SCIENCE

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*THE DEVELOPMENT OF CHEMISTRY.**

THE American Chemical Society exists for the advancement of chemical science, and the betterment of the chemical profession. Every member of it is supposed to contribute his share of thought and energy to the accomplishment of these ends; and so its work is prosecuted along many lines of activity. During the past ten years the growth of the Society has been most remarkable, and the diversity of its interests is well shown in the pages of its *Journal*. The once doubtful experiment of organization has justified itself by success, and there are no longer any apprehensions as to the future. The Society now stands before the world well established, well recognized, active and vigorous; its days of weakness and danger are over; we can look forward with confidence to greater prosperity, to larger growth, to steadily increasing usefulness. All chemistry is our province, whether it be organic, inorganic, theoretical, physical or applied; and the narrowness of specialism finds its best antidote in the varied interests of our meetings. To promote science and to uphold the dignity of our common profession are the objects which bind us together.

Optimism is a good thing, but it needs to

* Presidential address delivered at the Philadelphia meeting of the American Chemical Society, December 30, 1901.

be tempered by reason. Hopefulness and enthusiasm are fine qualities, but the restraint of common sense should keep them within bounds. Too much complacency is dangerous, and on occasions like this we may well pause in our gratulations over past achievements, to ask ourselves whither we are tending. As chemists, we owe something to the science which we represent, and the debt is one which can never be discharged absolutely. That we have done much is evidence that we can and should do more; as a society and as individuals we may well look about us and strive to see which way the path of duty lies. We cannot appraise the future, but we must help to make it. Only by acting with intelligent forethought can we hope to advance creditably.

Retrospection is the one safe basis for prophecy. The history of science is full of suggestions for the days to come, and even if we do no more than to avoid the repetition of mistakes, we shall gain much from the study. Great as the past has been, we can make sure of something better still, looking confidently forward to more perfect knowledge, to larger opportunities for research and to wider recognition in the republic of learning. Let us see how chemistry has developed hitherto, and how we can improve her present condition.

A little over a century ago chemistry was hardly more than an empirical art—a minor department in the broad field of natural philosophy. There were no chemists in the professional sense of the term, and no laboratories worthy of the name; that is, no buildings were planned and erected for chemical purposes alone; but chemical investigations were conducted in any room which happened to be available, with a disregard for convenience which would be intolerable to-day. Even at a later period the marvelous researches of Berzelius were performed in a laboratory which was essentially a kitchen. If we use the word in its true sense, the earlier chemists were amateurs; that is to say, men who labored for the love of truth and without ulterior professional motives. Priestley was a clergyman, who regarded his voluminous theological writings as more important than his contributions to science. Scheele was an apothecary; Lavoisier was a public official with multifarious duties; Dalton was a schoolmaster and arithmetician. Before these men and their contemporaries, a vast unexplored territory was outspread; and no one could suspect what hidden riches might lie beneath its surface. Lavoisier, with his emphasis upon quantitative methods; Dalton, with the atomic theory; Davy, the discoverer and definer of elements; and Berzelius, with his genius for system and his untiring industry in the accumulation of details, opened the main roads into the new empire. Specialism in chemistry was practically unknown; all portions of its domain seemed to be equally inviting; but inorganic problems were perhaps the most obvious, and, being easiest to grasp, received the greater share of attention.

There were, from the beginning, two great stimuli to chemical research; the intellectual interest of the problems to be solved, and the practical utility of many discoveries. Both forces were essential to the rapid development of our science; neither one alone would have been adequately effective. Economic considerations, taken by themselves, help but little towards the symmetrical organization of scientific knowledge, for the practical man has usually a limited, although very direct, purpose in view, and may not wander far from his main issue. On the other hand, the purely scientific investigator can rarely exercise his full powers without a certain measure of popular support and encouragement, to which the expectation of usefulness contributes. That discovery must precede application is obvious; that systematic knowledge outranks empiricism is also clearly true; but theory and practice react upon each other, and it is only when they work harmoniously side by side that the best results are attainable. The purist in science too often overlooks this fact, and fails to recognize his enormous debt to industry. The commercial demand for chemical data was an important factor in the establishment of our profession, and from it we derive a large part of our resources. At bottom, however, the demand is essentially selfish; and the manufacturer who seeks chemical aid, nay, even the technical chemist himself, is not uncommonly forgetful of his obligations to pure research. Every chemical occupation is based upon discoveries which were made without thought of material profit, and which sprang from investigations undertaken in the interests of truth alone. Even theory, which the ignorant worker affects to despise, has its place in the economic world, and the indebtedness of the coal-tar industry to Kekulé can hardly be overestimated. Without theory science is impossible; we should have, instead, only a chaotic anarchy of disconnected facts, a body without a soul. Theory is to science what discipline is to an army; it implies system, method and the intelligent direction of affairs; it is the coordination of knowledge, through which the experience of others becomes best available to us. The victories of research are rarely accidental; if they were, then the untrained tyro would have an equal chance of success with the greatest masters. Among ourselves, these considerations may be commonplace, but they are opposed by certain popular misconceptions which hinder our advancement and work mischief to our cause. *Cui bono* is the one question which science cannot ask.

