

structural formula involves an extension of the older atomic hypothesis, in that it asserts definitely that the combining atoms do not blend, but come into juxtaposition in some orderly and systematic fashion, while stereochemistry, or chemistry in space, asserts that the parts of these systems are to a certain extent fixed in relative position, not rotating about each other after the manner of the members of the solar system. As to the inner nature of the atom itself, however, it says absolutely nothing.

To sum up, the laws of Constant and Multiple Proportions have led the chemist to regard matter as not continuous, but composed of units or atoms, these having the same mass and specific chemical properties in the same element, but other masses and other chemical properties in other elements. Innumerable facts lead him to believe that the atoms in the molecule are not blended, nor so juxtaposed as to have an arbitrary and constantly varying relation, but combined in such a manner that there is a more intimate relation between some atoms than others, some forming connecting links between the rest, a relation which is schematically represented by structural formulas. Finally, stereochemical phenomena indicate that the molecule possesses a certain definite geometrical structure, not necessarily rigid, but not having a mobility of its parts analogous to that of the parts of the solar system. As to the further divisibility of the atoms, their unalterableness, indestructibility, form, origin, and, in short, their absolute nature, the chemist knows nothing and has no opinion of real weight—for him they are merely centers through which energy manifests itself.

If those who adopt the atomic theory would carefully distinguish between essentials and non-essentials, and if those who deny the possibility of interpreting vital phenomena in terms of physics and chemistry would bear in mind that we know

scarcely more of the inner nature of the fragment of carbon than of the protoplasm into whose composition it enters, and that affinity is after all is as great a mystery as consciousness, we might possibly hear less of the impossibility of gross, inert, dead matter containing within itself "the promise and potency of every form and quality of life."

H. N. STOKES.

---

*SOME OBJECTIONS TO THE ATOMIC THEORY.\**

For the purpose of this discussion, all metaphysical conceptions or discussions are ruled out, and it is explicitly confined to that definition of the atom or molecule connoted by Dalton's famous hypothesis with such amplifications or modifications as have been brought about by the subsequent advance of physical science. In its inception the hypothesis was not without objectors, and properly so.

The efforts of Wallaston and others to insist upon the importance of considering 'combining numbers' or 'reacting masses,' things which one could really know about and determine experimentally, rather than hypothetical atoms, the existence of which in the nature of things was beyond physical proof, was philosophically sound, as far as it went. But the historical vicissitudes of the hypothesis, interesting though they be, can not be considered here, but rather, attention must be given to the comparatively recent discussions on this subject as they have appeared in contemporaneous epistemological writings.

The attempt will be made to present the present status of the subject with due regard to relative perspective values, rather than to cite article and authority in an historical retrospect. Dalton's hypothesis

\* Paper read before the joint meeting of the Chemical and Philosophical Societies of Washington. Time allotted for this contribution to the program, 15 minutes.

according to the generally accepted view of its genesis, was the result of an attempt to provide a mechanical explanation for the solution of a gas by a liquid. Under the influence of some force the particles of the gas are impelled, or diffuse among the particles of the liquid. The same is true of all solutions, the substance going into the solution disappearing as such. And furthermore, the solution, when in equilibrium, is homogeneous; consequently the particles must be very small though they remain discrete, and they must be uniform in mass, volume, and other properties.

The selection of the historical name atom for the particle did not necessarily imply anything more than has just been given. The apparently ready explanation these views gave for the laws of definite and multiple proportions which Dalton had already advanced, undoubtedly had a preponderating influence in bringing about their general acceptance. It is easy to see that granting the atomic hypothesis, the combination of two substances to form another, always identical, must be in definite proportions, or number of atoms or when there are multiple proportions they should bear a simple ratio to one another. But it is not easy to see how the atomic constitution of matter follows, as a necessary, if sufficient condition, from the laws of chemical combination, looked at purely objectively.

It would seem as though the human mind was so constituted that it necessarily demanded a mechanical explanation of recognizable phenomena. Indeed, some of our foremost thinkers apparently insist upon the truth of this proposition, and to Lord Kelvin, if memory serve me correctly, is credited the remark that he finds it impossible to understand phenomena for which he cannot construct a mechanical model, meaning mentally, of course.

