M. CHEVREUL.

If a chance promenade should lead you some Sunday to the garden of the Museum and into the quiet alleys that skirt the extremity of Cuvier Street, opposite the Seine, you may possibly see, in the second story of a house half hidden by verdure, a venerable man gazing upon the trees, the plants, the flowers, the heavens, and the playing of the children upon the sand. It is M. Chevreul; you will recognize him by his long black surtout and his very white hair, that falls in curls over a pair of shoulders that age has been unable to bend. He has, since the 31st day of last August, been in his ninety-ninth year, and none of the ordinary fatigues of old age seems to have overtaken him. He belongs to the race of oaks.

Here he lives, on Sunday only, at a couple of steps from the Museum where he has so long and so brilliantly taught. The rest of the week he goes, comes, works, and studies the composition of dust in his laboratory at the Gobelins, assists at the meetings of the Academy, presides over the discussions of the National Society of Agriculture, directs the labors of the Consulting Committee of Arts and Manufactures, comments upon Newton and Leibnitz, discusses the labors of his confreres, quarrels with his friend M. Faye *a propos* of his "pirouettes," and, in short, devotes his life and strength to science, which he loves so well.

Recently, when the Academy congratulated M. Chevreul upon his longevity, he said to his colleagues, "I owe it to the moderation of my tastes." This man, in fact, whom a single one of his discoveries might have enriched, and who had only to hold out his hand to gather millions, has been content to pursue his studies and publish the result of them. His professorship, his working cabinet, a few lectures in the library, his conversations with all the illustrious men of the age, these have occupied his life—no luxury, no show, no love of money, nothing but an ardent passion for work. He but rarely leaves his laboratory at the Gobelins, where he has been professor of chemistry and director of the dye houses since 1824. When he enters his apartment near the Museum, he sets himself at work amid the pamphlets and notices with which his room is filled. The room is a modest one, almost severe, with its furniture of the time of the Empire, its old mahogany closet, its little commode—those nothings amid which their owner has passed his busy life. As well known, M. Chevreul calls himself, with charming good nature, the dean of French students. This room of his might also pass for the dean of the rooms of French students. It contains furniture that he certainly owned at the time when, fresh from the Central School of Angers (in 1803), he was studying chemistry at Paris in the Vauquelin manufactory of chemical products. On the walls are a few portraits of friends, a Virgin painted by a relative, and silhouettes of laughing children, all of them objects marked with a souvenir.

We have said that the least of M. Chevreul's discoveries might have procured him a large fortune had he preferred money to science and a desire to be useful. There is one of these which has already put millions into the hands of those who have taken advantage of it, and that is the one that concerns the treatment of fatty bodies and their decomposition into acids and glycerine—a true gold mine. The stearine candle industry was born of this. The dyeing industry, likewise, has profited by those studies upon colors that permitted the illustrious savant to formulate the law of their simultaneous contrast.

M. Chevreul has one of those minds that everything interests, because it sees therein an occasion for verifying or formulating a law, according as the theory explaining this or that phenomenon has or has not been formed. Now, when he took up his labors on colors, the latter had already given rise to interesting observations on the part of the learned Jesuit, Father Scheffert, and these M. Chevreul knew. Father Scheffert had remarked that upon fixing the eyes upon a white surface, after steadily gazing at a colored object, red, for example, the surface assumed, in this case, a green color. Each color gave rise to another, which Father Scheffert called a "reversed" one. These simple observations gave M. Chevreul the idea of seeking all the combinations of colors with each other. The task suited him admirably, for it gave him an occasion to seek an explanation of one of those numerous phenomena of vision. Leibnitz had remarked that "our senses give us only unrealities," and this expression appears to have had a decisive influence upon M. Chevreul's studies. He was the more convinced of the truth of it when he remarked that one color juxtaposed upon another becomes perceptibly modified. In his cabinet he has an album composed of pages of various colors, and upon each of which there is a palm painted in gray. Now this latter appears rose-colored, greenish, orange, etc., according to the color of the page upon which it is painted. Again, red placed beside green appears redder, orange makes blue brighter, and violet is heightened by yellow. This heightening of a color is due to the juxtaposition of its "complementary," for that is the term that M. Chevreul uses as a substitute for Father Scheffert's "reversed." It is the same phenomenon that Leonardo da Vinci witnessed at daybreak, when the shadows of opaque objects appeared to him to be violet.

Besides his curious album, M. Chevreul has his "pirouettes"—a term that is not very scientific, but one that he adheres to notwithstanding the friendly grumbling of M. Faye. These pirouettes, or whirligigs, consist of a disk of cardboard, half white and half colored, traversed in the center by a wire. When this disk is made to revolve, it will, if the colored part is red, appear greenish while in motion; and so with all the other colors, their complementary will appear.

At the present moment, without any interruption in his ordinary work at the Gobelins, M. Chevreul is making a microscopic examination of the dust gathered from various objects with a feather duster. This work he has been at for six months—taking notes and preparing new publications. His memory has in no wise failed him, and if, in speaking of Buffon, for example, you are simply deceived in regard to the date of an edition of one of his works, he will give you at once the exact date of it.