Four agencies have been chiefly instrumental in building up the chemical structure of to-day, namely, private enterprise, the commercial demand, governmental requirements, and the extension of scientific teaching in the universities. Under the first of these headings the foundations of chemistry were laid, and the researches of Cavendish upon the composition of the atmosphere, may be taken as types of the class. Unfortunately, however, the men who combine the requisites of wealth, leisure, the inclination and the ability for scientific investigation are few in number, and the output of their labors is relatively small. Still, we must admit that the work so accomplished is often far above the average in quality, and that if it were to cease, our science would be much the poorer. Its motive is always high, and unaffected by any annoying pressure from necessity; its objects are purely scientific.

Seen from the commercial side, chemistry presents quite another aspect. Questions of utility are now paramount, and the advancement of science as such has become a secondary affair. The manufacturer seeks to improve his products or to cheapen his processes, and calls for information which shall enable him to do so; specific industrial problems require immediate attention, and each one is taken by itself, regardless of its broader philosophical bearings. From these conditions a certain narrowness must follow; no time can be wasted over considerations not directly related to the matters in hand, for the success or failure of a great enterprise may depend upon the quickness with which the obviously essential work is done. As against this urgency of demand, no just criticism can be offered; we may only ask that it shall be reasonable, and that science shall be treated less as a servant, and more as a faithful ally. The commercial chemist owes something to his profession, as well as to his employer; and his industrial duties ought not to be incompatible with his responsibilities as a scientific man. The education of the manufacturer is one of the functions which he has to perform, and it is one which is not always easy of accomplishment. Two points of view have to be reconciled; self-interest is on the one side, the benefit of science on the other.

Several difficulties beset the pathway of applied science, and interfere with the work of its practitioners. The limitations of the field have already been suggested; but a more serious obstacle to progress is found in the secretiveness of the employer. The industrial chemist can not publish his researches, or at best can publish little; he therefore fails to receive before the world the credit which is his due, and science as a whole is the loser. A secret process, an unpublished investigation, adds nothing to the sum of human knowledge, and it represents a policy which is both short-sighted and unwise. It often covers ground which has been well covered before, and in that case it stands for misdirected effort, for wasted energy. I have seen, under the seal of confidence, a 'secret process' which had been in print for twenty years; its too practical inventor, ignorant of the literature of his subject, had worked out his methods independently; had he consulted others, he might have saved both expense and time. On still broader grounds I believe we may claim that the publicity of science is more economical than the current exclusiveness. Where several competing establishments produce the same class of goods, each one tries to hide its workings from the others. Each, therefore, gains only that new knowledge which it can develop by itself, whereas with greater wisdom it might profit by the experience of all. Secrets will leak out, in spite of precautions; a full interchange of thought merely anticipates the danger, and at last the manufacturer may find that instead of suffering loss, he has really received much for little. Possibly the combination of industries under the so-called 'trusts' may act favorably upon scientific research, for when rivalry ceases, the incentive to secrecy disappears also.

If we study the reaction between science and industry at all closely, I think we shall find that an economic revolution of remarkable importance is well under way. Like all the greater social movements, it is going on quietly, without noise or bluster, but it is nevertheless far-reaching in its effects. Manufacturing, once a matter of empirical judgment and individual skill, is more and more becoming an aggregation of scientific processes, a system in which accurate quantitative methods are replacing the old rules of thumb. Exact weight and measure are taking the place of guesswork, and by their means waste is diminished and economy of production is insured. I can remember the day when few establishments in America gave regular employment to chemists; now laboratories are maintained in connection with nearly all productive enterprises, and the demand for scientific service, which was formerly sporadic, has become well-nigh universal. A railway system, making contracts for supplies, does so upon the basis of chemical reports; and the work is performed in its own offices by experts who are permanently retained. In the management of an iron furnace, ore, flux, fuel and product are analyzed from day to day, by methods of amazing rapidity and considerable exactness. Fertilizers are sold upon chemical certificate after preparation under chemical rules; sugar is refined by chemical processes, and taxed according to chemical standards; medicine is enriched by new remedies of chemical origin; in short, our science touches every productive industry at many points, and aids in its transformation. Metallurgy is becoming more and more a chemical art; photography, a modern science, rests upon chemical foundations; with the aid of the electric furnace new chemical industries are springing into existence; and every one of these agencies reacts upon the chemist, by increasing the demand for his services and his wares. In Germany this development of applied science has gone the farthest; and in that country a single establishment may employ from fifty to more than a hundred chemists in its regular work. Some of these men are analysts merely, but others are engaged in systematic research, which has both science and industry in view. This appreciation of research as such is something to which few of our American manufacturers have attained; and it marks the highest step yet taken in the line of industrial progress. The modern era began when hand labor, which means individualism, gave way to machinery; but the machine is a symbol of organized intellectual power, and science is the bed-rock of its foundation. Chance and supposition are out of place in the industrial world of to-day.

