he can among animals, children, defectives, the insane, criminals, paupers, saints, sinners, the sick, the well; must know grief and joy—these, as well as the clinic and the laboratory, for here he fronts the bottom facts of the world. Next, he must supplement his at best meager first-hand experience with the proxy experience of others as recorded in books. Psychology lives not merely in the study, but where doubt and belief, sanity and inherited insanity, struggle together; where temptation and conscience wage their wars, in the mob, the cloister; where rage, terror and pity become convulsive and sweep all before them, and where love of the lie usurps that of the truth. Once it was thought that the study of pure should precede that of applied science, but we are now coming to almost reverse this maxim in education. So psychology, especially in our practical age and land, must first study and teach how to live, love, learn, labor; must have something to say to all who reflect on reproduction, disease, health, and thus must first serve man well if it would later rule him wisely. If this view be correct we must abandon many supposed certainties and finalities, and with faith in a future far greater than the past has been, devote ourselves to severe and unremitting toil perhaps for generations; must often practise that hardest of all forms of self restraint in our field—the suspense of judgment—assured that in the end psychology is to become queen of those sciences that deal with man, and reign among all the humanities somewhat as chemistry and physics are coming to do over the material world, with a method, perhaps, sometimes no less exact and certain than these already have. So we shall at last attain a true metaphysics of realities behind sense and feeling which is the necessary crown of all science when it becomes complete.

G. Stanley Hall.

PRESENT PROBLEMS OF ORGANIC CHEMISTRY.[*]

There is a strong tendency on the part of some chemists, at the present time, to claim that chemical science in the true sense includes only such portions of our knowledge as can be stated in accurate mathematical terms. One distinguished representative of this school of chemistry has said, 'It is not in the province of science to explain phenomena,' and another has written, 'It is not a part of its ultimate object (*i. e.*, of natural science) to acquire knowledge in regard to mentally conceived existences, such as the atoms of matter, or the particles of luminiferous ether, which are of such a magnitude and character as to lie far beyond the limits of human perception.' I think that nearly all of those now actively engaged in working over the problems of organic chemistry would dissent strongly from these statements. Long experience in dealing with the cumulative, non-mathematical evidence upon which our knowledge of chemical structure is founded has led to a very firm conviction that human knowledge is not bounded by the limits of sense-perception. We are inclined rather to the view that, while there are, undoubtedly, many things which will always remain beyond any direct cognizance of our senses, yet, so far as these have a real existence we may in the end secure, regarding them, very practical and positive knowledge. It is impossible to conceive that those theories with regard to structure which have guided the work of thousands of chemists for the last fifty years do not in some measure express the actual truth with regard to atoms and their relation to each other in organic compounds.

Let us follow, for a few moments, in very brief outline, the steps which have led to the present standpoint. So far as the mat-

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[*] Read at the International Congress of Arts and Science in St. Louis, September 21, 1904.

ters which interest us most are concerned, there was practically no knowledge of organic chemistry before the nineteenth century. The first steps were, of course, the preparation of pure substances and the development of accurate methods of analysis. In both of these fields Liebig was the great master. The formulæ which were calculated were, at first, of little value except to check the accuracy of the analyses and as a simple expression for empirical composition. I need not dwell on the confusion which existed throughout the first half of the century because there was no agreement as to the basis for molecular weights or atomic weights nor upon the large part played by the study of organic compounds in finally clarifying the view of chemists upon these matters. Yet, in spite of this confusion, two discoveries of fundamental importance date from this period: (1) That the empirical composition alone does not fix the nature of a compound, *i. e.*, the fact of isomerism; (2) that certain groups of atoms may remain together in passing from one compound to another through a whole series. The first fact furnishes one of the strongest reasons why an empirical formula for an organic compound is not enough, and the second fact furnishes the most important experimental basis at the foundation of our structural formulæ.