The philosophical uncertainty in which M. Chevreul has always lived has estranged him from all systems. He has remained undisciplined, convinced that nature contains more things than the best philosophies can conceive of. If you speak to him of Saint Simon, the founder of Saint-Simonism, and in whose company he dined at M. De Blainville's, he will tell you that he remembers that the celebrated apostle wore horseman's boots, drawers, and a surtout. If you speak to him of Auguste Comte, he will say to you that the author of positivism was wrong in drawing from the experimental sciences conclusions as exact as from the science of pure reasoning. Press him, in order to get his own idea of optimism and pessimism, and he will give his opinion

that neither of these doctrines changes things in any respect; the optimist, who regards everything as beautiful, and the pessimist, who looks at everything as ugly, are not philosophers, but merely peculiar temperaments that see in such or such a manner. The truth is that there is a progress in the sciences, an improvement in social situations, and the old peasants of Anjou, whom M. Chevreul knew, led a harder life than they do to-day.

Finally, ask him what faith he embraces, whether he is a spiritualist, deist, or atheist? He will answer you that no one better than Voltaire has demonstrated the existence of a sole, intelligent power; but that, on another hand, Moliere, who knew many things, painted a very ludicrous personage, the philosopher Marphurios, a man who did not know whether he ought to say the figure or the form of a bat, and who was sure neither of what he saw nor heard until Sganarella beat him with a stick in order to recall him to reality.

And if these answers do not satisfy you, and you insist on knowing the depths of his thought, you will see the great old man put his hands to his forehead as if to disentangle the truth from his compact bundle of knowledge, and then put them out to you smilingly and repeat Montaigne's *mot*, "What know I?"—*L'Illustration*.

The following biographical sketch of this remarkable scientist we find in Appleton's Encyclopædia:

"Chevreul, Michael Eugene, a French chemist, born at Angers, August 31, 1786. Having completed his studies in the Central School of Angers, he studied chemistry under Vauquelin in Paris, and afterward took charge of the laboratory of that chemist. In 1810 he became preparator of the chemical course in the Museum of Natural History, and 1813 professor in the Lycee Charlemagne. In 1824 he was appointed director of the dye works and professor of special chemistry at the Gobelins, where he distinguished himself by his researches on colors. In 1826 he became a member of the Academy of Sciences, and in 1830 succeeded Vauquelin as professor of applied chemistry in the Museum of Natural History. He was afterward made fellow of the Royal Society of London, and president of the Society of Agriculture. In 1851 he was awarded a premium at the London Industrial Exhibition for the benefit his labors had conferred on industry, and 1852 he received from the Society for the Encouragement of Useful Inventions the prize of 12,000 francs for his work, 'Recherches chimiques sur les corps gras d'origine animale,' which had been published in 1823, and which is yet of high value, its suggestions having given rise to the manufacture of stearine candles and to the use of oleic acid in the preparation of wool for cloth. Another work of his, on sanitary influences, introduced the practice of charring the interior of water casks. He was a member of the international jury at the universal exhibition of 1851 in London and of 1855 in Paris. He was appointed director of the Museum of Natural History for five years in 1864 and again in 1869. Besides many articles on scientific subjects in the 'Dictionnaire des sciences naturelles,' the 'Journal des Savants,' and other periodicals, he published numerous works, among which are: 'Leçons de chimie appliquée à la teinture' (1828-31); 'De la loi du contraste simultané des couleurs et de l'assortiment des objets colorés' (1839); 'Des couleurs et de leurs applications aux arts industriels à l'aide des cercles chromatiques' (1864); 'Considérations sur l'histoire de la partie de la médecine qui concerne la prescription des remèdes' (1865); and 'Histoire des connaissances chimiques,' the first volume of which appeared in 1866. Many of these works have been translated into various European languages. He has also contributed many important papers to scientific societies, and in 1873, at the age of 86, was still lecturing on chemistry."

THE GENERAL PROGRESS OF CHEMISTRY.*

By Sir H. E. Roscoe.

WITH the death of Berzelius in 1848 ended a well-marked epoch in the history of our science; with that of Dumas—and, alas! that of Wurtz also—in 1884 closes a second. It may not perhaps be unprofitable on the present occasion to glance at some few points in the general progress which chemistry has made during this period, and thus to contrast the position of the science in the "sturm und drang" year of 1848 with that in the present, perhaps quieter, period.

The differences between what may probably be termed the Berzelian era and that with which the name of Dumas will forever be associated show themselves in many ways, but in none more markedly than by the distinct views entertained as to the nature of a chemical compound.

According to the older notions, the properties of compounds are essentially governed by a qualitative nature of their constituent atoms, which were supposed to be so arranged as to form a binary system. Under the new ideas, on the other hand, it is mainly the number and arrangement of the atoms within the molecule which regulate the characteristics of the compound, which is to be looked on not as built up of two constituent groups of atoms, but as forming one group.

Among those who successfully worked to secure this important change of view on a fundamental question of chemical theory, the name of Dumas himself must first be mentioned, and, following upon him, the great chemical twin-brethren Laurent and Gerhardt, who, using both the arguments of test-tube and of pen in opposition to the prevailing views, gradually succeeded, though scarcely during the lifetime of the first, in convincing chemists that the condition of things could hardly be a healthy one when chemistry was truly defined "as the science of bodies which do not exist." For Berzelius, adhering to his preconceived notions, had been forced by the pressure of new discovery into the adoption of formulæ which gradually became more and more complicated, and led to more and more doubtful hypotheses, until his followers at last could barely succeed in building up the original radical from its numerous supposed component parts. Such a state of things naturally brought about its own cure, and the unitary formulæ of Gerhardt began to be generally adopted.