It is natural that this feeling should exist. Our earliest impressions are associated with

mechanical phenomena. These phenomena are intimately connected with our visual and tactual impressions, with just those senses, normally most cultivated, and most closely associated with the logical faculty. All through life a very large proportion of every day experience is with mechanical processes. We thus come to look for the 'mechanism' of all phenomena of which we become conscious, and when it is not obvious we supply it by analogies from better known phenomena; thus the tendency to reduce all phenomena to the lowest terms of matter and the three laws of motion. This leads to the consideration of the Herbartian School, which is hardly within the scope of this paper. The point to be made is that the conception of the atomic constitution of matter to explain the laws of chemical combination is not in itself susceptible of proof, nor in the nature of things is it probable that it ever will be; and that after all, as we have it to-day, it is nothing more nor less than an *analogy* with a conceivable mechanical process. This idea has been more or less clearly brought out sometime since by J. J. Thomson, Mach and others. It has been treated, though not perhaps specifically, by Ostwald, in his now famous paper on the 'Failure of Scientific Materialism,' wherein he very clearly indicates the ultimate weakness of mechanical explanations of phenomena where they long held sway, instancing the *ignorabimus* polemics resulting from the famous address of Du Bois-Reymond; and more familiar perhaps the development of the theory of light, from the corpuscular theory, through the undulatory theory with its hypothetical ether, demonstrated by Kelvin to be necessarily unstable and physically non-existent, to the recent electromagnetic theory; and cites Hertz with whose name this theory is so closely associated as declaring that he saw nothing in it but six differential equations, in an effort

to save it from the inherent weakness of a possible mechanical explanation. A further instance of the failure of mechanical conceptions to account for observed phenomena is cited in the attempt of Helmholtz, Clausius, Kelvin, not to mention less well known workers, to modify Mayer's conception of the equivalency of various forms of energy, by the notion that all forms of energy are fundamentally the same—mechanical energy. Whereas it has remained impossible to conceive of a working mechanism for certain forms of energy, this idea is no longer urged as an appendage to the original conception, though it is pointed out its freedom from any arbitrary hypothesis should have been a sufficient reason. In another place he calls attention to the arbitrary hypothesis in the kinetic theory of gases; of artificially neutralizing the properties of directions by assuming that collisions are taking place equally in all directions; and the consequent failure, when attempts are made to extend the theory to electrical energy for example. This has been clearly pointed out by Mach also.

What we know of the outside world is through our senses, inherently energy manifestations. Of what gave rise to the sensations we know naught but these energy phenomena, or differences of energy. We are not accustomed to regard them objectively however, and we conceive for ourselves a mental picture, a mechanical one, *matter*, which it is true we cannot attempt to disassociate from energy, as giving rise to the energy manifestations which we can and do know. And to this hypothetical matter are ascribed properties, the most striking being its permanency or 'indestructibility.' Says Mach, "all our effort to mirror the world in thought would be futile if we found nothing permanent in the varied changes of things. It is this that impels us to form the notion of substance."

The hypothetical existence of matter is then merely a mental effort to give a mechanical explanation to observed energy phenomena; not as will be presently indicated that it is necessary we should have a mechanical explanation of phenomena, but that it has become a habit of mind with us. And with the notion of matter and of matter as made up of discreet molecules or atoms, we bring in many other arbitrary hypotheses. In accounting for special phenomena we have modified our original hypotheses with special attributes to meet each specific case: the present notions regarding the asymmetric carbon atoms; space isomers and polymers in general; varying valency; complex or 'physical' molecules determining the symmetry of crystals, etc. It is not contended by any one that these hypotheses have not been useful. Indeed, in the field of organic chemistry it is not easy to see how it could have reached its present highly developed stage without them. But it is contended that with a realization of the exact position of the hypothesis with relation to the phenomena for which it seeks to account, should come a realization of the retarding influence it undoubtedly has had, and may have on the development of science. It has been well said, that "it would not become physical science to see in its self-created, changeable, economical tools, molecules and atoms, realities behind phenomena. Forgetful of the lately acquired sapience of her older sister, philosophy, in substituting a mechanical mythology for the old animistic or metaphysical scheme, and thus creating no end of supposititious problems. The atom must remain a tool for representing phenomena like the functions of mathematics."

But if it be philosophically weak to use this mechanical device in accounting for observed phenomena, because unnecessary, because it necessitates a constant modification by subsidiary hypotheses and finally shows

evident signs of being ultimately as futile as mechanical explanations in general have proved for all classes of phenomena the question immediately arises, why not abandon it? Especially when we are already in possession of so elegant and flexible a method for the statement of phenomena as is furnished by the language of mathematics.—An instrument so highly developed, so fertile in suggesting interrelations, by its facility in bridging long mental processes as often to create an uncanny feeling to which the great Euler gave expression, that “his science in the person of his pencil surpassed himself in intelligence.”

Should we not rather agree with Mach who has defined physics (including of course all physical science) as “experience arranged in economical order,” that the “aim of research is the discovery of the equations which subsist between the elements of phenomena.”