The studies of this period furnished a knowledge of the empirical composition of many natural products and of the products obtained from these by oxidation, reduction and the action of various agents. But while some might, perhaps, be inclined to look upon this mass of empirical knowledge as the most valuable acquisition of that time and to think that the theories in vogue were so imperfect or erroneous as to be of no value, such a view is certainly superficial. There were plenty of chemists in that day, too, who were ready to decry theories which seemed to them worthless, and it is interesting to read to-day what the great Laurent said upon this matter. He wrote in 1837:* "If I could believe that the purpose of my work was only to find a few new compounds or that it would end in my being able to say that there is an atom more or less in this compound or that, I would give it up on the spot. Only the desire of finding an explanation for some phenomena and of proposing some more or less general theories can give me the courage to follow a course in which I have found so little encouragement and where I have met with so many obstacles to overcome." Any one who has followed the story of how the older theories of radicals paved the way for the theory of types and of how the typical formulæ were so easily transformed into structural formulæ when the fact of valence was once grasped, can not fail to see that the larger and fuller view is an outgrowth from the earlier theories. And we must acknowledge that Laurent was right and that the theories upon which he was working were of vastly more importance than the mass of empirical facts which furnished him with their scaffolding.

Do not misunderstand me. There were two theories of radicals at that time—one which devised radicals in the study which should accord with the electrochemical theories held at the time and which did not attempt to secure evidence of their existence from the conduct of the compounds containing them, another which kept in much closer touch with the facts discovered in the laboratory. It was only the latter theory which contributed much to the growth of our knowledge. A theory which can not secure for itself a sound experimental basis is, of course, of only ephemeral value.

These, then, are the steps which have led to our present standpoint in organic chem-

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* *Ann. d. Chem.* (Liebig), **22**, 143.

istry: The discovery of isomerism, the discovery of radicals, the older radical theory, the theory of types, the establishment of true molecular weights, the discovery of the fact of valence, the determination of structure.

I think that all workers in organic chemistry will accept the following as a conservative statement of our present knowledge: (1) That in organic compounds, at least, each atom is attached *directly* to only a limited, small number of other atoms; (2) that in the sense of the order of the successive direct attachments the structure of a very large number of compounds is known with a degree of probability that amounts to practical certainty.

This brings me to the task which has been set, an attempt to outline the problems which lie before us in the further development of our science.

In the first place, there is still much to be done to extend our knowledge of compounds found in nature. This field is much less cultivated, relatively, than was the case sixty years ago. There has been good reason for this because of the problems of absorbing interest which have arisen in the preparation and study of new compounds and in the extension of our knowledge of old ones. But there must still remain many compounds to discover among both animal and vegetable products. On this side organic chemistry resembles the descriptive sciences of botany, zoology and mineralogy. And just as botanists think it worth their while to secure as complete a description as possible of the plants to be found on the earth, so it lies in our province to isolate and identify the carbon compounds of the animal and vegetable worlds—with the difference that in our case each compound, new or old, may be the starting point for the preparation of an almost endless number of others. But here most of us recognize that unless a compound has some further interest than that it is new it is not worth the time taken in its preparation. I am afraid, however, as we look over the pages of our journals, there is too much evidence that not every one lives up to this view. Our ever-increasing army of nascent doctors must needs have something to do, and it is so easy to make new compounds and so difficult to find something new of larger scope and really worth the doing.

There still remains much to do in the determination of the structure of compounds which have long been known. The study of a single compound often involves an incredible amount of work. Baeyer worked with indigo for fifteen years before his labors were crowned with a successful synthesis, and twenty years more and the work of very many chemists were needed before the scientific achievement could become a commercial success.

It was nearly twenty-five years after the first structural formula was proposed for camphor before Bredt was fortunate enough to suggest the true arrangement of its atoms, and it was ten years longer and required in all the work of more than fifty chemists before Bredt's suggestion was confirmed by Komppa's beautiful synthesis.

More than thirty formulæ were proposed for camphor, and those who think little of organic chemistry have some reason if they say that we jump at conclusions too hastily and propose too many formulæ that are mere guesses. Some might even say that the last formula isn't worth much, but those who have followed the matter know that step by step we have arrived at an almost positive certainty even in this complex problem.

The final solution of a problem with regard to the structure of a compound of natural origin is not usually considered to have been satisfactorily attained until its synthesis has been effected. Those who have attempted work of this character know that months or even years of work are frequently spent to obtain the synthesis of a single compound. In spite of the wealth of methods at our command—a wealth so great that it is often very difficult to select between several which are equally unpromising—it is evident that these methods of synthesis need improvement at many points. Not only do we need new and better methods, but many old methods require further study to disclose why they succeed in some cases and fail in others and to secure a fuller knowledge of secondary reactions which often occur. As recent remarkable achievements in this field of synthetic methods may be mentioned the brilliant results obtained by Grignard with magnesium compounds, Bouveault's elegant new solution of the old problem of transforming an acid into the corresponding alcohol and Scheuble's reduction of the amides of bibasic acids to the corresponding glycols.