It was not, however, merely as an expression of the nature of the single chemical compound that this change was beneficial, but, more particularly, because it laid open the general analogies of similarly constituted compounds, and placed fact as the touch-stone by which the constitution of these allied bodies should be ascertained. Indeed, Gerhardt, in 1852, gave evidence of the truth of this in his well-known theory of type, according to which organic compounds of ascertained constitution can be arranged under the four types of hydrogen, hydrochloric acid, water, and ammonia, and of which it is, perhaps, not too much to say

that it has, more than any other of its time, contributed to the clearer understanding of the relations existing among chemical compounds.

Another striking difference of view between the chemistry of the Berzelian era and that of what we sometimes term the modern epoch is illustrated by the so-called substitution theory. Dumas, to whom we owe this theory, showed that chlorine can take the place of hydrogen in many compounds, and that the resulting body possesses characters similar to the original. Berzelius opposed this view, insisting that the essential differences between these two elements rendered the idea of a substitution impossible, and notwithstanding the powerful advocacy of Liebig, and the discovery by Melsens of reverse substitutions (that is, the reformation of the original compound from its substitution product), Berzelius remained to the end unconvinced; and that which was in reality a confirmation of his own theory of compound radicals, which, as Liebig says, "illuminated many a dark chapter in organic chemistry," was looked upon by him as an error of the deepest dye. This inability of many minds to see in the discoveries of others confirmation of their own views is not uncommon; thus Dalton, we may remember, could never bring himself to admit the truth of Gay-Lussac's laws of gaseous volume-combination, although, as Berzelius very truly says, if we write *atom* for *volume*, and consider the substance in the solid state in place of the state of gas, the discovery of Gay-Lussac is seen to be one of the most powerful arguments in favor of Dalton's hypothesis.

But there is another change of view, dating from the commencement of the Dumas epoch, which has exerted an influence equal, if not superior, to those already named on the progress of our science. The relative weights of the ultimate particles, to use Dalton's own words, which up to this time had been generally adopted by chemists, were the equivalent weights of Dalton and Wollaston, representing, in the case of oxygen and hydrogen, the proportions in which these elements combine, viz., as 8 to 1. The great Swedish chemist at this time stood almost alone in supporting another hypothesis; for, founding his argument on the simple laws of volume-combination enunciated by Gay-Lussac, he asserted that the true atomic weights are to be represented by the relations existing between equal volumes of the two gases, viz., as 16 to 1. Still these views found no favor in the eyes of chemists until Gerhardt, in 1843, proposed to double the equivalent weights of oxygen, sulphur, and carbon, and then the opposition which this suggestion met with was most intense, Berzelius himself not even deigning to mention it in his annual account of the progress of the science, thus proving the truth of his own words: "That to hold an opinion habitually often leads to such an absolute conviction of its truth that its weak points are unregarded, and all proofs against it ignored." Nor were these views generally adopted by chemists until Cannizzaro, in 1858, placed the whole subject on its present firm basis by clearly distinguishing between equivalent and molecular weights, showing how the atomic weights of the constituent elements are derived from the molecular weights of their volatile compounds based upon the law of Avogadro and Ampere, or where, as is the case with many metals, no compounds of known vapor density exist, how the same result may be ascertained by the help of the specific heat of the element itself. Remarkable as it may appear, it is nevertheless true that it is in the country of their birth that Gerhardt's atomic weights and the consequent atomic nomenclature have met with most opposition, so much so that within a year or two of the present time there was not a single course of lectures delivered in Paris in which these were used.

The theory of organic radicals, developed by Liebig so long ago as 1834, received numerous experimental confirmations in succeeding years. Bunsen's classical research on cacodyl, proving the possibility of the existence of metallo-organic radicals capable of playing the part of a metal, and the isolation of the hydrocarbon ethyl by Frankland in 1849, laid what the supporters of the theory deemed the final stone in the structure.

The fusion of the radical and type theories, chiefly effected by the discovery of 1849 of the compound ammonias by Wurtz, brings us to the dawn of modern chemistry. Henceforward organic compounds were seen to be capable of comparison with simple inorganic bodies, and hydrogen not only capable of replacement by chlorine or by a metal, but by an organic group or radical.

To this period my memory takes me back. Liebig at Giessen, Wohler in Göttingen, Bunsen in Marburg, Dumas, Wurtz, and Laurent and Gerhardt in Paris, were the active spirits in Continental chemistry. In our own country, Graham, whose memorable researches on the phosphates had enabled Liebig to found his theory of polybasic acids, was working and lecturing at University College, London; and Williamson, imbued with the new doctrines and views of the twin French chemists, had just been appointed to the Chair of Practical Chemistry in the same college, vacant by the death of poor Fownes. At the same time, Hofmann, in whom Liebig found a spirit as enthusiastic in the cause of scientific progress as his own, bringing to England a good share of the Giessen fire, founded the most successful school of chemistry which this country has yet seen.

