WINTER MEETING.

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PRESIDENT'S ADDRESS.

LAVOISIER.

BY W. A. NOYES.

On May 8, of this year was the centennial of the execution of a man who influenced profoundly the development of scientific knowledge. To Antoine Laurent Lavoisier belongs chiefly the honor of the saying that "Chemistry is a French science," a saying which possesses a certain amount of truth, though it does injustice to much good work done elsewhere, and is entirely false as regards the present condition of the science.

During many centuries, such workers in chemistry as there were followed mostly a vain search after gold and after the elixir of life. During this period of sordid aim many facts were discovered, but little real progress was made, for facts do not constitute a science. Then, for another century, chemistry was pursued mainly in connection with the study of medicine with the thought that the science would hold in its grasp the secret of all disease and its cure. During this period, too, there was some progress, for the aim was a little less sordid and base, and somewhat more rational means were used, but the chemistry of that day, very much like a good deal of the medical science even of to-day, labored under the difficulty of being an applied science without any satisfactory foundation in pure science. As in all such cases, the science was constantly confronted with the necessity of doing something immediately, when it had nothing but the crudest empiricism to guide it. The best deductions which were possible were made from a few and very imperfect data, and the conclusions were very often in error. Often years, or even centuries of experience are required for the discovery, by such methods, of the right course of procedure, which may, later, be known as a simple corollary from a single principle of pure science.

Pure science belongs to all time, and can wait for a fact or a principle till the time is ripe for its discovery. Applied science is essentially ephemeral, and must have *to-day* the best it can get. If it can not find certain knowledge it must guess to the best of its ability. And so it follows that only those forms of applied science which follow in lines of pure science make great and lasting progress.

Nowhere has this been more clearly evident than in the development of chemistry. Medicine, during the era of medical chemistry, probably killed more of its patients than it cured, and the applied chemistry of to-day makes greater advances in a decade than were made during whole centuries of empiricism.

It is scarcely more than two centuries since a few men first began to search into the composition of bodies with the pure, high aim of an endeavor to extend human knowledge. From this period dates the beginning of chemistry as a pure science, and in this sense those who refer the beginning of chemistry to Lavoisier do injustice to such men as Boyle, Stahl, Black, Scheele, Priestly, Cavendish and many others.

These men worked with the same spirit and purpose, and often in the face of far greater difficulties than those which later workers were compelled to face. These were the real pioneers of pure chemistry.

In the hands of these workers we find for the first time in the science one of the best and highest characteristics of any pure science, the proposal, development and general acceptance of an important theory—a theory which coördinated and explained from one point of view many and diverse phenomena—a theory conceived in a pure philosophical spirit—one step in the constant endeavor of the highest minds to tear away from before our eyes the things which are fortuitous and misleading and to get a little closer to the realities which lie at the basis of all material existence. I refer, of course, to the theory of phlogiston.

In outward appearance ordinary combustion is of the nature of a decomposition and this view of the phenomena was held from the earliest times. Building, as every founder of a theory must, on the best knowledge which was possessed, and recognizing the close connection between the oxidation of metals and ordinary combustion the chemists of this time proposed the theory that all bodies capable of combustion or oxidation contain a common substance or principle called phlogiston, and that combustion consists in the escape of the phlogiston leaving behind that with which it was combined.

In accordance with this view, wood, charcoal and similar substances are rich in phlogiston and mostly disappear in burning. Metals are composed of phlogiston and the metallic calex or what we now know as the oxide—the metal being considered compound and the oxide as one of its parts.

At the time when the theory was proposed and developed it gave a quite satisfactory explanation of most of the phenomena then known. It served as a means of bringing together under one point of view very many and diverse facts and of coördinating them all under a system which was clear and intelligible. As new facts were discovered they were explained and systematized as far as possible in accordance with the theory. And so it happened that, while the theory contained only a partial truth, and even that half-truth was so badly distorted that we have some difficulty even now in recognizing it, the development of the science was comparatively rapid during this period. And we may be sure that this theory furnishes one important reason why chemistry made more progress during the century of its proposal, development and general acceptance than during many centuries before. But, as often happens, a theory which was extremely valuable for a time and which was probably the best which the science of its day was capable of accepting, outlived its usefulness and was generally believed after the facts necessary for its overthrow had been discovered. At such times there comes the necessity for a man with a profound reverence for facts as of supreme importance and as beyond and underlying all theories—a man, too, with great power to see through all external phenomena and grasp their true explanation in spite of any preconceived notion or any theory no matter how generally accepted. I think it not without significance that the man who could do all this for chemistry was produced in France during that period before the revolution when the country was full of the fermentation of those ideas which led to that tremendous revolt against all forms of dogmatism and authority when men were ready to question ideas and beliefs which had been held sacred for centuries and when the feeling was prevalent that all knowledge and even all forms of society must be torn down and rebuilt from the very foundations.