Turning now to the governmental side of science, we find that the services of the chemist are everywhere in demand. Every civilized government now maintains chemical laboratories, and for purposes of the most varied kind. The accuracy of the coinage is determined by the assayer; supplies for public use are tested by analytical methods; taxes are assessed in terms which need chemical interpretation; the armor of the battleship and the explosive of the torpedo depend for their efficiency upon the skill with which our work is done. The sanitation of cities; their water supply; the disposal of sewage; the effectiveness of antiseptics; the quality of gas for lighting or of asphalt for paving; the warfare against the adulteration of food—all of these questions are essentially chemical in character, and are, or should be, settled in the official laboratory. The aggregate of this work is something enormous; and yet, like commercial chemistry, it has utility, not science, in view. Science may advance because of it, but that is not the main purpose; the application of existing knowledge to public uses, and the creation of new knowledge are two distinct things. Here again chemistry is a servant, nothing more.

Throughout the scientific bureaus of the government this secondary character of chemistry appears. In the Geological Survey it is an aid to geology; in the Department of Agriculture, agriculture is to be advanced; in the medical service of the army or the navy, the interests of medicine come first. Chemistry for its own sake has as yet little or no governmental support; astronomy is encouraged, geology receives assistance, the biological sciences are given opportunities for growth; but our profession is merely utilized, without thought of its significance, its laborers being too often overworked and underpaid.

In an incidental way, however, the governmental laboratories accomplish something for pure science, albeit with little direct encouragement and in spite of difficulties. The official chemist, unlike his commercial brother, is not always crowded for time; his work can be done in a somewhat more leisurely manner, for it is unaffected by any demand for immediate financial returns; and so abstract researches, if they bear in any way upon the problems which are assigned him, are sometimes within his reach. Chemistry owes much to investigations of this class; and the papers which issue from official laboratories are by no means to be despised. Good work is done, but there ought to be more of it; research should become a recognized duty, rather than an employment for spare time. It would be well if every government could be made to see that the use of science implies the encouragement of science; for then we might hope for the establishment of laboratories for purposes of investigation alone. To this proposition I shall recur later.

We now come to the fourth of the agencies by which chemistry has been developed, the educational, and this is the most important of all. Scientific research has become a definite function of the modern university, wherein the creation of knowledge is given equal rank with the distribution thereof. Education to-day differs from the education of former times, in that a lower place is given to mere authority; it goes more to the foundation of things, and so secures a foothold from which it can build much higher. Research, both for its own sake and as an example to the student, is now expected of the teacher; his pupils, coming face to face with the limitations of knowledge, are shown the problems which demand solution, and are taught something, by practice and by precept, of the manner in which they can be solved. The student learns that science is a living growth, and that every earnest, sincere, well-trained scholar can do something towards its development. If we examine the chemical journals of the nineteenth century, we shall find that by far the larger part of the discoveries therein recorded were made in the laboratories of universities or schools. Even in our own journal, with all its contributions from technical and official sources, over sixty per cent. of the communications published are of this class. The significance of this fact, however, must not be overestimated; we should remember the restrictions under which the technical chemist labors, whereas to the university professor publication is almost as the breath of life. His professional standing, his chances of promotion, are profoundly affected by the amount and character of the work which he puts forth; silence, to him, means the possible reproach of inactivity; he must publish or remain obscure. Furthermore, we must not forget that the teacher owes a debt to technology which can never be repaid. The commercial demand for applications of science has enlarged the field of education, by compelling the establishment of polytechnic schools. These institutions, all of them of recent date, give employment to thousands of instructors; they supplement the universities, they multiply the facilities for scientific work, and from them, too, there flows a steady stream of contributions to knowledge, to which the chemist is adding his full share.

Apart from the freedom to publish, the university teacher has one great advantage over the technical man. He is not confined to any limited field of operations, such as the chemistry of soap, or iron, or coal-tar; the whole domain of the science lies open before him to explore where he will. The possible utility of the work need not occupy his mind; he can attack any problem he chooses, and from any point of view. And yet, with all incentives to breadth, his researches may still be tainted with narrowness, for the inevitable tendency to specialize puts its restrictions upon him. It is much easier to be a physical chemist, an organic chemist, an agricultural chemist or an analyst, than it is to be a chemist; and chemists, in the larger sense, are few. It was Berzelius, I think, who said that he was the last man who could ever know all chemistry, and the saying was both wise and true. Sixty years ago our science could be mastered in its entirety by one industrious student; to-day it is so vast that subdivision is necessary. Still, special research is not incompatible with breadth of view; every chemist should understand the nature of the great central problems; he should stand high enough to overlook the field, no matter how small a corner of it he may prefer to cultivate personally. Broadness of mind does not imply a scattering of resources, a futile waste of opportunity; it means an intelligent appreciation of all good scientific work, whether it be within our own bailiwick or elsewhere. To exalt one specialty at the expense of others, to claim supremacy for our own small interests, indicates a self-conceit which is both mischievous and absurd.