At the Edinburgh meeting of this Association in 1850, Williamson read a paper on "Results of a Research on Ætherification," which included not only a satisfactory solution of an interesting and hitherto unexplained problem, but was destined to exert a most important influence on the development of our theoretical views. For he proved, contrary to the then prevailing ideas, that ether contains twice as much carbon as alcohol, and that it is not formed from the latter by a mere separation of the elements of water, but by an exchange of hydrogen for ethyl, and this fact being in accordance with Avogadro's law of molecular volumes, could only be represented by regarding the molecule of water as containing two atoms of hydrogen to one of oxygen, one of the former being replaced by one of ethyl to form alcohol, and the two of hydrogen by two of ethyl to form ether. Then Williamson introduced the type of water (subsequently adopted by Gerhardt) into organic chemistry, and extended our views with the analogies between alcohols and acids, by pointing out that these latter are also referable to the water-type, predicting that bodies bearing the same relations to the ordinary acids as the ethers do to the alcohols must exist, a prediction shortly afterward (1852) verified by Gerhardt's discovery of the anhydrides. Other results followed in rapid succession, all tending to knit together the framework of modern theoretical chemistry. Of these the most important was the adoption of condensed types of compounds constructed on the type of two and three molecules of water, with which the names of Williamson and Odling are connected, culminating in the researches of Brodie on the higher alcohols, of Berthelot on

* Opening address by Prof. Sir Henry Enfield Roscoe, Ph.D., LL.D., F.R.S., F.C.S., President of Section B, Chemical Science, of British Association, Montreal.

glycerine, and of Wurtz on the dibasic alcohols or glycols; while, in another direction, the researches of Hofmann on the compound amines and amides opened out an entirely new field, showing that either a part or the whole of the hydrogen in ammonia can be replaced by other elements or elementary groups without the type losing its characteristic properties.

Again, in 1852, we note the first germs of a theory which was destined to play an all-important part in the progress of the science, viz., the doctrine of valency or atomicity, and to Frankland it is that we owe this new departure. Singularly enough, while considering the symmetry of construction visible among the inorganic compounds of nitrogen, phosphorus, arsenic, and antimony, and while putting forward the fact that the combining power of the attracting element is always satisfied by the same number of atoms, he does not point out the characteristic tetrad nature of carbon; and it was not until 1858 that Couper initiated, and Kekulé, in the same year, thoroughly established, the doctrine of the linking of the tetrad carbon atoms, a doctrine to which, more than to any other, is due the extraordinary progress which organic chemistry has made during the last twenty years—a progress so vast that it is already found impossible for one individual, even though he devote his whole time and energies to the task, to master all the details, or make himself at home with the increasing mass of new facts which the busy workers in this field are daily bringing forth.

The subject of the valency of the elements is one which, since the year above referred to, has given chemists much food for discussion, as well as opportunity for experimental work. But whether we range ourselves with Kekulé, who supports the unalterable character of the valency of each element, or with Frankland, who insists on its variability, it is now clear to most chemists that the hard and fast lines upon which this theory was supposed to stand cannot be held to be secure. For if the progress of investigation has shown that it is impossible in many instances to affix one valency to an element which forms a large number of different compounds, it is also equally impossible to look on the opposite view as tending toward progress, inasmuch as to ascribe to an element as many valencies as it possesses compounds with some other element is only expressing by circuitous methods what the old Daltonian law of combination in multiple proportion states in simple terms. Still we may note certain generally accepted conclusions: in the first place, that of the existence of non-saturated compounds, both inorganic and organic, as carbon-monoxide on the one hand, and malic and citraconic acids on the other. Secondly, that the valency of an element is not only dependent upon the nature of the element with which it combines, but that this valency is a periodic function of the atomic weight of the other component. Thus the elements of the chlorine group are always monads when combined with positive elements or radicals, but triad, pentad, and heptad with negative ones. Again, the elements of the sulphur group are dyads in the first case, but tetrad and hexad in the second. The periodicity of this property of the atoms, increasing and again diminishing, is clearly seen in such a series as

as well as in the series of oxides. The difficulties which beset this subject may be judged of by the mention of a case or two: Is vanadium a tetrad because its highest chloride contains four atoms of chlorine? What are we to say is the valency of lead when one atom unites with four methyls to form a volatile product, and yet the vapor-density of the chloride shows that the molecule contains one of metal to two of chlorine? Or, how can our method be said to determine the valency of tungsten when the hexachloride decomposes in the state of vapor, and the pentachloride is the highest volatile stable compound? How again are we to define the point at which a body is volatile without decomposition? Thus sulphur tetrachloride, one of the most unstable of compounds, can be vaporized without decomposition at all temperatures below -22° , while water, one of the most stable of known compounds, is dissociated into its elements at the temperature of melting platinum.

But, however many doubts may have been raised in special instances against a thorough application of the law of valency, it cannot be denied that the general relations of the elements which this question of valency has been the means of bringing to light are of the highest importance, and point to the existence of laws of nature of the widest significance; I allude to the periodic law of the elements first foreshadowed by Newlands, but fully developed by Mendeleff and Lothar Meyer. Guided by the principle that the chemical properties of the elements are a periodic function of their atomic weights, or that matter becomes endowed with analogous properties when the atomic weight of an element is increased by the same or nearly the same number, we find ourselves for the first time in possession of a key which enables us to arrange the hitherto *disjecta membra* of our chemical household in something like order, and thus gives us means of indicating the family resemblances by which these elements are characterized.