Work along the lines suggested needs to be done in order to fill out and complete our knowledge in a systematic way, and occasionally work along such lines is rewarded by results of epoch-making significance, as when Gomberg discovered triphenylmethyl in his endeavor to prepare hexaphenylethane. Such work is not likely, however, to greatly advance our insight into the real nature of carbon compounds and we all feel that there are far more fundamental problems which demand attention.

As outlined above, the theories of valence and of structure now universally accepted imply a certain amount of knowledge of the arrangement of atoms in space. So far as the original and fundamental conceptions are concerned, however, this knowledge is quite vague. The much more definite conception proposed by van't Hoff and in a somewhat different manner by Le Bel is, of course, familiar to you all. In discussing any hypothesis it is always important to have clearly before us the facts upon which it is based. As I have already hinted, I believe that the theory of valence and the theory of structure in the sense of a sequence of atoms within the molecule are supported by our knowledge of such a vast accumulation of consistently interrelated phenomena that we are justified in believing that we have positive knowledge with regard to the structure of the molecules of organic compounds. I am as ready as any one to demand that every theory, no matter how old or how universally accepted, shall be continually brought back to the test of agreement with experimental facts, but I am not willing to admit that we may not, in the end, acquire positive knowledge by the process of inductive reasoning.

Assuming, then, the fact of a knowledge of the sequence of atoms in organic compounds, we have this basis for van't Hoff's hypothesis: (1) When four unlike atoms or groups are combined with a single carbon atom optical activity results in such a manner that there may always be found two compounds having identical sequence of the atoms within the molecule, and exactly equal rotary power, but of opposite signs. (2) That when two adjacent carbon atoms are combined each with three unlike groups, two compounds may result which, while optically inactive and having the same sequence of atoms, still differ in physical properties. An illustration of this is found in racemic and mesotartaric acids. (3) Rings containing five and six atoms are formed with especial ease, those containing three, four and seven atoms less readily, and rings containing more than seven atoms are scarcely known. (4) Derivatives of cyclopropane, cyclobutane, cyclopentane and cyclohexane having two substituents combined with different carbon atoms often exist in two isomeric forms in which the sequence of the atoms is the same. (5) Derivatives of ethylene often exhibit a similar isomerism.

Assuming as true that we have acquired a knowledge of the sequence of atoms in carbon compounds, the facts which I have enumerated lead almost inevitably to the corollary that the four atoms attached to a given carbon atom are arranged in approximate symmetry around the center of that atom for their position of most stable equilibrium. The relation between this conclusion and the theory of the sequence of atoms in carbon compounds, or what is ordinarily understood as structure, is very similar to the relation between the atomic theory and Avogadro's law. If we accept the atomic theory, there seems to be no rational escape from the acceptance of Avogadro's law. In a similar manner, if we accept the theory of the sequence of atoms in carbon compounds, there seems no reasonable possibility other than that van't Hoff's hypothesis is true in its broad outlines.

I hope I may be pardoned here for a brief digression. I am aware that Franz Wald* believes that he can give a satisfactory explanation of the laws of fixed and multiple proportion and of combining weights without the aid of the atomic theory, and that Professor Ostwald in his recent Faraday lecture† has accepted and expanded the same thought. I will say frankly that their reasoning does not appear to me conclusive. Ostwald defines a chemical individual as 'a body which can form hylotropic phases within a finite range of temperature and pressure,'‡ and deduces from this the fact that a given hylotropic phase must have a fixed composition. He appears to forget that the existence of these hylotropic phases implies that the properties of matter are discontinuous, or, in other words, that there is a finite number of hylotropic bodies, one of the facts for which the atomic theory gives an explanation.

There is another characteristic, too, of a chemical compound which all chemists will agree is at least as important as that it shall consist of a 'hylotropic phase.' This is that the compound must not only have a fixed composition, but this composition must bear a definite relation to those numerical quantities which represent the proportion in which each element of which it is composed always combines with other elements. I need hardly add that these numerical quantities are so deeply seated in the properties of matter that, having adopted a unit, all chemists are absolutely agreed in selecting one and only one such quantity for each of the well-known elements.