This I take it may be regarded as a fair presentation of the ‘phenomenology’ or, ‘mathematico-physico-phenomenology’ point of view in contradistinction to that of ‘atomistics.’ Of the many contributions to the discussion, direct or indirect which contemporaneous literature furnishes, perhaps the most notable is from Boltzmann. He insists that both methods of presenting phenomena are but *methods*; that each possesses inherent advantages, that neither can with fairness be dogmatically declared superior, and until it shall have come about that the one has absorbed the other, both methods should be developed together. By both methods it is possible to present *comprehensive* conceptions of fields of phenomena not possible to direct description. But we should guard against introducing any unnecessary arbitrariness, rather than follow Ostwald in attempting no concept at all. That as a matter of fact, the concepts of the calculus rest fundamentally on the notion of a finite number of elements; otherwise

the theory of limits has no meaning and the differential equation does not represent a possibility. This is of course an essentially atomistic conception.

In avoiding arbitrariness as far as possible, by assigning as few properties as may be necessary to the atoms in any particular field of phenomena, we obtain special hypotheses.

Phenomenology attempts to co-ordinate these special hypotheses in one concept. There are at least two difficulties; the corresponding differential equations differ, making their comparisons a very complicated matter; they relate to stationary or nearly stationary conditions and cannot fairly represent turbulent reactions. The ‘energetic phenomenology’ attempts to consider what is common to these various fields, such as the energy laws, but fails because its results are too general, and the analogies are not applicable in all details.

Atomistics would attempt to co-ordinate these fields, by modifying the assigned properties of the atoms, and thus obtain a simultaneous, comprehensive view of the whole, to an extent not approached by phenomenology. Further, by this method some notion is to be had of turbulent actions. But the assigned properties of the atoms must be in accord with the special concept of the phenomenology, and therefore this latter should be developed also. So that atomistics, though they have hindered progress at times, still have a use. And the danger is in confusing the phenomenology of results already established with the atomistic hypotheses which sever to hold them together.

Volkman suggests a further qualification to these views to which Boltzmann assents, dividing physical phenomena into three classes: Coarser phenomena as elasticity or capillarity, where atomistic hypotheses are unnecessary; finer phenomena as electrolyses, dispersion of light, etc., when

atomistics may be useful; and a middle field when the usefulness of atomistics is doubtful, and to this last field only does Boltzmann's contention apply that both methods should be developed together.

Whether or no we may agree with Boltzmann as to the atomistic basis of the calculus, or the propriety of Volkmann's classification, we shall be inclined to hold his main contention as sound and conservative, albeit on utilitarian grounds. Both the language of mathematics, the medium of expression of the phenomenologist, and that of the atomisticist are but methods, after all, human instruments, ingeniously devised and beautifully developed, but merely instruments.

Whether we shall obtain a theory for observed phenomena which shall be as comprehensive as the atomic hypotheses without its adherent drawbacks, as flexible, as labor saving, as suggestive as the calculus, without its complexity in certain desired applications, by the absorption of one method by the other, it is not possible to say as yet. Quite possibly the ideal theory is to come from an entirely different direction. But for the present we must use those instruments which are at hand, and as long as they prove useful each in itself is worth the highest development we can give it. The idea expressed by one of the previous speakers this evening, that the coming generation will shake off the fetters of 'mechanism' and concern itself with 'parameters' may be true, but it seems to me a rather bold prophecy. More likely is it that notions of the 'atoms' and 'parameters' will develop side by side, the distinguishing feature of their study being a clearer view, a *realizing sense* of their exact relationship to phenomena. And with this it seems fair to assume will come those new principles which Ostwald has prophesied to account for phenomena, where the 'energistics' has failed and whose form it is futile to predicate at present.

To this end the discussion was probably necessary and should prove most useful. It is to be regretted that the time allotted me will not suffice to call to your attention the views of other thinkers on this most interesting subject. I trust that what I have been able to bring before you will at least indicate the status and importance of the subject.

FRANK K. CAMERON.

---

ON ARTIFICIAL PARTHENOGENESIS IN SEA URCHINS.

IN the last October number of the *American Journal of Physiology* I published a preliminary note on the artificial production of larvæ from the unfertilized eggs of the sea urchin. I mentioned that unfertilized eggs were able to develop into normal plutei after having been in a solution of equal parts of a 20/8 n MgCl<sub>2</sub> solution and sea water for about two hours. The control experiments by which the possibility of a fertilization of these eggs through spermatozoa had been excluded were briefly mentioned. In the April number of the same journal a full description of my experiments was published which I believe puts an end to any doubt concerning the possibility of an error. Nevertheless I decided to repeat these same experiments with the additional precaution of using *sterilized* sea water. Through the kindness of the board of trustees of the Elizabeth Thompson Fund I was enabled to make further experiments on artificial parthenogenesis at the Pacific Coast. These experiments have led to a number of new results which will be published in the *American Journal of Physiology*. Here I will confine myself to a description of the precautions which were taken in these experiments to exclude the possibility of a fertilization of the eggs through spermatozoa.

The sea water used for these experiments was heated the day before, very slowly, to