And here we may congratulate ourselves on the fact, that by the recent experiments of Brauner, and of Nilson and Pettersen respectively, tellurium and beryllium, two of the hitherto outstanding members, have been induced to join the ranks, so that at the present time osmium is the only important defaulter among the sixty-four elements, and few persons will doubt that a little careful attention to this case will remove the stigma which yet attaches to its name. But this periodic law makes it possible for us to do more; for as the astronomer, by the perturbations of known planets, can predict the existence of hitherto unknown ones, so the chemist, though, of course, with much less satisfactory means, has been able to predict with precision the properties, physical and chemical, of certain missing links among the elements, such as ekaluminium and ekaboron, then unborn, but which shortly afterward became well known to us in the flesh as gallium and scandium. We must, however, take care that success in a few cases does not blind us to the fact that the law of nature which expresses the relation between the properties of the elements and the value of the atomic weights is as yet unknown; that many of the groupings are not due to any well ascertained analogy of properties of the elements, and that it is only because the values of their atomic weights exhibit certain regularities that such a grouping is rendered possible. So, to quote Lothar Meyer, we shall do well in this, as indeed in all similar cases in science, to remember the danger pointed out in Bacon's aphorism, that "The mind delights in springing up to the most general axioms, that it may find rest, but after a short stay here it disdains experience," and to bear in mind that it is only the lawful union of hypothesis with experiment which will prove a fruitful one in the establishment of a systematic inorganic chemistry which need not fear comparison with the

order which reigns in the organic branch of our science. And here it is well to be reminded that complexity of constitution is not the sole prerogative of the carbon compounds, and that before this systematization of inorganic chemistry can be effected we shall have to come to terms with many compounds concerning whose constitution we are at present wholly in ignorance. As instances of such I would refer to the finely crystalline phospho-molybdates, containing several hundred atoms in the molecule, lately prepared by Wolcott Gibbs.

Arising out of Kekulé's theory of the tetrad nature of the carbon atom, came the questions which have caused much debate among chemists: (1) are the four combining units of the carbon atom of equal value or not? And (2) is the assumption of a dyad carbon atom in the so-called non-saturated compounds justifiable or not? The answer to the first of these, a favorite view of Kolbe's, is given in the now well ascertained laws of isomerism; and from the year 1862, when Schorlemmer proved the identity of the hydrides of the alcohol radicals with the so-called radicals themselves, this question may be said to have been set at rest; for Lossen himself admits that the existence of his singular isomeric hydroxylamine derivatives can be explained otherwise than by the assumption of a difference between each of the combining units of nitrogen, and the differences supposed by Schreiner to hold good between the methyl-ethyl carbonic ethers have been shown to have no existence in fact. With respect to the second point the reply is no less definite, and is recorded in the fact, among others, that ethylene chlorhydrin yields on oxidation chloracetic acid, a reaction which cannot be explained on the hypothesis of the existence in ethylene of a dyad carbon atom.

Passing from this subject, we arrive, by a process of natural selection, at more complicated cases of chemical orientation—that is, given certain compounds which possess the same composition and molecular formulæ, but varying properties, to find the difference in molecular structure by which such variation of properties is determined. Problems of this nature can now be satisfactorily solved, the number of possible isomers foretold, and this prediction confirmed by experiment. The general method adopted in such an experimental inquiry into the molecular arrangement or chemical constitution of a given compound is either to build up the structure from less complicated ones of known constitution, or to resolve it into such component parts. Thus, for example, if we wish to discriminate between several isomeric alcohols, distinguishing the ordinary or primary class from the secondary or tertiary class, the existence of which was predicted by Kolbe in 1862, and of which the first member was prepared by Friedel in 1864, we have to study their products of oxidation. If one yields an acid having the same number of carbon atoms as the alcohol, it belongs to the first class, and possesses a definite molecular structure; if it splits up into two distinct carbon compounds, it is a secondary alcohol; and if three carbon compounds result from its oxidation, it must be classed in the third category, and to it belongs a definite molecular structure, different from that of the other two.

In a similar way orientation in the much more complicated aromatic hydrocarbons can be effected. This class of bodies forms the nucleus of an enormous number of carbon compounds, which, both from a theoretical and a practical point of view, are of the highest interest. For these bodies exhibit characters and possess a constitution totally different from those of the so-called fatty substances, the carbon atoms being linked together more intimately than is the case in the latter named group of bodies. Among them are found all the artificial coloring matters, and some of the most valuable pharmaceutical and therapeutical agents.

The discovery of the aniline colors by Perkin, their elaboration by Hofmann, the synthesis of alizarin by Graebe and Liebermann, being the first vegetable coloring matter which has been artificially obtained, the artificial production of indigo by Baeyer, and lastly the preparation, by Fischer, of kairin, a febrifuge as potent as quinine, are some of the well known recent triumphs of modern synthetic chemistry. And these triumphs, let us remember, have not been obtained by any such "random haphazarding" as yielded results in Priestley's time. In the virgin soil of a century ago, the ground only required to be scratched and the seed thrown in to yield a fruitful crop; now the surface soil has long been exhausted, and the successful cultivator can only obtain results by a deep and a thorough preparation, and by a systematic and scientific treatment of his material.