In attempting to deduce this law of combining weights Ostwald assumes that three elements form the compounds *AB, AC, BC* and *ABC*, and adds, 'There shall be but one compound of every [each] kind.' With this assumption, his reasoning may be sound, but I fail to see how it applies when we find ten thousand compounds *ABC* instead of one. The case which he supposes is so far theoretical that I have been unable to find an actual case where the compound *ABC* can be formed, by the union both of *AB* with *C* and of *AC* with *B*.* But I have

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* *Ztschr. Phys. Chem.*, **24**, 633, 1897.

† *J. Chem. Soc.* (London), **35**, 506.

‡ *Ibid.*, p. 515.

* It is quite possible that such an illustration may be found, but, in any case, Professor Ostwald's deduction can not be made to apply to those cases in which the compound *ABC* does not exist, nor to those cases where the compound *ABC* can not, even theoretically, be supposed to consist in turn of a known compound *AB* combined with *C* and of another known compound *AC* combined with *B*. Such cases are common because of the fact of valence. In its simplest form the law of combining weights is quite independent of the existence of the compound *ABC* and may be stated thus: If the com-

taken too much time with a matter which is aside from my main purpose. Before leaving this topic I must add, however, that I have used the phrase 'Avogadro's law' advisedly in spite of the fashion set by some chemists of calling it Avogadro's hypothesis.*

I remarked, a few moments ago, that the facts which have been outlined almost compel us to the acceptance of van't Hoff's hypothesis in some form. It is of the utmost importance for us to recognize, however, that we are here at the very confines of our present knowledge and that we must, at every step, bring ourselves back to the rigorous test of experimental fact. In accepting the hypothesis we are not compelled to consider molecules as set pieces of mechanism; on the contrary, there is strong reason for thinking that the positions assumed by the atoms are positions of dynamic and not of static equilibrium. While there have been many speculations in the matter, we have no strong reason for assuming, as yet, any definite shape for the carbon atom, nor even that there are within it definite points of attraction for other atoms. All that seems to be thoroughly established is that for their position of most stable equilibrium the four atoms or groups attached to a given carbon atom are arranged in approximate symmetry around its center. I say *approximate* symmetry because the existence of compounds containing rings of three and four carbon atoms demonstrates that the symmetry is not always absolute, and makes it probable that in cases where the four atoms or groups are unlike the symmetry is also imperfect. So far as I am aware, no fact inconsistent with this fundamental conception is known, while very many facts about optically active and cyclic compounds find in this conception the only satisfactory explanation which has thus far been given. It is true, also, that many facts with regard to optically active compounds indicate that when one group is exchanged for another the exact configuration is often retained, or, in other words, the entering group takes the same position with regard to the other three atoms or groups as was held by the group which was displaced. The manner in which it has been possible to work out, consistently, the complex relations between a considerable number of sugars, gives a very strong experimental basis for this statement. On the other hand, it is well known that such reactions often give racemic mixtures, which indicates that a shifting of groups with regard to a central carbon atom takes place much more easily than the shifting of a group from one carbon atom to another, at

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position of two compounds *AB* and *BC* has been determined, the composition of a series of compounds between *A* and *C* can be predicted and a compound which does not belong to this series has never been discovered. A still more general statement of the law, and one which includes, by implication, all of those facts which are used in the selection of atomic weights, is given above. In that form it is more properly called the law of atomic weights.

* Two reasons may be given for this usage. My own view is that we have, by a process of inductive reasoning, acquired such positive knowledge of the existence of atoms and molecules that the expression 'Avogadro's law' is fully justified. But even if we admit the contention of those who think that the atomic theory must always remain an unproved hypothesis, it is possible to frame a definition of the word molecule which would be merely a generalized statement of those empirical facts which lie at the basis of our atomic and molecular theories. Such a generalized, empirical definition must, of course, be very complex but it would not include the concept of discrete particles. Yet it will be still true of these empirically defined molecules that equal volumes of gases contain equal numbers under the same conditions of temperature and pressure. For instance, the term gram-molecule may be considered as a purely empirical generalization and it is true that a gram-molecule of one gas occupies the same volume as a gram-molecule of any other. But this is, in essence, Avogadro's Law.

least in saturated compounds. There are also a number of extremely interesting cases where a reaction gives rise to the optical antipode. Thus Walden has shown* that l-chlorsuccinnic acid is converted by silver oxide into l-malic acid, while potassium hydroxide converts it into the dextrorotatory acid. It is evident that in one case or the other there has been a shifting of the groups. Again Ascham† has shown that when d-camphoric acid is heated with hydrochloric and acetic acids it may be about half converted into l-isocamphoric acid, and that the latter suffers a similar transformation. This case is more complicated, as a 'cis' and 'trans' isomerism of cyclic compounds is involved as well as the optical difference. Not many cases of this character are known, at present, but such cases certainly deserve further study and must be reckoned with in considering the question we have before us. Le Bel‡ has already pointed out the theoretical significance of Waldron's work.