With so many opportunities for research, and with numberless problems in sight, chemistry should have grown according to some law of symmetry, giving us to-day a well-balanced and harmonious whole. History, however, tells a different tale. The science has expanded enormously in some directions, and advanced slowly in others; a glaring disproportion is the result. For this condition of affairs there are two reasons: lack of coordinated labor and the influence of fashion; for there are fashions in thinking, just as there are in dress, and only the most original minds can escape from their domination. Theoretically, every investigator is free to follow his own bent; practically, his course is shaped by a complexity of circumstances. The line of least resistance is the easiest line to take, and in science that is determined by temporary conditions. Certain researches have been fruitful; and so, like miners flocking to a new camp, we are tempted to enter the same field, rather than to play the pioneer elsewhere. The greatest prospect of immediate success is the power which attracts us. Through influences of this kind chemistry has developed unevenly, with one side over-cultivated and another suffering from neglect.

To illustrate my meaning. I do not wish to underrate the importance of organic chemistry, nor to question, in the smallest degree, the value of its achievements. Its interest, its attractiveness, the beauty of its methods, its profound influence upon chemical theory, are all admitted; and yet it has received, it seems to me, an undue share of attention. During fifty years a large majority of all chemical investigators devoted themselves to this one branch of chemistry, leaving only a few workers to occupy other fields. Organic chemistry was the fashion; in it reputations were easiest made; the great professional prizes, the best positions, went to its devotees.

Now, in spite of all that organic chemistry has accomplished, we may fairly admit that chemical research should have a broader scope. Carbon is but one element among many; and all must be considered before we can be sure that our interpretations of chemical phenomena are sound. Special cases are easily mistaken for general laws; and to such errors we become liable when we confine our studies within too narrow bounds. Fortunately for chemistry, a broadening process has begun; and the prospects for the future are most encouraging.

During the past ten or fifteen years two movements have gained headway in the chemical world. One is marked by the revival of interest in inorganic problems, the other by the development of physico-chemical research. To a certain extent the two have much in common; each one is aided, I might say fertilized, by conceptions borrowed from the organic field; both are already fruitful to a remarkable degree. Independent journals devoted entirely to inorganic or physical chemistry, have come into existence, and investigators of the highest rank fill them with contributions. It is not my purpose to discuss either movement in detail; I mention them as symptoms of a more liberal spirit in research, as indicating the commencement of a new era. Physical chemistry in particular is becoming the center of interest; laboratories are built and equipped for its benefit alone; it bids fair to surpass even organic chemistry in its dominion over chemical thought. One danger, however, confronts it—the danger of self-exaggeration, stimulated by over-popularity. Physical chemistry, to achieve the best results, has need of data drawn from other lines of chemical research; if they are neglected, it in turn will suffer. Even now too large a proportion of its votaries are working in one field; that is, on questions growing out of the current theory of solutions, and other subjects fail to receive the attention which they deserve. This state of affairs, this lack of proportion, is doubtless only temporary, for towards physical chemistry all chemical theories converge, and no phase of it, therefore, can long escape consideration. The very nature of physical chemistry implies the prohibition of narrowness; broad conceptions and deep insight are essential to its being.

When we consider the complex influences, the varied demands, through which chemistry has developed hitherto, we can only wonder at the outcome. Under the circumstances, a symmetrical growth was impossible; the marvel is that so much could have been accomplished. Out of unorganized, uncoordinated, individual efforts a true science has come into existence, equal in dignity to any other within the domain of learning. All science is defective, but in its very imperfections we find its greatest charm. Through them alone effort becomes possible; a wise discontent on our part is the first condition for progress. If all were known, research would come to an end; nothing could arouse our curiosity; the human mind would atrophy for want of exercise. The search for truth is better than the truth itself—if I may be allowed thus to paraphrase the well-known words of Lessing. In what direction, then, shall we pursue our search, and with what promise for the future? What are the needs of chemistry?

Pardon me, now, if I apparently indulge in commonplace; if I cite some considerations of almost alphabetic simplicity. Fundamental principles lie so close to our eyes that they are easily overlooked; and from negligence of that kind, misdirected effort may follow. We must review our lessons sometimes in order to make sure of what we really know. In the first place it is well to bear in mind that chemistry and physics are not sharply distinct; that they are two parts of the same great body of truth; and that neither can be studied to the best advantage without aid from the other. Both rest upon the same two basic doctrines—the conservation of energy and the persistence of matter—conceptions which supplement each other and which give our work its philosophical validity.