In no department of our science has the progress made been more important than in that concerned with the accurate determination of the numerical, physical, and chemical constants, upon the exactitude of which every quantitative chemical operation depends. For the foundation of an accurate knowledge of the first of these constants, viz., the atomic weights of the elements, science is indebted to the indefatigable labors of Berzelius. But "humanum est errare," and even Berzelius' accurate hand and delicate conscientiousness did not preserve him from mistakes, since corrected by other workers. In such determinations it is difficult, if not impossible, always to ascertain the limits of error attaching to the number. The errors may be due in the first place to manipulative faults, in the second to inaccuracy of the methods, or lastly to mistaken views as to the composition of the material operated upon; and hence the uniformity of any series of similar determinations gives no guarantee of their truth, the only safe guide being the agreement of determinations made by altogether different methods. The work commenced by Berzelius has been worthily continued by many chemists. Stas and Marignac, bringing work of an almost astronomical accuracy into our science, have ascertained the atomic weights of silver and iodine to within one hundred-thousandth of their value, while the numbers for chlorine, bromine, potassium, sodium, nitrogen, sulphur, and oxygen may now be considered to be correct to within a unit in the fourth figure. Few of the elements, however, boast numbers approaching this degree of accuracy, and many may even still be erroneous from half to a whole unit of hydrogen. And, as Lothar Meyer says, until the greater number of the atomic weights are determined to within one or two tenths of the unit, we cannot expect to be able to ascertain the laws which certainly govern these numbers, or to recognize the relations which undoubtedly exist between them and the general chemical and physical properties of the elements. Among the most interesting recent additions to our knowledge made in this department we may note the classical experiments, in 1880, of J. W. Mallet on aluminum, and in the same year of J. P. Cooke on antimony, and those, in the present year, of Thorpe on titanium.

Since the date of Berzelius' death to the present day, no discovery in our science has been so far-reaching, or led to such unforeseen and remarkable conclusions, as the foundation of spectrum analysis by Bunsen and Kirchhoff in 1860.

Independently altogether of the knowledge which has been gained concerning the distribution of the elementary bodies in terrestrial matter, and of the discovery of half a dozen new elements by its means, and putting aside for a moment the revelation of a chemistry not bounded by this world, but limitless as the heavens, we find that over and above all these results spectrum analysis offers the means, not otherwise open to us, of obtaining knowledge containing the atomic and molecular condition of matter.

Let me recall some of the more remarkable conclusions to which the researches of Lockyer, Schuster, Liveing and Dewar, Wullner, and others in this direction have led. In the first place it is well to bear in mind that a difference of a very marked kind, first distinctly pointed out by Alex. Mitscherlich, is to be observed between the spectrum of an element and that of its compounds, the latter only being seen in cases in which the compound is not dissociated at temperatures necessary to give rise to a glowing gas. Secondly, that these compound spectra—as, for instance, those of the halogen compounds of the alkaline earth metals—exhibit a certain family likeness, and show signs of systematic variation in the position of the lines, corresponding to changes in the molecular weight of the vibrating system. Still this important subject of the relation of the spectra of different elements is far from being placed on a satisfactory basis; and in spite of the researches of Lecoq de Boisbaudran, Ditte, Troost and Hautefeuille, Ciamician, and others, it cannot be said that as yet definite proof has been given in support of the theory that a casual connection is to be found between the emission spectra of the several elements belonging to allied groups and their atomic weights or other chemical or physical properties. In certain of the single elements, however, the connection between the spectra and the molecular constitution can be traced. In the case of sulphur, for example, three distinct spectra are known. The first of these, a continuous one, is exhibited at temperatures below 500° , when, as we know from Dumas' experiments, the density of the vapor is three times the normal, showing that at this temperature the molecule consists of six atoms. The second spectrum is seen when the temperature is raised to above $1,000^\circ$, when, as Deville and Troost have shown, the vapor reaches its normal density, and the molecule of sulphur, as with most other gases, contains two atoms, and this is a band spectrum, or one characterized by channeled spaces. Together with this band spectrum, and especially around the negative pole, a spectrum of bright lines is observed. This latter is doubtless due to the vibrations of the single atoms of the dissociated molecule, the existence of traces of a band spectrum demonstrating the fact that in some parts of the discharge the tension of dissociation is insufficient to prevent the reunion of the atoms to form the molecule.

To this instance of the light thrown on molecular relations by changes in the spectra, others may be added. Thus the low temperature spectrum of channeled spaces, mapped by Schuster and myself, in the case of potassium, corresponds to the molecule of two atoms and to the vapor density of 79 observed by Dewar and Dittmar. Again, both oxygen and nitrogen exhibit two, if not three, distinct spectra; of these the line spectrum seen at the highest temperatures corresponds to the atom; the band spectrum seen at intermediate temperatures represents the molecule of two atoms; while that observed at a still lower point would, as in the case of sulphur, indicate the existence of a more complicated molecule, known to us in one instance as ozone.

That this explanation of the cause of these different spectra of an element is the true one, can be verified in a remarkable way. Contrary to the general rule among these elements which can readily be volatilized, and with which, therefore, low temperature spectra can be studied, mercury exhibits but one spectrum, and that one of bright lines, or, according to the preceding theory, a spectrum of atoms. So that, judging from spectroscopic evidence, we infer that the atoms of mercury do not unite to form a molecule, and we should predict that the vapor density of mercury is only half its atomic weight. Such we know, from chemical evidence, is really the case, the molecule of mercury being identical in weight with its atom.

The cases of cadmium and iodine require further elucidation. The molecule of gaseous cadmium, like that of mercury, consists of one atom; probably, therefore, the cadmium spectrum is also distinguished by one set of lines. Again, the molecule of iodine at $1,200^\circ$ separates, as we know from Victor Meyer's researches, into single atoms. Here spectrum analysis may come again to our aid; but, as Schuster remarks, in his report on the spectra of the non-metallic elements, a more extensive series of experiments than those already made by Ciamician is required before any definite opinion as to the connection of the different iodine spectra with the molecular condition of the gas can be expressed.