While we may feel that we have comparatively sure ground in the application of the theory of van't Hoff and Le Bel to optically active and to cyclic compounds, the case is quite different when we come to the consideration of what are commonly known as 'double' and 'triple' unions. Professor Michael has done a very great service to chemistry in showing that the supposition of a more or less definite tetrahedral shape for the carbon atom and of 'favored' configurations often leads to conclusions which are at variance with the facts. Philips§ and Blanchard‖ and myself have found a case in which the addition of hydrobromic acid to an unsaturated compound produces an optically active body which evidently has the same configuration as the amino and hydroxy acids from which the unsaturated body is formed by the loss of ammonia or of water. We have here, apparently, a potential asymmetry occasioned by the double union which it is difficult to reconcile with the prevailing conception of such unions. This case is complicated by the presence of a second asymmetric carbon atom in the molecule and is worthy of further study. Rabe and Billmann* have recently described a similar case, but very few instances of this kind are known.

Pfeiffer† has recently suggested a new interpretation of van't Hoff's hypothesis as applied to unsaturated compounds. Pfeiffer assumes that unsaturated compounds retain essentially the same configuration as the saturated compounds, from which they are derived. On this side his interpretation is closely related to the old theory of free valences, which, if I understand him correctly, is favored by Professor Michael. Pfeiffer also brings his interpretation into a close relationship to Werner's theory of inorganic metallic compounds. The most serious objection to the theory is that it supposes the existence either of trivalent carbon atoms or of free valences, in ethylene and its derivatives, an objection which has appeared to most chemists very strong in the past. Pfeiffer points out, it is true, that since the discovery of triphenylmethyl we can no longer deny the possible existence of a trivalent carbon atom‡. It would

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* *Ber. d. Chem. Ges.* **32**, 1855 (1899).

† *Ibid.*, **27**, 2004.

‡ *J. Chim. Phys.*, **2**, 344, 1904.

§ *Am. Chem. J.*, **24**, 428.

‖ *Ibid.*, **26**, 281; **27**, 428.

* *Ann. d. Chem.* (Liebig), **332**, 25.

† *Ztschr. Phys. Chem.*, **43**, 40.

‡ The fact that triphenylmethyl exists as a doubled molecule in solution should not, I think, lead us to discard the monomolecular formula for it any more than we consider that acetic acid has, in the ordinary sense of structure, a doubled molecule because it exists as a doubled molecule in solution in benzene or in the state of vapor just above its boiling point, nor because it forms acid salts. In these cases the chemical evidence ap-

seem, however, that the great difference between the intense chemical activity of triphenylmethyl and the comparative inactivity of ethylene demonstrates that, if the latter does in reality have free valences, the fact that there are two such valences reduces the activity of each enormously. The inactivity of carbon monoxide may be significant in this connection.

A more serious objection to Pfeiffer's hypothesis lies in the fact that he supposes so slight a difference in the configuration of fumaric and of racemic acids that it is difficult to see why the former as well as the latter might not be split into a pair of optically active bodies.

We must admit, then, that we have, at present, no satisfactory theory of double and triple unions and that we have here a problem which demands a large amount of further work before it is solved. When the solution is reached we shall probably gain a new insight into the perennial question of the structure of benzene, and our knowledge of tautomerism will cease to be, as it is at present, almost purely empirical. It is possible, perhaps probable, that Thiele's 'conjugated double unions' will contribute toward the solution.

While I have no comprehensive theory with regard to double unions to advance, I will, with a good deal of hesitation, venture to express some thoughts with regard to the combination of atoms in general which have some bearing on this question. We are all familiar with Faraday's law that if a current of electricity is passed through a number of cells filled with solutions of different electrolytes and arranged in series, exactly equivalent amounts of the various components will be liberated at the electrodes in the successive cells. The beautiful experiments of Professor T. W. Richards have demonstrated that we are dealing here with a law which is true for different solvents and over a wide range of temperature; and also that the law is true with a degree of absolute accuracy which is of the same order as the laws of the combination of elements by weight. We are compelled, then, to believe that there is associated with each valence of an ion as it is transported through a solution, or at least as it separates at an electrode, a quantity of electricity which is invariable and independent of the nature of the ion. In other words, we have here a natural electrical unit which can be defined in its relation to atomic weights with a degree of accuracy which seems to be limited only by the refinement of our manipulations.