Lavoisier was a fit product of such an age—a man capable of proposing a heresy in the face of all orthodox scientists and with the ability, too, to prove, in the end, that his heresy was true and orthodoxy was false.

Lavoisier was born in 1743. His father was a wealthy merchant, who was, himself, interested in science, and personally acquainted with some of the most noted scientific men of Paris. The son received a thorough education under the best teachers of the city. He seems to have been especially interested in mathematics and chemistry, but studied carefully other sciences as well. He was first known to the scientific world through his competing, when 21 years old, for a prize offered by the French government for the best method of lighting a great city. The prize of two thousand livres was awarded to Lavoisier, but he caused the money to be divided between three of his competitors to repay them for their outlay in making experiments. He received, however, through the French Academy, a medal granted him by the king in recognition of his services, and it was largely in consideration of this work that he was chosen a member of the Academy at the early age of twenty-five. While Lavoisier devoted most of his time and energy to the prosecution of researches in pure science he seems always to have retained a lively interest in technical applications of scientific knowledge, and often rendered valuable services to his country in such matters. For a long time he had oversight of the manufacture of saltpeter and gunpowder for the French government, and it is remarked that during this period the gunpowder of France was the best in the world, while after his death it became much inferior. We can scarcely find a better answer to those who would have us think that an interest in technical applications is beneath the dignity of those who are devoted to the development of the higher departments of science. We find, on the contrary, that scientific men of the very highest rank have shown great interest in the material advantages which would result from their discoveries and have frequently taken time for the careful study of technical problems. While the absorbing consideration of material results, which is required of those who are engaged in technical pursuits, is undoubtedly incompatible with any high scientific attainment, I believe that the scientist who occasionally studies carefully and thoroughly some technical application of his science will find that his mental horizon has been broadened by the process. We have too many men nowadays who are so absorbed in some narrow corner of their science as to lose all breadth of view and all true sense of relative value and importance in scientific work, and who become one-sided and seriously dwarfed in character. It is, after all, important that one should be a man, and retain broad human interests as well as that he should attain high rank as a scientist.

In speaking of Lavoisier's work in pure science I shall not attempt an exhaustive catalogue of his researches, for it is not my purpose to give a history of his life, but rather, if possible, to gain a clear conception of his character and his work and of the relation which these bear to the development of the science of chemistry.

The first work in which we can see some clear relation to his later achievements was published in the memoirs of the Paris Academy for 1770. It concerned the conversion of water into earth. The mere fact that such a topic should require careful experiment and serious discussion gives us a glimpse of how very radically different from ours was the opinion of the best science of that day upon such fundamental subjects as the indestructibility and interconvertibility of matter. From the earliest times it had been believed that water may, under various conditions, be converted into earth. In later times it was thought that this view had been confirmed by the work of many careful experimenters. Glass vessels were almost universally used for the distillation and evaporation of water, and many different observers found that even water which had been repeatedly distilled left behind, on evaporation, small amounts of earthy matter which were thought to have been formed by the section of heat upon the water. To test the matter Lavoisier placed some water in a sealed vessel so arranged that the water could be boiled in the lower part, while the steam would condense above and run back. He kept three pounds of water boiling in this way for more than three months. At the end of the time he evaporated the water and obtained from it 20.4 grains of earthy matter, while the vessel used had lost 17.4 grains in weight. The difference he considered as due to unavoidable errors of experiment, and from the imperfect data he drew the correct conclusion that water can not be changed into earth. Such results as these must have given to Lavoisier the feeling that he could not trust the observations of other chemists, but must test every experimental fact for himself. This attitude, which was, undoubtedly, not without some reason, is closely connected with one of the worst sides of his character—a tendency to belittle the work of others, and even to appropriate as his own discoveries made by others. We find that Lavoisier repeatedly described discoveries which had been made by some one else in such a manner as to give the impression that the discovery had been made by himself. It is true that in some cases the discovery acquired in his hands an entirely different meaning. This is especially true of the discoveries of oxygen and of the composition of water. Lavoisier was, undoubtedly, the first to see the true significance and importance of these discoveries, and the very great value of the discoveries to the scientific world depends far more on the labors of Lavoisier than on those of Priestley and Cavendish. Yet this can not lead us to condone the desire which was shown of appropriating for himself the honor which belonged to others. Indeed, we can not but feel that such conduct is more than usually reprehensible in one whose own work was really so very great and who, of all men, had so little need to seek for honor that was not entirely his own. There was certainly something lacking in the moral fiber of the man which detracts very much from our opinion of his personal character however much we admire his scientific achievements.