If we try to consider chemistry by itself, to conceive of it as an independent branch of learning, we shall find that it has but one fundamental problem, namely, the study of chemical reactions. From certain kinds of matter certain other kinds are produced; and we merely investigate the laws which govern the transformations. If we prepare new compounds, we discover that such and such reactions are possible, and we describe their products. If we are interested in chemical equilibrium, we seek to determine the limits between which a given change can occur. Even our notions of chemical structure and atomic linking are but devices through which reactions and their products may be coordinated. In every case the reaction is the ultimate object of purely chemical research, and we try to ascertain its laws. Beyond this we enter the realm of physics; we describe each kind of matter in thermal, optical, electrical, mechanical and gravitational terms, and we discuss the phenomena of chemical change in similar phraseology.

Let us take, for example, any reaction whatever, and see what its *complete* investigation signifies. At once the problem will resolve itself into four parts, two statical and two dynamical, not one of which can logically be neglected. First, there are the substances which enter into the reaction; secondly, the physical stimulus, thermal, electrical or actinic, which starts the reaction; thirdly, the phenomena which occur during the reaction; and finally, the substances produced by the reaction. An initial state of equilibrium is disturbed by some application of energy; transformations of energy take place, and in a final state of equilibrium the process comes to an end. Through a mixture of gases having certain physical properties we pass an electric spark; they unite to form a liquid with different physical properties, the process being attended by a change of volume and great evolution of heat. The fact of union is chemical; the other phenomena are physical; and the two sets of considerations are so interlaced that we are compelled to take them together. Intellectually we can discriminate between them, but the line of demarcation is essentially ideal. The chemical composition of matter cannot be studied apart from its physical relations, nor discussed without the aid of physical terminology.

It is easier to preach than to practice; to say what should be done than to do it. Between the theoretical statement of a problem and the practical method by which it may be solved there is a profound gulf, over which a direct passage is perhaps impossible. No reaction has yet been exhaustively studied on the lines which I have laid down, and possibly none ever will be, for the difficulties in the way of such a research are almost insuperable. Of all the snares which nature sets before our unwary feet, that of apparent simplicity is the most deceptive. Honest complexity, evident at sight, we may hope to overcome; it is the unseen obstacle which baffles us. In the present instance a prime difficulty is the definition, the isolation of a reaction by itself, apart from other chemical changes. Nearly every reaction which we can observe is, in reality, a complex of several reactions—a series of steps, some of which may easily escape our notice. We measure certain phenomena, only to find at last that our result is an algebraic sum, and that we have more unknown quantities than equations. We cannot solve our problem until these factors have been recognized and separated.

To study individual reactions, then, except for the determination of definite, special phases, is not the best mode of procedure; chemistry would advance but slowly were we restricted to such a method. In ordinary chemical research, in the work of the compound-maker, for example, the initial and final stages of a series of reactions are investigated, and in that way valuable data are obtained. But the aim of science is not so much to amass facts as to connect them by laws and principles; and the more general the latter become, the greater is their intellectual value. We can not build, of course, until we have the materials, but between brick-making and architecture the difference is great indeed.

Leaving now the apparently simple, and turning to the visibly complex, let us see whether we cannot attack all reactions collectively, and in that way reach a more general statement of our real experimental problems. All reactions display the same fundamental phenomena, namely, changes of composition, changes of properties and transformations of energy; if we can classify our data under these categories, we shall begin to see more clearly the road we are to follow.

Now, recurring for a moment to the analysis of a single reaction, we may consider its two statical terms, the nature of the substances with which we begin and end. In any particular instance these questions are special and limited; but through them we discover facts which may be grouped with others of like kind. Presently we shall reach the discrimination between elements and compounds; and sooner or later we shall find ourselves face to face with one of the ultimate problems of all science—the nature of matter itself. In this problem all questions of chemical composition come to a focus; it goes back of the reaction to the substances which react; but it belongs equally to physics, and its essential details admit of description only in physical terms. Chemistry, however, is doing the most towards its solution, for it is through chemical researches that variations in the composition of matter are best explained. The indebtedness of chemistry to physics is thus fully repaid.

What is matter? Is it continuous or discrete, atomic or made up of vortex rings in the ether? These questions admit of only partial answers, and doubtless their final solution is unattainable by man. They are, nevertheless, perfectly legitimate questions for science to ask; and a tentative reply, of great practical value, is given by the atomic theory. Whether it be true or false, whether the chemical atoms are ultimate or divisible, this doctrine is the connecting thread upon which our profoundest generalizations are strung, and it is hard to see how we could do without it. Once a mere speculation of philosophy, Dalton gave it quantitative meaning; and from his day to the present every great advance in chemical theory has found its clearest statement in atomic terms. Chemical equations and formulæ; the laws which correlate the density of a gas with its composition; the law of Dulong and Petit; our ideas of valency and molecular structure; the periodic law; and the relations of stereochemistry, are all connected by the atomic theory, whose retention in science is therefore fully justified. It may not be beyond criticism; indeed, it should be criticized; but it would be the utmost folly to abandon the theory before something better has been framed to take its place. Vague and unsatisfactory are the attempts which have so far been made to supplant it. Physics, unaided by chemistry, may reach the conception of molecules; but the subdivision of the latter, the identification of their parts, is the function of the chemist alone.