It is not always recognized as clearly as it should be that this unit quantity of electricity which is associated with one valence of any ion is not a unit of electrical energy. If it were, the same energy would be required to decompose the equivalent quantity of one electrolyte as of every other, which is manifestly not true. While the same current causes the separation of equivalent quantities in the different cells, the differences of potential, and so the amounts of energy required for the separation, vary greatly. It is evident then that when we say that a unit quantity of electricity is associated with each valence of every ion we do not use the term *quantity* in the sense of quantity of electrical energy. Instead of this, when this conception of a unit quantity of electricity is examined it will be seen that it is a conception of something whose properties are those of matter rather than those of energy. The facts appear to be consistent with the idea that the unit quantity of electricity of which we are speaking is of a material nature and you have doubtless already perceived that

pears to be more important and more conclusive than the physical. It is probable that the doubled physical molecule is the result of forces which do not produce a stable structure in the ordinary sense.

I have the theory of electrons in mind. The ingenious experiments of J. J. Thompson have given us considerable reason for thinking that the negative electrons are capable of an independent existence and have also given a probable estimate of their mass, which is small in comparison with the mass of the hydrogen atom.

It has been customary to think of the unit charge of electricity as being involved only in those reactions which occur in solution. If, however, we accept the theory of electrons it is evident that the electrons must be present in the molecule of an electrolyte no matter in what manner it is formed. It is but a step further to the conclusion that the electrons are involved in every combination or separation of atoms and, indeed, may be the chief factor in chemical combination.

Professor Kahlenberg* has shown that a practically instantaneous reaction takes place between hydrochloric acid and copper oleate in a solution in dry benzene, although the solution does not conduct an electric current and there is no evidence of the dissociation of either the copper oleate or of the hydrochloric acid. Professor Kahlenberg points out very justly that there is no apparent difference between these reactions and those which take place in aqueous solutions, where we have much independent evidence of the existence of ions. He draws the conclusion that no ions exist in either case. It would seem that we are equally justified in supposing that a substance not already in the form of ions may separate into them under the influence of a second substance with which it can react.

Some time ago Mr. Lyon and myself† showed that the primary reaction between chlorine and ammonia gives nitrogen trichloride, nitrogen and hydrochloric acid, and that these products are formed in such proportion as to lead to the conclusion that three molecules of ammonia react simultaneously with six molecules of chlorine. It was pointed out at the time that the simplest explanation of this result is to be found in supposing that chlorine atoms separate during the reaction into positive and negative ions, while the ammonia separates partly into positive nitrogen and negative hydrogen and partly into negative nitrogen and positive hydrogen.* This hypothesis has met with some approval,† but has also received the criticism that such a dissociation as is supposed would result in the spontaneous decomposition of ammonia into nitrogen and hydrogen.‡ This criticism loses its force if we suppose that the separation into ions takes place only under the immediate influence of the chlorine with which the ammonia reacts. It has been pointed out by many different authors§ that a separation of atoms from each other must occur either before or at the same time that they enter into combination with other atoms. The only part essentially new in the hypothesis proposed is that this separa-

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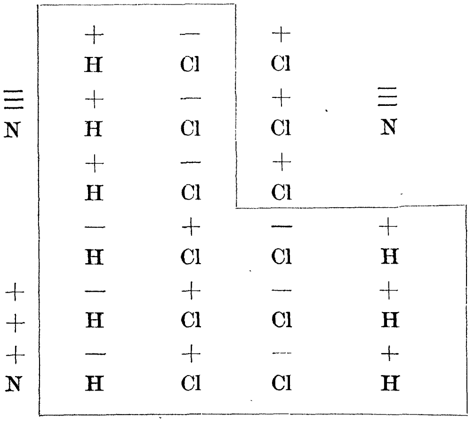
* *J. Phys. Chem.*, **6**, 1.

† *J. Am. Chem. Soc.*, **23**, 460.

* This was represented graphically thus:



† Stuglitz, *J. Chem. Soc.*, **23**, 707.

