreme minuteness, some of them not exceeding in size the particles themselves, will be found on the surface of the drop of water, and nearly or altogether at rest; while the particles in the centre or towards the bottom of the drop continue to move with their usual degree of activity.

By means of the contrivance now described for reducing the size and prolonging the existence of the drops containing the particles, which, simple as it is, did not till very lately occur to me, a greater command of the subject is obtained, sufficient perhaps to enable us to ascertain the real cause of the motions in question.

Of the few experiments which I have made since this manner of observing was adopted, some appear to me so curious, that I do not venture to state them until they are verified by frequent and careful repetition.

I shall conclude these supplementary remarks to my former Observations, by noticing the degree in which I consider those observations to have been anticipated.

That molecular was sometimes confounded with animalcular motion by several of the earlier microscopical observers, appears extremely probable from various passages in the writings of Leeuwenhoek, as well as from a very remarkable Paper by Stephen Gray, published in the 19th volume of the Philosophical Transactions.

Needham also, and Buffon, with whom the hypothesis of organic particles originated, seem to have not unfrequently fallen into the same mistake. And I am inclined to believe that Spallanzani, notwithstanding one of his statements respecting them, has under the head of *Animalletti d'ultimo ordine* included the active Molecules as well as true Animalcules.

I may next mention that Gleichen, the discoverer of the motions of the Particles of the Pollen, also observed similar motions in the particles of the ovulum of *Zea Mays*.

Wrisberg and Muller, who adopted in part Buffon's hypothesis, state the globules, of which they suppose all organic bodies formed, to be capable of motion; and Muller distinguishes these moving organic globules from real Animalcules, with which, he adds, they have been confounded by some very respectable observers.

In 1814 Dr. James Drummond, of Belfast, published in the 7th volume of the Transactions of the Royal Society of Edinburgh, a valuable Paper, entitled "On certain Appearances observed in the Dissection of the Eyes of Fishes."

In this Essay, which I regret I was entirely unacquainted with

with when I printed the account of my *Observations*, the author gives an account of the very remarkable motions of the spicula which form the silvery part of the choroid coat of the eyes of fishes.

These spicula were examined with a simple microscope, and as opaque objects, a strong light being thrown upon the drop of water in which they were suspended. The appearances are minutely described, and very ingenious reasoning employed to show that, to account for the motions, the least improbable conjecture is to suppose the spicula animated.

As these bodies were seen by reflected and not by transmitted light, a very correct idea of their actual motions could hardly be obtained; and with the low magnifying powers necessarily employed with the instrument and in the manner described, the more minute nearly spherical particles or active Molecules which, when higher powers were used, I have always found in abundance along with the spicula, entirely escaped observation.

Dr. Drummond's researches were strictly limited to the spicula of the eyes and scales of fishes; and as he does not appear to have suspected that particles having analogous motions might exist in other organized bodies, and far less in inorganic matter, I consider myself anticipated by this acute observer only to the same extent as by Gleichen, and in a much less degree than by Muller, whose statements have been already alluded to.

All the observers now mentioned have confined themselves to the examination of the particles of organic bodies. In 1819, however, Mr. Bywater, of Liverpool, published an account of *Microscopical Observations*, in which it is stated that not only organic tissues, but also inorganic substances, consist of what he terms animated or irritable particles.

A second edition of this *Essay* appeared in 1828, probably altered in some points, but it may be supposed agreeing essentially in its statements with the edition of 1819, which I have never seen, and of the existence of which I was ignorant when I published my pamphlet.

From the edition of 1828, which I have but lately met with, it appears that Mr. Bywater employed a compound microscope of the construction called Culpepper's, that the object was examined in a bright sunshine, and the light from the mirror thrown so obliquely on the stage as to give a blue colour to the infusion.

The first experiment I here subjoin in his own words.

“ A small portion of flour must be placed on a slip of glass, and mixed with a drop of water, then instantly applied to the microscope;

microscope; and if stirred and viewed by a bright sun, as already described, it will appear evidently filled with innumerable small linear bodies, writhing and twisting about with extreme activity."

Similar bodies, and equally in motion, were obtained from animal and vegetable tissues, from vegetable mould, from sandstone after being made red hot, from coal, ashes, and other inorganic bodies.

I believe that in thus stating the manner in which Mr. Bywater's experiments were conducted, I have enabled microscopical observers to judge of the extent and kind of optical illusion to which he was liable, and of which he does not seem to have been aware. I have only to add, that it is not here a question of priority; for if his observations are to be depended on, mine must be entirely set aside.

July 28, 1829.

XXV. *On the Atomic Weight of Oxalic Acid and of Mercury.*  
By Mr. JOHN PRIDEAUX.

*To the Editors of the Philosophical Magazine and Annals.*

Gentlemen,

IN composing a scale of equivalents, now in course of publication,—more extensive, and designed to be more practical, than the one now in use,—I had occasion to examine the atomic weight of mercury, which I fancied Dr. Thomson had doubled; and of oxalic acid in crystals, wherein Dr. Prout having found but three atoms of water, while Dr. Thomson had found four, the latter suggested the probability of more than one variety existing. Being accustomed to meet with two varieties of these crystals, one firm and transparent prisms, the other acicular, friable, and with the aspect of quadroxalate of potash; and happening to possess some of each, I thought they might verify this suggestion.

1st.—18 grains of the friable crystals were dissolved in distilled water, and gradually mixed with a solution of 36 grains of dry transparent crystals of carbonate of soda. The mixture, boiled to drive off the carbonic acid, reddened litmus paper; and required for neutralization 5.15 grains of carbonate of soda. A minute portion more gave signs of alkali.

2nd.—9 grains of the same acid were neutralized with ammonia, and 6.25 grains of carbonate of lime, in clean rhombic crystals, were placed in a test tube with a little distilled water, adding muriatic acid, three drops at a time, until with the aid of heat it was dissolved, when it was washed out into the segment of a Florence flask and slowly evaporated to dryness.

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