Lavoisier's study of the conversion of water into earth was of especial interest because of the way in which he attacked the problem. Previous to his time very few chemists paid any attention to quantitative relations in chemical phenomena, and his use of the balances in studying the question proved in his hands the beginning of a new era. Too much has often been made, however, of this distinction between the chemistry of the era of phlogiston and that which immediately followed the downfall of that theory. Cavendish spent a great deal of time on quantitative experiments, and many of his results exceeded in accuracy those of Lavoisier, yet all of his work was conceived and his results were interpreted in terms of the theory of phlogiston. Methods of quantitative analysis similar to those still in use were developed by Bergmann and by other contemporaries of Lavoisier who still held entirely to the old theories. The quantitative method was "in the air" as it were, and was coming into more and more extended use in the hands of many different chemists. And, even after Lavoisier's views were generally accepted, quantitative results were usually very inaccurate till some time after Dalton's atomic theory had given a sharp means of control. Lavoisier's greatness was not so much in the introduction of a new method, as in that wonderful insight which enabled him to see through the appearances on the surface and find the real reasons which lay beneath.

The beginning of his most valuable work seems to have been made in 1772, when he was not yet thirty years of age. In a short note written at this time and published in 1773, he states that the oxidation of metals and also the combustion of phosphorus and sulphur is accompanied by an increase of weight and by the absorption of a large amount of air, also that on the reduction of metallic oxides a large amount of gas, or "air" is evolved. In these crude and imperfect statements, we see the germ of all his greatest discoveries. In 1774 he described more accurately his experiments with tin. He placed the metal in a retort, sealed it hermetically and weighed the whole. He then heated the retort till the tin was oxidized, and then weighed the whole again, showing that there was no change in weight. On opening the retort, air entered, and there was an increase of weight which he says was exactly equal to the gain in weight of the tin due to oxidation. We know that this could not have been strictly accurate, for oxygen had been absorbed, and air, which is specifically lighter than oxygen, had entered, but we see once more the great power which the man had of drawing correct conclusions from imperfect data. A chemist of some standing has recently said, "that it is not the province of science to explain anything," and "that the business of science is to *describe* phenomena in a simple manner, to seek actual relations between measureable quantities, to deal only with things which can be handled and measured." How erroneous and imperfect such a view of the province of science is, was never better illustrated than in the present case. Essentially the same fact in almost all of its details had been observed by Boyle one hundred years before, and many others had observed that metals increase in weight when oxidized. The fact alone was barren, the fact in conjunction with its correct explanation became fruitful in wonderful scientific developments.

In these first experiments Lavoisier does not seem to have recognized but what air, as a whole, was absorbed in processes of oxidation and combustion. On August 1, 1774, Priestley, in England, discovered oxygen gas, and visiting Paris

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1. J. E. Trevor, Jr., Am. Chem. Soc., *16*, 520.

soon after, he described his discovery to Lavoisier. Priestley, with the other chemists of his time, held to the theory of phlogiston, and expressed his discovery in terms of that theory. In accordance with that theory he called oxygen dephlogisticated air, and nitrogen, or in general, air which had lost the power of supporting combustion, whether pure nitrogen or not, phlogisticated air. The thought conveyed by these terms was that air possessed a certain capacity for absorbing the phlogiston which was supposed to be given off during combustion, but that ordinary air already contained a considerable amount of phlogiston. If this phlogiston were removed the capacity to take it up again would, of course, be increased, and the resulting substance which we call oxygen could properly be called dephlogisticated air, while nitrogen, which was supposed to have taken up all the phlogiston which it could hold, was called phlogisticated air. It is evident, at once, that while the honor of the discovery of oxygen really belongs to Priestley, the new substance was not to him a separate and distinct element in any such sense as we now understand it, but was rather a sort of modified air. The theory of phlogiston dealt chiefly with outward appearances and qualitative phenomena, and the time had now come when the theory was inadequate and a hindrance to further progress. Lavoisier seems to have been the only chemist of the time who recognized this. After Priestley had told him of his discovery he repeated the experiments for himself, and soon came to a comparatively clear and correct view of the composition of air, and the real nature of oxidation and combustion. But while even at this early date he must have begun to see that the theory of phlogiston was unnecessary, and probably fallacious, his open conflict with the theory does not seem to have begun till several years later. He contented himself with a description of his experiments and explanation of his results, rather ignoring than directly combatting the prevailing theory. He had acquired reputation by this time as a careful experimenter and as one thoroughly acquainted with the history and theories of his science. He was recognized, therefore, when the time came, as one competent to criticise current theories, and as one whose criticism must, at least, receive respectful attention.