It is not to be wondered at that these relations are only exhibited in the case of a few elements. For most of the metals the vapor density remains, and probably will remain, an unknown quantity, and therefore the connection between any observed changes in the spectra and the molecular weights must also remain unknown. The remarkable changes which the emission spectrum of a single element—iron, for instance—exhibits have been the subject of much discussion, experimental and otherwise. Of these, the phenomenon of long and short lines is one of the most striking, and the explanation that the long lines are those of low temperature appears to meet the fact satisfactorily, although the effect of dilution, that is, a reduction of the quantity of material undergoing volatilization, is, remarkably enough, the same as that of diminution of temperature. Thus it is possible, by the examination of a spectrum by Lockyer's method, to predict the changes which it will undergo, either on alteration of temperature or by an increase or decrease of quantity. There appears to be no theoretical difficulty in assuming that the relative intensity of the lines may vary when the temperature is altered, and the molecular theory of gases furnishes us with a plausible explanation of the corresponding change when the relative quantities of the luminous elements in a mixture are altered.

Lockyer has proposed a different explanation of the facts. According to him, every change of relative intensity means a corresponding change of molecular complexity, and the lines which we see strong near the poles would bear the same relation to those which are visible throughout the field as a line spectrum bears to a band spectrum; but then almost every line must be due to a different molecular grouping, a conclusion which is scarcely capable of being upheld without very cogent proof.

The examination of the absorption spectra of salts, saline and organic liquids, first by Gladstone, and afterward by Bunsen and by Russell, as well as by Hartley for the ultra-violet, and by Abney and Festing for the infra-red region, have led to interesting results in relation to molecular chem-

istry. Thus Hartley finds that, in some of the more complicated aromatic compounds, definite absorption bands in the more refrangible region are only produced by substances in which three pairs of carbon atoms are doubly linked, as in the benzene ring, and thus the means of ascertaining this double linkage is given. The most remarkable results obtained by Abney and Festing show that the radical of an organic body is always represented by certain well-marked absorption bands, differing, however, in position, according as it is linked with hydrogen, a halogen, or with carbon, oxygen, or nitrogen. Indeed, these experimenters go so far as to say that it is highly probable that by this delicate mode of analysis the hypothetical position of any hydrogen which is replaced may be identified, thus pointing out a method of physical orientation of which, if confirmed by other observers, chemists will not be slow to avail themselves. The result, it is interesting to learn, has been rendered more than probable by the recent important researches of Perkin on the connection between the constitution and the optical properties of chemical compounds.

One of the noteworthy features of chemical progress is the interest taken by physicists in fundamental questions of our science. We all remember, in the first place, Sir William Thomson's interesting speculations, founded upon physical phenomena, respecting the probable size of the atom, viz., "that if a drop of water were magnified to the size of the earth, the constituent atoms would be larger than small shot, but smaller than cricket balls." Again, Helmholtz, in the Faraday lecture, delivered in 1881, discusses the relation of electricity and chemical energy, and points out that Faraday's law of electrolysis, and the modern theory of valency, are both expressions of the fact that, when the same quantity of electricity passes through an electrolyte, it always either sets free, or transfers to other combinations, the same number of units of affinity at both electrodes. Helmholtz further argues that, if we accept the Daltonian atomic hypothesis, we cannot avoid the conclusion that electricity, both positive and negative, is divided into elementary portions which behave like atoms of electricity. He also shows that these charges of atomic electricity are enormously large as compared, for example, with the attraction of gravitation between the same atoms; in the case of oxygen and hydrogen, 71,000 billion times larger.

A further subject of interest to chemists is the theory of the vortex-ring constitution of matter thrown out by Sir William Thomson, and lately worked out from a chemical point of view by J. J. Thomson, of Cambridge. He finds that if one such ring be supposed to constitute the most simple form of matter, say the monad hydrogen atom, then two such rings must, on coming into contact with nearly the same velocity, remain enchaind together, constituting what we know as the molecule of free hydrogen. So, in like manner, systems containing two, three, and four such rings constitute the dyad, tryad, and tetrad atoms. How far this mathematical expression of chemical theory may prove consistent with fact remains to be seen.

Another branch of our science which has recently attracted much experimental attention is that of thermo-chemistry, a subject upon which in the future the foundation of dynamical chemistry must rest, and one which already proclaims the truth of the great principle of the conservation of energy in all cases of chemical as well as of physical change. But here, although the materials hitherto collected are of very considerable amount and value, the time has not yet arrived for expressing these results in general terms, and we must, therefore, be content to note progress in special lines and wait for the expansion into wider areas. Reference may, however, be properly made to one interesting observation of general significance. It is well known that, while, in most instances, the act of combination is accompanied by evolution of heat—that is, while the potential energy of most combining bodies is greater than that of most compounds—cases occur in which the reverse of this is true, and heat is absorbed in combination. In such cases the compound readily undergoes decomposition, frequently suddenly and with explosion. Acetylene and cyanogen seem to be exceptions to this rule, inasmuch as, while their component elements require to have energy added to them in order to enable them to combine, the compounds appear to be very stable bodies. Berthelot has explained this enigma by showing that, just as we may ignite a mass of dynamite without danger, while explosion takes place if we agitate the molecules by a detonator, so acetylene and cyanogen burn, as we know, quietly when ignited, but when their molecules are shaken by the detonation of even a minute quantity of fulminate, the constituents flew apart with explosive violence, carbon and hydrogen, or carbon and nitrogen, being set free, and the quantity of heat absorbed in the act of combination being suddenly liberated.

