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JACQUES LOEB

JACQUES LOEB, THE MAN

JACQUES LOEB, savant, philosopher, mechanist, satirist, rebel, crusader, humanitarian—of whom shall I write these lines? The man is so big and still so near—too difficult to be embraced by a friend's eye, more so to be analyzed. I see his face, sunshine and shadows, smiles and frowns. How rapidly they interchanged, each capturing by its own force. Mankind and its weaknesses were his concern and the thoughts of these brought on the clouds, but to him, nothing was so sad as to be void of the humorous and thus the very grief begot amusement. And then there was Science, the guardian, the hope, the weapon!

I see Jacques Loeb at his desk in his modest home among his devoted family and surrounded by pictures of his spiritual friends, Voltaire, Diderot, D'Alambert, the men who dreamed of a realm where Reason was supreme authority, where all the crudities of superstition and outlived traditions were no longer permitted to crush human aspirations and human ambitions, where tolerance towered above all virtues. In them Jacques Loeb found his inspiration, their dreams became his dreams, their ideals he sponsored and to them he remained loyal and true through all his career and under all circumstances. It may not seem credible that a man who in his main pursuit was led by pure intellect should be moved by a motive emotional, should be inspired by the fire of a poet, by the zeal of a crusader. But such is the truth.

The efforts of Voltaire, Diderot and the entire group of encyclopedists were not in vain. The great French Revolution is the monument to their lives. But years passed and new tyrannies arose, new superstitions superseded the old and some of the old took a renewed hold. Times have changed and the methods of the old humanitarians no longer fitted the new conditions. Words, beautiful as they may be, eloquent as they may sound, were not the weapon Jacques Loeb chose to use in his struggle against superstition, in his crusade for intellectual freedom and for mutual tolerance. His ambition was to convince by realities hidden from the common eye. Thus, Jacques Loeb espoused Science, not merely as a career, nor as a medium to satisfy his personal curiosities. Science became his idol, his armor and his armament.

I see Jacques Loeb in his laboratory, again simple, surrounded by very few instruments and apparatus. From the walls are looking at him his old friends, Arrhenius, Van't-Hoff and Ostwald. Later the group was enlarged by Rutherford, Einstein and others. To them his eyes often turned for support and sympathy. It is here that the rebellious spirit inherited from Voltaire and fortified by revolutionaries in modern science found its full expression. Academic problems did not interest him; popular problems bored him. His weapon was always directed towards phenomena which had been surrounded by mystery and still more by superstition. In his quiet composed way, with tools as simple as toys, he gave battle after battle. As a Napoleon he tore to tatters the mantle of mystery from one phenomenon after another. His attacks were nearly always directed against phenomena which for generations had been considered supernatural—phenomena which academies by tradition regarded as lying beyond the scope of human analysis. Thus, in one of his first endeavors, he threw his gauntlet to the old metaphysical philosophers and dared from the behavior of plants to explain the workings of the human will. Such was the origin of the work on tropisms and on the physiology of the brain. Later came other challenges. Parthenogenesis, regeneration, senescence; phenomena so much used in support of the ideas of vitalism were all divested of their mystery and presented in the simple light of rationalism.

Jacques Loeb realized the responsibility of his position. He anticipated criticism and antagonism. He realized that the more fantastic his ideas might seem, the saner must be the method which led to them. Thus, from the very beginning, his efforts were directed to support his doctrines by the methods of sciences which are the oldest and the most accurate; by mathematics, physics and chemistry. His true genius was in the discovery of simple systems in which life manifested itself in a simple way accessible to analysis by the methods of accurate sciences.

P. A. Levene

Jacques Loeb, the Scientist

The death of Jacques Loeb at the zenith of his career comes as a great shock to all who knew him. His loss will be keenly felt not only among biologists but among men of science throughout the world.

I shall not here attempt to disclose the qualities which quickly made him a recognized leader in biology or the personal traits which endeared him to an ever widening circle of devoted friends. It need only be said that his goodness is an open book to all who knew him and his greatness will loom even larger with the lapse of time.

Of his influence on biology it would be difficult to give an adequate account. All his fellow-workers felt its spell and his death will not impair it: many who never knew him will be unconsciously guided by it in ways little guessed.

His conceptions were often so bold and original as to startle conventional thinkers. Fearless in attacking difficult questions of fundamental importance, he showed almost uncanny insight into the most obscure and baffling matters. His discoveries often had a dramatic quality in their unexpected and beautiful solutions of perplexing problems. His results were reached by methods so simple as to compel admiration. His papers were always clear, cogent and convincing. It is no wonder that he was a powerful stimulus to his fellow-workers.

His mechanistic view-point profoundly affected biology and medicine and had an important influence on psychology and philosophy.

It is possible only to recall some important facts in his career, which to so many has served as example and inspiration.