‡ *Ztschr. Phys. Chem.*, **41**, 378.

§ See Erlenmeyer, Jr., *Ann. Chem.* (Liebig), **316**, 50.

tion is into positive and negative parts and that the same atom may be sometimes positive and sometimes negative. The idea of a dissociation which occurs under the influence of a reacting substance appears to be implied in a part of Professor Nef's discussion of methylene dissociation, but it is not always clear whether he has in mind chiefly a dissociation of this sort or one which is independent of the interaction of different compounds.

The thought that the same atom may be at one time positive and at another time negative is related to the older electrochemical theory which supposed water to be positive in acids and negative in bases.

We assume, then, that in every combination of atoms each union involves an attraction between the positive and negative electrons which are associated with the two atoms that unite. In saying this I do not lose sight of the fact that such a thing as attraction *per se* in the sense that one body can influence another at a distance without an intervening medium is, apparently, inconceivable. I think of the attraction as probably caused by some motion of the electrons which enables them to act on each other through the aid of the ether. It is convenient, however, to speak of this effect as an attraction, since our conception of its real nature is, of necessity, very vague. One advantage of the idea that the attraction of the electrons is of a kinetic nature is that we may conceive of the same electron as becoming positive or negative, according to the nature of its motion.

The common conception, at present, is that an atom which has lost an electron becomes positive, while either the electron in its independent existence or the atom to which it is attached becomes negative. So far as I am aware, it has not been pointed out that this view leads to the conclusion that the same atom must, under different conditions, have a different weight. Thus a bivalent copper atom which has lost two electrons must weigh less than a univalent copper atom, which has lost only a single electron. It is true that our methods of determining atomic weights are scarcely accurate enough to detect differences of this order. The suggestion which is made is that the electrons of two atoms which are united have motions which correspond to positive and negative charges, respectively, and that when the atoms separate these motions may be retained, or lost, as in the case of a mercury atom which is uncombined, or that the motions may be reversed. In accordance with the hypothesis outlined above, we must assume that when two atoms separate either one may become positive; dependent partly on their nature, partly on the nature of the reacting substance. The conception here proposed is that of something very similar to the action of the pole of a magnet, which may attract another pole of the opposite kind, or induce the formation of a pole of the opposite kind, or it may reverse the polarity of another magnet.* This is, perhaps, simpler than to suppose the transfer of an electron from one atom to another in those cases where the electrical charges of the atoms are reversed in the ionization. A very accurate determination of the atomic weight of cupric copper as compared with that of cuprous copper might possibly decide between the two hypotheses.

It should be noted that the hypothesis that the electrical charges associated with the atoms are of a kinetic nature, and that these charges may be transferred without gain or loss of matter, is quite independent of the first hypothesis, which is that the

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* This is, of course, only an analogy and must not be pressed too far; just as the electrical changes of atoms or ions conduct themselves very differently from those of masses. The latter divide themselves between two bodies in contact; the former may be transferred *completely* from one ion to another.

atoms are ionized when they separate from each other and that the same atom may become either positive or negative.

In following farther the thought of the attraction between electrons as the cause of chemical combination, we must suppose that in addition to the effect of this attraction in holding together the atoms which are immediately attached there is a residual effect upon other atoms within the molecule. This gives a rational explanation of the very great difference in the stability of the union between carbon atoms in different compounds as, for instance, the instability of acetacetic acid in comparison with butyric acid, occasioned by the substitution of an oxygen atom for two hydrogen atoms of the latter. The study of organic compounds has given us a knowledge of a large number of cases of this sort and our text-books contain many empirical rules about them, but there have been few, if any, attempts to give for such facts any rational explanation.

In considering double unions three explanations suggest themselves: (1) We may suppose with Pfeiffer that such unions are in reality single unions and free valences. In this case the presence in adjacent carbon atoms of positive and negative electrons which are uncombined would reduce the attraction of each for the electrons of another molecule, thus explaining why two free valences are so much less active than a single one. (2) We may suppose that the carbon atoms are in reality doubly united, but that, owing to the localization of the electrons in definite parts of the carbon atoms, the four electrons involved can not approach as near to each other as is the case in a single union. This is Baeyer's theory of strain and is much better in accord than is the theory of free valences with the fact that cyclopropane and propylene appear to be about equally unsaturated as evidenced by their heats of combustion and by their conduct toward bromine. On the other hand, it seems to lead logically to conclusions with regard to the addition of bromine to triple unions which Professor Michael has shown are contrary to the facts. (3) Without a condition of strain, we may suppose that the presence of both a positive and a negative electron in each of the atoms united by the double union causes a lessening of the attraction of the electrons. This would result in such a union being less stable than a single union. The second and third views appear, at present, most in accord with the facts—possibly the truth lies in some combination of the two.