If the nature of matter is the first element in the study of chemical reactions, the nature of chemical union is the second. If combination consists in a juxtaposition of atoms, what is the force which draws and holds them together? Whether we can answer this question or not, we may investigate the laws under which chemical action is operative, and so develop an important portion of physical chemistry. Problems of chemical equilibrium, of limiting conditions, of affinity and the speed of reactions, all come under this heading, and these are fit subjects for investigation in the laboratory. For instance, chemical action is impossible at very low temperatures, and at sufficiently high temperatures all compounds dissociate; each reaction, therefore, is confined to a certain part of the thermometric scale, which in many cases is measurable. In other words, chemical change is a function of temperature, no matter what additional factors its complete study may involve. It may also be effected through the agency of electrical or actinic impulses; and here again experimental research has a wide field. Were physical chemistry restricted, as it is not, to this class of investigations alone, it would still have abundant occupation. These illustrations are enough for my immediate purpose, but they could be multiplied indefinitely.

Directly growing out of these two fundamental questions, and partly identifiable with them, are two other problems of great generality and importance. First, what laws connect the properties of compounds with their composition? Secondly, what laws govern the transformations of energy during chemical change? Along each of these lines a large amount of work has been done, mostly empirical; and some regularities, some minor laws, are already recognized. Systematically, however, neither field is well known, and both offer rich prizes to the investigator. Great masses of more or less available data now exist; but rarely do we find any group adequately developed. The determination of constants or the measurement of thermochemical relations is tedious in the extreme; but a vast amount of such work needs to be done under some definite system or plan. At present we have a datum here and a datum there; some one in Germany makes a few measurements, some one in France, or England, or America makes a few more; but seldom is there any attempt at cooperation, and the isolated facts do not always fit together. The thermochemical data are especially difficult to determine accurately, and still more difficult to discuss in such a way as to develop any clearly defined law. Indeed, thermochemistry, of late years, has fallen out of favor; for to many chemists, despite its promise, it seems to lead nowhere. But laws must exist under all these troubling questions, and we cannot despair of their discovery. We can accomplish little, however, unless we consider each of the four great fundamental problems with reference to the others, for they are separable only in theory. Scientific research is not linear, step following step in regular succession; it is a network, rather, whose interlacing threads are woven into patterns of infinite variety. We trace individual fibers, we see, more or less clearly, a part of the design; and this is the most that any one of us can ever hope to do.

Now, whether we regard the fundamental questions of chemistry as four in number, or condense them into two, we can use our classification as an aid to research. Success in the latter means a wise selection of problems, a choice which is conditioned by our strength and our resources; but the first step is to understand the bearings of what we are trying to do. Whether our purposes are modest or ambitious, our work must have an influence upon that of others, and the broader the plan upon which it is conceived, the better the outcome will be. One bullet well aimed is worth more than a volley at random. One fact with a purpose outweighs a hundred scattering observations. We may well ask, therefore, what investigations are most needed by chemistry to-day?

First, as to the nature of matter, with all that that question implies. Taking all kinds of matter into consideration, and starting with the established distinction between elements and compounds, it would seem to be obvious that work is most imperatively needed where our information is least complete. Some elements, some classes of compounds, have been much more exhaustively studied than others; they, therefore, can best bear a temporary neglect, our attention, in the meanwhile, being concentrated elsewhere. I do not mean by this that any kind of research should cease, only that each department should assume something like reasonable proportions. To organic chemistry, for example, we are indebted for many methods of research, and for theoretical conceptions of great fertility; but it is now time to apply them to inorganic substances, and to see whether they are generally valid. Whatever result is reached, organic chemistry itself will be the gainer; enriched by new suggestions and resting upon firmer foundations, its future advancement can be made all the more certain. Meanwhile, carbon compounds, by virtue of their serial relations, are of peculiar value in certain lines of physico-chemical investigations; and they may also be profitably studied along the vague boundary which separates organic from inorganic chemistry. What we may call the contact phenomena between any two departments of knowledge are always interesting.

In the present revival of inorganic chemistry, a limited number of subjects have received the most attention. Among them I may name the study of double salts, of the rare earths and of complex acids and bases. All this work is of value; some of it is fundamental; but more urgent, probably, is a revision of the older data concerning much simpler bodies. This task is not attractive; it is far from brilliant in character and promises no startling discoveries; but it is none the less essential if we wish to establish the foundations of chemistry more securely. Consider any group of inorganic compounds, as, for example, the anhydrous metallic halides, and we soon find that our knowledge of them is full of gaps, and that the descriptions of many presumably well-known substances are wretchedly incomplete and defective. To remedy this condition of affairs is no small matter; there are errors to eliminate and careless work to be done over; but with modern resources a great improvement is possible. Now, thanks to physical chemistry, we can determine molecular weights, either by cryoscopic or ebullioscopic methods; and in the periodic law we have a basis for scientific classification. With these aids to research the new data should assume a theoretical value which formerly was lacking. For instance, the structural side of inorganic chemistry has been wofully defective; but now, knowing the molecular weights of substances, problems of structure may be attacked to advantage. The conception of valency can thus be tested to the uttermost degree.