He was born in Mayen, Germany, April 7, 1859. He attended the Askanisches Gymnasium in Berlin, 1877–1880; the Universities of Berlin, 1880; Munich, 1880–81; Strassburg, 1881–1885 (M.D., 1884; Staatsexamen, 1885), and in 1885–1886 he was again at the University of Berlin.

He was assistant in physiology at the University of Würzburg, 1886–1888, and at the University of Strassburg, 1888–1890. From 1889 to 1891, at the Naples Zoological Station, he laid the foundation for his work on marine biology which was afterward continued at the Marine Biological Laboratory at Woods Hole, Massachusetts, and at Pacific Grove, California. In 1891 he came to America and became associate in biology at Bryn Mawr College. From there he went to the University of Chicago as assistant professor of physiology and experimental biology in 1892; later he became associate professor, and then professor of physiology. In 1902 he accepted the professorship of physiology at the University of California. In 1910 he became Member of The Rockefeller Institute for Medical Research and remained there until his death.

Attracted in his student days by certain aspects of metaphysics, he became especially interested in problems connected with the freedom of the will. The idea that certain brain functions are localized in definite centers and that human conduct may be profoundly affected by disturbances in these centers led him to study medicine to gain the technical knowledge needed for experiments in order to learn to what extent apparently volitional acts can be controlled by physical and chemical agencies. These studies led him, before the age of thirty, to the revolutionary conception that the actions of animals may be largely explained on a physicochemical basis. He took from the botanist, Julius Sachs, the idea of tropisms and applied it to animals. He sought a mechanistic explanation of animal conduct which should drop the question of purpose and reduce the reactions of animals to quantitative laws. His researches in this field are summarized in English in two volumes, "Comparative Physiology of the Brain and Comparative Psychology" (1900) and "Forced Movements, Tropisms and Animal Conduct" (1918).

He next undertook to control the growth and form of animals by physical and chemical means. The studies in this field, which he named "Physiological Morphology," covered a very wide range and continued to receive his attention up to the time of his death. The goal at which he aimed was to secure the same degree of control over living matter that the chemist and physicist have over their material and he felt that the best prospect of success lay in applying their methods to biology.

It was characteristic that in the pursuit of these researches he was prompt to utilize recent discoveries in physical chemistry, particularly those connected with the ionic theory and the theory of osmotic pressure. From 1897 on he published papers applying these theories to biological phenomena and they may be said to form the leitmotif of his subsequent work. Out of them arose, almost at once, two important discoveries, antagonism and artificial parthenogenesis. Both resulted from experiments of the simplest kind, carried out on marine organisms.

It was found that the fish, *Fundulus,* can grow and develop in distilled water but soon dies if sodium chloride is added. The addition of other salts, particularly those of potassium and calcium, which are themselves toxic when they alone are present, produces a harmless solution. Loeb called this a balanced solution, that is, one in which the toxic effects of one substance are offset by the antagonistic action of other substances. This conception proved to be a fruitful one.

As the result of subsequent experiments he concluded that these facts may be accounted for by the effects of the antagonistic substances on the permeability of the protoplasm. It may be added that his experiments on permeability, which covered a wide range, led to the conclusion that Overton's hypothesis is untenable.

His experiments on artificial parthenogenesis may be truly called epoch-making. In 1913 he reviewed them in a volume entitled "Artificial Parthenogenesis and Fertilization" (following the publication of two volumes on this subject in German). He continued work in this field for some years after the publication of this volume. He aimed at a complete analysis of the mechanism of fertilization, development and heredity. His work on fertilization indicated that the principal function of the sperm in stimulating development is to carry into the egg a substance which produces a surface change leading to the production of the fertilization membrane. This effect could be brought about, independently of sperm, by a variety of physical and chemical agencies.

At the same time he succeeded in finding means to bring about crosses which never occur in nature. By a slight change in the composition of the sea water eggs could be fertilized by sperm of other species which could not normally enter the egg.

In the course of these studies a great number of questions presented themselves which excited his keen interest. The variety and extent of these problems can only be realized by a careful consideration of the contents of the volumes entitled "Studies in General Physiology" (1905), "Dynamics of Living Matter" (1906), "The Mechanistic Conception of Life" (1912) and "The Organism as a Whole" (1916).

Among theese subjects may be mentioned the rôle of oxygen in metabolism, toxicity and development. Another in which he was deeply interested is the cause of natural death and the means of lengthening life. In the course of these studies he found that in certain cases low temperature prolongs life to a remarkable degree. In this connection it may be recalled that the first studies on the temperature coefficient of the heart beat and of the transmission of stimuli in nerves were made by his students.

Another investigation which he initiated, and which grew naturally out of these studies, showed that the electric potentials existing in the organism can, in many cases, be accounted for qualitatively and quantitatively by relatively simple means, and that they can be imitated to a considerable extent by artificial models.