Whatever view we may take, it is noteworthy that double unions are usually formed by the loss of a positive and negative atom or group from adjacent carbon atoms, as hydrogen and hydroxyl or hydrogen and bromine. It is also true that in many double unions one of the carbon atoms is more positive than the other, causing the addition of halogen acids in a definite manner which may be predicted in accordance with Michael's 'positive negative law.' Applying this thought to conjugated double unions, we see that of the four atoms involved the two central ones are likely to be positive and negative respectively and neutralize each other's attraction for outside atoms, while an intensified attraction for outside atoms would be found in the exterior atoms. The effect may be analogous to that of the attractive forces of a magnet which exhibit themselves chiefly at the ends.

But I have permitted myself to wander much farther in the field of speculation than was my first intention—farther than is at all profitable, I fear, for these questions furnish, at present, few points for experimental study, and speculations divorced from experiment have usually been profitless. I should be very sorry if what has been said should give encouragement to such speculations. On the other hand, I have a very firm conviction that we should not be content with rounding out organic chemistry as a descriptive science nor even with adding to the number of empirical rules which enable us to predict certain classes of phenomena. We must, instead, place before ourselves the much higher ideal of gaining a clear insight into the nature of atoms and molecules and of the forces or motions which are the real reason for the phenomena which we study. When we consider the progress which has been made and the knowledge of structure we now possess, which would have appeared sixty years ago to lie beyond the limits of possible acquirement, it is not presumptuous to think that a more complete knowledge of these questions will at some time be gained. This fuller knowledge will take account, too, of many lines of work upon which I have no time to dwell, such as the question of changing atomic volume to which Professors Richards and Traube have directed our attention, and the knowledge of heats of combustion, of molecular refraction and dispersion, of color, viscosity, dielectric constants and other physical properties. The future must give to us a new theory or a development of old ones which shall include all of these phenomena in one comprehensive view.

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SCIENTIFIC BOOKS.

MUSEUMS AND MUSEUM APPLIANCES.

The Museum. By L. P. GRATACAP. Reprinted from the *Journal of Applied Microscopy and Laboratory Methods,* Vols. V. and VI.

Bericht über einige Neue Einrichtungen des Königlichen Zoologischen und Anthropologisch-Ethnographischen Museums in Dresden. Von Dr. A. B. MEYER. 4to. Pp. 25; pls. I.–XX.

The first of these is a reprint of Mr. Gratacap's series of papers which appeared in the *Journal of Applied Microscopy* and is a pamphlet of about 100 pages with many illustrations. In book form it would make a good-sized volume and as these articles contain a good résumé of the principles of museum construction and methods of installation, it is a pity that they could not have been issued in such shape. Mr. Gratacap is well qualified to treat of museum matters, and since there is not space to note all the good things he says, it must suffice to discuss a few concerning which there may be room for a difference of opinion. The first is to be found almost at the beginning, where Mr. Gratacap makes a plea for a uniform system in museum methods. The field covered by museums is so vast and the educational features at present so little developed that there is ample scope for diversity in the treatment of museum exhibits, the more that those features that are universally good can only be ascertained by experiment, to say nothing of the fact that the ends sought for may be very different in different museums. To illustrate this we may consider the questions of the display of skeletons and of fossil vertebrates. If the aim is to show the structure and relationship of vertebrates as a whole the two should be combined, the number of specimens should be limited, and fragmentary fossils almost entirely excluded, this being a case where a cast or good drawing is much better than an imperfect specimen, since it is necessary to compare animals in their entirety. If the object is to show the succession of life on the globe, then the fossils should be arranged zoologically under their respective geological periods, so that the visitor can see the successive steps by which the present fauna of the globe has been reached. Still a third arrangement of fossil vertebrates is possible, that adopted by Professor Osborn, of showing by numerous specimens the phylogeny of various groups. If it is desired to show the structure and characters of vertebrates, then skeletons and other anatomical preparations may be placed with mounted animals. No one museum can do all these things and each institution must decide on the plan that best suits its circumstances.