During the ten years that followed, from 1775 to 1785, Lavoisier busied himself almost exclusively with experiments more or less closely connected with combustion and oxidation. Gradually he proved, by careful experiments made with a great number of different substances, that ordinary combustion consists in all cases of a combination with oxygen. He showed that "fixed air" is formed by the combustion of the diamond and of charcoal; that phosphoric acid, according to the nomenclature of the period which followed, is formed by the combustion of phosphorus, and also by its oxidation with nitric acid; that both sulphurous and sulphuric acids are compounds of sulphur with oxygen, and that "fixed air" is also formed by the combustion of candles and of other organic matter. These experiments led him to not only clear and correct views of the phenomena of combustion and oxidation, but they also gave rise to a radically new conception of the nature of acids and salts. The opinions which he developed were afterwards found to be imperfect, but they were a very great advance on anything which had preceded, and were of incalculable value in the development of chemical science. After finding from his own experiments and those of others that oxygen is a common constituent of carbonic, phosphoric, sulphurous, sulphuric and nitric acids he made the generalization that oxygen is the source of all acid properties, and called it by its present name, which means "acid former." To him an acid was simply a compound of carbon, sulphur, or some other element with oxygen, and a salt was a compound of such an oxide with an oxide of a metal. This view held practical sway in chemistry for sixty years, and is at the basis of many expressions which chemists still use. While doubtless less perfect than the view which considers acids as compounds of hydrogen, it nevertheless expressed clearly some truths which our modern chemistry does not quite so clearly express, for oxygen is still, as always, a great acid-forming principle, and salts contain metals as well as non-metals in an oxidized form. Lavoisier considered that the combination of a metal with an acid may take place in two ways. Either the metal combines with a part of the oxygen of its acid forming an oxide which then combines with the acid, or as we should say, with the anhydride of the acid; or the metal, by the aid of the acid, decomposes the water present, combining with its oxygen and liberating its hydrogen, and the oxide formed there combines with the acid. The first view may still be considered as essentially correct as an explanation of such cases as the action of concentrated sulphuric acid on copper; here copper oxide is undoubtedly formed, for some of it escapes combination with the acid, and sulphur tri-oxide is present as an independent compound at the temperature of the reaction, and very probably combines with the copper oxide as it is formed, to produce copper sulphate. As regards the second view, which applies to such cases as the solution of zinc in dilute sulphuric acid, there is still some diversity of opinion and some uncertainty in the minds of chemists. The common statement of our text-books is that the action consists in a substitution of the metal for the hydrogen of the acid, and this is undoubtedly correct, as a superficial view of the matter. The explanation which has been more recently proposed, however, and which has already gained many adherents, is that direct substitution takes place in such cases with very great difficulty, if at all, and that action takes place readily only when the acid has been dissociated into its ions, and that the real action consists in the exchange of charges of electricity between atoms of hydrogen and atoms of the metal, the atoms of the metal, with their newly acquired charge, becoming ions in the solution. Whatever may be the truth of the matter, the views of Lavoisier were of very great value in the development of chemistry. They contributed to a clearer conception of the nature of salts, and they laid the foundation for a rational nomenclature, which was introduced for the first time in connection with Lavoisier's system, though the principles of the nomenclature seem to have been proposed by De Morveau, and Berthollet and Fourcroy aided Lavoisier in their development.

Beside the theories of combustion and oxidation and of the relations of acids, oxides and salts, which must be considered as his greatest contribution to science, Lavoisier worked successfully in a number of other directions. He paid close attention to the heat relations involved in combustion; he studied carefully the alcoholic fermentation and gained a very close and correct conception of the process and made some attempts to determine the quantitative composition of organic bodies. These attempts were not very successful, but the methods used were correct in principle and laid the foundation for the better work which was done years afterwards. In the domain of physiological chemistry and in physics Lavoisier also did some excellent work.

His literary activity consisted chiefly in the preparation of papers describing his work. No less than sixty communications of this kind were published in the Memoirs of the Paris Academy from 1768 to 1787. Not till toward the close of his life did he gather the results of his work together in a systematic treatise on chemistry, which appeared in 1789. I can not refrain from quoting two extracts from this book, which give us a glimpse of the character of the man and show us something of the secret of his wonderful power. The first is from his preface.