Underlying all work upon compounds, however, is the study of the elements themselves. We may speculate as to their ultimate nature, or we may condemn speculation as useless; but we must agree that accurate knowledge of their relations and properties is most desirable, and especially so with respect to physico-chemical researches. In order to correlate the properties of compounds with those of their components, we must first determine the latter, and our present knowledge in this direction is exceedingly incomplete. Not one element is thoroughly known on the physical side, and some, indeed, have not as yet been definitely isolated. What we require is the exact measurement of all the physical properties of all the chemical elements at all available temperatures; from such data laws are sure to follow. Here again the periodic law can guide us; for in its curves the measured constants are easiest compared. In this scheme, evidently, the accurate determination of atomic weights is an important feature, for with them all else is coordinated. We also need to know, more completely than we do at present, the molecular weights of the free elements, because the reactions which we really observe are between molecules and not between atoms. Thus, when monatomic mercury unites with octatomic sulphur, the phenomena which occur involve the breaking down of the sulphur molecule. If, instead of mercury, we have diatomic oxygen or tetratomic arsenic, the reaction with sulphur becomes still more complex, for in each case, before combination, two molecules must be dissociated. The dissociation, of course, implies a loss of energy, of unknown amount; and in thermochemical discussions this undetermined factor is the chief obstacle to progress. If we could study reactions between monatomic molecules alone, we should have ideally the simplest conditions for thermochemical measurement. But such reactions might be difficult to identify, if indeed, they are possible at all. These considerations are obvious enough, but, unfortunately, they are sometimes overlooked.

Of the second great problem of chemistry, the nature of chemical combination, I need say little more. Some of the subordinate questions which grow out of it have been already mentioned, and each of them is a center of activity in the chemical research of the day. The entire field, however, is not covered, and here and there we can see evidences of neglect. First, we need to know under what conditions chemical change is possible. Then, if we would truly understand what chemical attraction means, we must study much more fully than hitherto its relations to other forces. How do heat, or light, or electricity inaugurate a reaction, and how are they produced by it? Questions of equilibrium are important, but they are subordinate to these. Furthermore, is chemical union of one kind only, or do we confuse different phenomena under the single name? Some authors write of atomic and molecular combinations as if they were distinct; are they really so, or is the separation nothing more than a confession of ignorance? For example, what is water of crystallization? Here is one of the commonest phenomena of chemistry entirely unexplained.

Up to this point I have considered the needs of chemistry from the theoretical side alone, as if we had only a matter of pure science to deal with. But the question has other aspects, of equal importance to us, and these now claim our attention. In order to enlarge the possibilities of research, what more do we need in the way of opportunities and resources?

To the sporadic, the piecemeal, the almost accidental character of scientific investigation I have already referred. Rarely do we find a man who can take up a large problem in a large way, with all its ramifications and details; even the most favored investigator must confine his personal work within narrow bounds, and do the best he can in his own corner. The greater part of chemical discovery has been the result of individual effort—the work of men who labored independently of one another, with rare cooperation, and often under conditions of the least favorable kind. By an army of volunteers, undisciplined and unofficered, the victories of science have been won. The time is now ripe for something better—how to organize research is the problem to be solved.

I do not mean to imply, by this suggestion, that any existing agency for research should be destroyed, or even supplanted; for such a proposition would be foolish in the extreme. Individual initiative, personal enthusiasm, are too precious to be lost; they have their part to play in the development of science; and the smallest fact, discovered by the humblest worker, will always be welcome. I do believe, however, that present conditions may be improved; that the efficiency of the individual can be increased; and to this end I urge upon your consideration the possibility of cooperation between those investigators who happen to be laboring in the same field. Ten men, pulling together, can do more than twenty who are apart. Duplication of effort, the useless repetition of work, can at least be avoided.

On several former occasions I have advocated, as the most urgent need of science, the regular endowment of research. By this I do not mean the payment of salaries to men working at random, who shall each choose his own small problem and attack it in his own way. Such a procedure would increase facilities, no doubt, but it might prove to be wasteful in the end. I look rather to the establishment of institutions, wherein bodies of trained men should take up, systematically and thoroughly, the problems which are too large for individuals to handle. Suppose that some of the wealth which chemistry has created should return to it in the form of a well-built, well-equipped, and well-endowed laboratory, devoted to research alone—what might we not expect from such a foundation! Libraries, museums, schools and universities receive endowments by the score; observatories are equipped for astronomical research; why should not chemistry come in for her share of the benefactions? Are our achievements so great that we seem to need no aid? In this hint there is a modicum of truth; the users of chemistry, the great industrial leaders, see the wonderful resources of our science, and do not realize that she can require more. That the giver of help should herself demand assistance is a hard thing to explain.