As he truly said, many biologists accept a mechanistic explanation of various functions of the organism but fail to employ mechanistic conceptions in dealing with the larger problems of organization and adaptation. It was, however, precisely these problems which fascinated him and he did not hesitate to attack them from his point of view. He showed that many characteristics of the organisms which are regarded as adaptive may be explained on a mechanistic basis. He felt that here, as elsewhere in biology, progress requires quantitative investigation and with this in view he began the quantitative studies on regeneration upon which he continued to work until his death. His latest book, "La nature physiochemique de la régénération" (Paris, 1924), is devoted to this subject.

His work began, as was said, with questions concerning the freedom of the will. But he found that in order to study these he must attack the simpler problems involved in the behavior of lower organisms. These in turn required for their understanding a study of the physical and chemical reactions on which they are based. It is, therefore, not surprising that in the closing years of his life he came to devote his attention almost wholly to the properties of colloids, upon which life phenomena so largely depend. In a recent volume on "Proteins and the Theory of Colloidal Behavior" (1922) he contends that the behavior of colloids may be explained by the ordinary laws of chemistry without recourse to theories based on adsorption. As in earlier researches he had found a clue to the solution of many problems by applying the theories of electrolytic dissociation and osmosis, so in the work of his later years he discovered a guiding principle in the theory of the Donnan equilibrium. By applying this he was able to give quantitative explanations of some of the most important properties of proteins and to reduce them to simple mathematical laws. These studies, important for chemistry as well as for biology, form a fitting termination of his activity.

Thus closed a career rich in the joy of pioneer adventure in fresh fields of thought, abounding in brilliant discoveries, and splendidly stimulating far beyond the boundaries of biology. It will always stand out as a poriment feature of the progress of biology toward the status of an exact science. It is a career which reveals everywhere a creative imagination and capacity found only in minds of the highest order.

W. J. V. OSTERHOUT

THE SUBJECT-MATTER OF A COURSE IN PHYSICAL CHEMISTRY[1]

THIS is not intended to be a pedagogical paper. Rather, it is proposed to review briefly the recent progress in theoretical chemistry with the idea of suggesting how the content of the more or less classical course in physical chemistry should be altered. It is quite likely that recent progress will indicate that the emphasis should be shifted from some subjects to others, and it is not unlikely that some of the ideas we have been imparting to the students in the past may in the light of recent developments prove erroneous.

Some instructors in political science, I believe, are wont to present to students the various theories of applied government, old and new, without prejudice, leaving the student to choose for himself those which appear the more logical or feasible. The student thus occasionally becomes infatuated with some highly ingenious theory such as the single tax, to the great disgust of conservative tax-payers who proceed to denounce the university for the inculcation of radical ideas. Now such procedure may be entirely justifiable in the more controversial sciences, but it does not appear desirable in the teaching of theoretical chemistry. In the first place, theories in chemistry are subject to immediate experimental verification and while the results are sometimes ambiguous, if a theory has failed to establish itself after years of experimental test, we may conclude that it is badly conceived, or at least not likely to prove fruitful as a guide to new facts. In the second place, in the limited time available, we can scarcely make the undergraduate familiar with the well-established fundamental principles of theoretical chemistry, and we certainly have little time to spend upon matters of speculation and controversy.

The logical although perhaps not the best psychological way to begin a course in physical chemistry is to develop the concept of the atom and its structure. If we begin this way, the laws of combining weights and multiple proportions become mere axioms, while the experimental work in radio-activity, X-ray study of crystallography and positive rays become exhibit A in proving the case for the existence of the atom. To begin the structure of the atom, we have the Rutherford nucleus, although recent work is said to show that the experiments which Rutherford used to demonstrate the existence of a small nucleus might equally well be used to prove that the atom had the constitution originally assumed by J. J. Thomson. Nevertheless, the work of Bohr and Lewis has given us a picture of the structure of the atom which is sufficiently definite and which explains the necessary and important properties of the atom. While this picture leaves many things to be desired, the evidence for its validity is as direct and abundant as was the evidence thirty years ago for the existence of the atom itself. And one of the greatest mistakes that the physical chemist has made was his adoption under the leadership of Ostwald of an agnostic attitude toward the existence of the atom.

One of the most important things that we can do is to give the student an up-to-date model of the periodic table. The old Mendeleieff table attempted to show only the chemical relationships of the elements and it did that rather badly as its author himself realized. An improved periodic table ought (1) to be based upon atomic number instead of atomic weight, (2) it ought to show the number of valence electrons, (3) it ought to show the number of shells of electrons, (4) it ought to show the real chemical resemblances, (5) it ought to indicate the degree to which the element possesses the property whose presence is variously designated as electronegativity or acidity and whose absence is termed electropositivity or basicity. Now in the nature of things it is not possible to arrange a table which will comply completely with all these requirements. If anything is to be sacrificed, I should prefer it to be some of the far-fetched chemical resemblances.

Any arrangement of the periodic table should pre-

[1] Paper presented at the Cincinnati meeting of the American Association for the Advancement of Science, Section C.