After calling attention to the fact that in every day affairs our mistakes are constantly checked and corrected by the unpleasant effects which follow them, he goes on to say:

"In the study and practice of the sciences it is quite different; the false judgments we form neither affect our existence or our welfare and we are not forced by any physical necessity to correct them. Imagination, on the contrary, which is ever wandering beyond the bounds of truth, joined to self-love and that self-confidence we are so apt to indulge, prompt us to draw conclusions which are not immediately derived from facts; so that we become in some measure interested in deceiving ourselves. Hence it is by no means to be wondered that in the science of physics in general men have often made suppositions instead of forming conclusions. Those suppositions, handed down fromone age to another, acquire additional weight from the authorities by which they are supported, till at last they are received, even by men of genius, as fundamental truths.

"The only method of preventing such errors from taking place, and of correcting them when formed, is to restrain and simplify our reasoning as much as possible. This depends only on ourselves, and the neglect of it is the only source of our mistakes. We must trust to nothing but facts; these are presented to us by nature and can not deceive. We ought, in every instance, to submit our reasoning to the test of experiment, and never to search for truth but by the natural road of experiment and observation. Thus mathematicians obtain the solution of a problem by the mere arrangement of data, and by reducing their reasoning to such simple steps, to conclusions so very obvious, as never to lose sight of the evidence which guides them.

"Thoroughly convinced of these truths, I have imposed upon myself as a law never to advance but from what is known to what is unknown; never to form any conclusion which is not an immediate consequence necessarily flowing from observation and experiment; and always to arrange the facts, and the conclusions drawn from them in such an order as shall render it most easy for beginners in the study of chemistry thoroughly to understand them. Hence I have been obliged to depart from the usual order of courses of lectures and of treatises on chemistry, which always assumes the first principles of the science, as known, when the pupil or the reader should never be supposed to know them till they have been explained in subsequent lessons. In almost every instance these begin by treating of the elements of matter, and by explaining the table of affinities, without considering that, in so doing, they must bring the principal phenomena of chemistry into view at the very outset; they make use of terms which have not been defined and suppose the science to be understood by the very persons they are only beginning to teach. It ought likewise to be considered, that very little of chemistry can be learned in a first course, which is hardly sufficient to make the language of the science familiar to the ears, or the apparatus familiar to the eyes. It is almost impossible to become a chemist in less than three or four years of constant application."

These statements are no less true to-day than one hundred years ago. No less apposite is the following, referring to the work to be done in chemistry:

"This is a vast field for employing the zeal and abilities of young chemists, whom I would advise to endeavor rather to do well than to do much. * * * * Every edifice which is intended to resist the ravages of time should be built on a sure foundation; and, in the present state of chemistry, to attempt discoveries by experiments, either not perfectly exact, or not sufficiently rigorous, will serve only to interrupt its progress, instead of contributing to its advancement."

During the stormy days of the Revolution, as well as before, Lavoisier rendered frequent services to his country. In 1787 he was elected to the Provincial Assembly of Orleans. In 1790 he was a member of the commission which devised the metric system of weights and measures. In 1791 as a member of a commission he published an essay on the National Resources of France, which entitles him to high rank as a political economist. These facts show that he was a man of broad interests as well as a chemist of preëminent rank.

Some of his public acts, and especially those in connection with the collection of taxes rendered it easy to find some trivial complaint against him. And during the reign of terror, while the power of Robespierre was at its height, a trivial complaint was equivalent to condemnation. After sentence he asked for a fortnight's delay that he might complete some scientific experiments, but with the words "We have no more need of philosophers," he was hurried to execution. So died, on May 8, 1794, the greatest chemist of the eighteenth century. I had almost said of any century. For we can scarcely find in the history of thought another who has so transformed the science with which he worked. He cleared away the misconceptions and erroneous speculations of centuries and, building on a solid basis of experimental facts, he laid a sure foundation for rapid and permanent growth in chemical knowledge.

PAPERS READ.

SOME FACTORS IN THE DISTRIBUTION OF GLEDITSCHIA TRIACANTHOS, AND OTHER TREES. BY ERNEST WALKER.

The importance of winds as factors in the distribution of plants has always been recognized by all who have written on subjects connected with plant-geography. It seems, however, that their effectiveness has been appreciated only in the case of extremely fine and light seed, or those provided with appendages for suspension in air, while in the case of heavier seeds, unprovided with such appendages, they are held even by many of our most authoritative writers to be of little or no consequence. Such seeds are thought to be too heavy to be affected in the least by any wind short of a "violent storm" or real "hurricane." As these are only