This, then, is our greatest need; the endowment of laboratories for systematic research, wherein chemistry and physics shall find joint provision. I say 'systematic research,' in order to distinguish it from the uncorrelated work of separate individuals. In physics, or for physics primarily, a beginning has already been made; the Reichsanstalt, at Berlin, the new physical laboratory in London, and the Bureau of Standards, at Washington, can cover a part of the ground. But it is only a part; for in each case, and in other like institutions, the researches are undertaken mainly in response to industrial demands; to furnish methods and standards rather than to develop principles and laws. The advancement of science as science is quite another affair. Neither does the Davy-Faraday Laboratory in London exactly meet our requirements. It is organized to help individuals, by giving facilities for work; but it does not provide for the systematic investigation of large problems, through the combined efforts of a body of chemists operating under a common plan. These institutions are all steps in the evolution of the research laboratory; but the development, as yet, is incomplete. Laboratories for instruction have been lavishly provided, but in them research is subordinate to teaching. The thesis of the student may represent good work; the leisure of the instructor may be fruitful also; but organized research is a different thing, and must have its own independent resources.

Either at public expense or by private enterprise, laboratories for research should be established in all of the larger civilized countries. By conference between them their work could be so adjusted as to avoid repetition, each one reinforcing the others. Their primary function should be to perform the drudgery of science; to undertake the tedious, laborious, elaborate investigations from which the solitary worker shrinks, but which are nevertheless essential to the healthy development of chemistry. Brilliant discoveries might be made in them, but incidentally, and not as their main purpose. Such discoveries would surely follow if the fundamental work was well done; but the latter should come first as being the most essential. Whether we serve pure science or applied science, we all feel the need of data which are as yet undetermined, and whose ascertainment we cannot undertake ourselves. How often are we baffled in our own researches for want of just such material! In the verification of methods and the determination of constants, the research laboratory would have plenty to do, even were nothing more attempted.

By the creation of laboratories such as I have suggested, the independent scholar might be aided in many ways. The antecedent data, without which his researches are crippled, could often be furnished, thus opening pathways where obstacles now exist. Furthermore, the desirable cooperation between investigators would become a much simpler matter to arrange than it is now. Every laboratory for research would become a nucleus around which individual enterprises might cluster, each giving and receiving help. A great work, wisely planned, always attracts colaborers; its mere suggestiveness is enough to provoke widespread intellectual activity. Here there is no monopoly, no limit to competition, no harmful rivalry; every research is the seed of other researches, and every advance made by one scholar implies the advance of all. In the realm of thought we gain by giving; and the more lavish our offerings, the richer we become.

We glory in the achievements of chemistry, and we find merit also in its imperfections, for they give us something more to do. Never can the work be finished, never can all its possibilities be known. Hitherto the science has grown slowly and irregularly, testing its strength from step to step, and securing a sure foothold in the world. Now comes the time for better things; for system, for organization, for transforming the art of investigation itself into something like a science. The endowment of research is near at hand, and the results of it will exceed our most sanguine anticipations.

F. W. CLARKE.
U. S. GEOLOGICAL SURVEY.

GRADED CONDENSATION IN BENZINE VAPOR, AS EVIDENCED BY THE DISTORTED CORONAS AND MARKED AXIAL COLOR EFFECTS ATTENDING CLOUDY CONDENSATION.

1. It would be difficult to read the admirable work on the relation of rain and atmospheric electricity which has issued from the Cavendish Laboratory, without being convinced of the strength of the arguments put forth. That in a repetition of these researches, in particular of the experiments of C. T. R. Wilson* on the comparative efficiency as condensation nuclei of positively and negatively charged ions, one would but reproduce his results admits of no doubt.

In so important a question, however, it is none the less desirable to reach identical conclusions from entirely different methods of approach. It has been part of my purpose to be driven to like inferences; in other words, to reach a point in my work where I should have to abandon the nucleus as an agency which for purely mechanical or thermodynamic reasons facilitates condensation, and be compelled to recognize the special activity due to its charge.

I had hoped to accomplish this in the following experiments with benzine when contrasted with the corresponding behavior of water; but the results, contrary to my expectation, are so curious and pronounced an accentuation of the nuclear theory that it seems worth while to specially describe them.

2. The work originated in the following point of view: if the action promoting condensation is in any degree of a chemical nature (such suppositions have been made; the production of hydrogen superoxide, for instance, has been suggested), then there should be a marked difference in the efficacy of the same nucleus when the saturated water vapor is replaced by the vapor of some electrolytically neutral liquid, like a hydrocarbon. I accordingly made a series of experiments with benzine, endeavoring at first to utilize benzine jet and color tube in the usual way. In this I failed for reasons without much relevant interest here. I then adopted the method of adiabatic cooling, partially exhausting a spherical receiver (Coulier, Kiessling) about 23 cm. in diameter, illuminated by

*C. T. R. Wilson, *Phil. Trans.*, London, Vol. CXCIII., pp. 289–308, 1899.