In conclusion, while far from proposing even to mention all the important steps by which our science has advanced since the year 1848, I cannot refrain from referring to two more. In the first place, to that discovery, more than foreshadowed by Faraday, of the liquefaction of the so-called permanent gases by Pictet and Cailletet; and secondly, to that of the laws of dissociation as investigated by Deville. The former, including Andrews' discovery of the critical point, indicates a connection, long unseen, between the liquid and the gaseous states of matter; the latter has opened out entirely fresh fields for research, and has given us new views concerning the stability of chemical compounds of great importance and interest.

Turning for a moment to another topic, we feel that, although science knows no nationalities, it is impossible for those who, like ourselves, exhibit strong national traits, to avoid asking whether we Anglo-Saxons hold our own, as compared with other nations, in the part we have played and are playing in the development of our science. With regard to the past, the names of Boyle, Cavendish, Priestley, Dalton, Black, Davy, are sufficient guarantees that the English have, to say the least, occupied a position second to none in the early annals of chemistry. How has it been in the era that I have attempted to describe? What is the present position of English chemistry, and what its lookout for the future? In endeavoring to make this estimate, I would take the widest ground, including not only the efforts made to extend the boundaries of our science by new discovery, both in the theoretical and applied branches, but also those which have the no less important aims of spreading the knowledge of the subject among the people, and of establishing industries dependent on chemical principles, by which the human race is benefited. Taking this wide view, I think we may, without hesitation, affirm that the progress which chemistry has made through the energies of the Anglo-Saxon race is not less than that made by any other nation.

In so far as pure science is concerned, I have already given evidence of the not inconsiderable part which English chemists have played in the progress since 1848. We must,

however, acknowledge that the number of original chemical papers now published in our language is much smaller than that appearing in the German tongue, and that the activity and devotion displayed in this direction by the heads of German laboratories may well be laid to heart by some of us in England; yet, on the other hand, it must be remembered that the circumstances of different countries are so different that it is by no means clear that we should follow the same lines. Indeed, our national characteristics forbid us to do so, and it may be that the bent of the Germanic lies in the assiduous collection of facts, while their subsequent elaboration and connection is the natural work of our own race.

As regards the publication of so-called original work by students, and speaking now only for myself as the director of an English chemical laboratory, I feel I am doing the best for the young men who, wishing to become either scientific or industrial chemists, are placed under my charge, in giving them as sound and extensive a foundation in the theory and practice of chemical science as their time and abilities will allow, rather than forcing them prematurely into the preparation of a new series of homologous compounds or the investigation of some special reaction, or of some possible new coloring matter, though such work might doubtless lead to publication. My aim has been to prepare a young man, by a careful and fairly complete general training, to fill with intelligence and success a post either as teacher or industrial chemist, rather than to turn out mere specialists, who, placed under other conditions than those to which they have been accustomed, are unable to get out of the narrow groove in which they have been trained. And this seems a reasonable course, for while the market for the pure specialist, as the color chemist for example, may easily be overstocked, the man of all-round intelligence will always find opportunity for the exercise of his powers. Far, however, from underrating the educational advantages of working at original subjects, I consider this sort of training to be of the highest and best kind, but only useful when founded upon a sound and general basis.

The difficulty which the English teacher of chemistry—and in this I may include Canada and the United States—has to contend against is that, while in Germany the value of this high and thorough training is generally admitted, in England a belief in its efficacy is as yet not generally entertained. "The Englishman," to quote from the recent Report of the Royal Commission on Technical Instruction, "is accustomed to seek for an immediate return, and has yet to learn that an extended and systematic education, up to and including the methods of original research, is now a necessary preliminary to the fullest development of industry, and it is to the gradual but sure growth of public opinion

APPARATUS FOR THE CONGELATION OF WATER BY MEANS OF SULPHURIC ACID.

in this direction that we must look for the means of securing to this country in the future, as in the past, the highest position as an industrial nation."

If, in the second place, we consider the influence which Englishmen have exerted on the teaching of our science, we shall feel reason for satisfaction; many of our text-books are translated into every European language and largely used abroad, often to the exclusion of those written by Continental chemists.

Again, science teaching, both practical and theoretical, in our elementary and many secondary schools, is certainly not inferior to that in schools of similar grade abroad, and the interest in and desire for scientific training is rapidly spreading throughout our working population, and is even now as great as, if not greater than, abroad. The universities and higher colleges are also moving to take their share of the work which has hitherto been far less completely done in our country than on the continent of Europe, especially in Germany, where the healthful spirit of competition, fostered by the numerous state-supported institutions, is much more common than with us, and, being of equal value in educational as in professional or commercial matters, has had its due effect.

Turning lastly to the practical applications of our science, in what departments does England not excel, and in which has she not made the most important new departures? Even in color chemistry, concerning which we have heard, with truth, much of German supremacy, we must remember that the industry is originally an English one, as the names of Perkin and of Maule, Simpson and Nicholson, testify; and if we have hitherto been beaten hollow in the development of this branch, signs are not wanting that this may not always be the case. But take any other branch of applied chemistry, the alkali trade for instance—what names but English, with the two great exceptions of Leblanc and Solvay, do we find in connection with real discoveries? In the application of chemistry to metallurgical processes, too, the names of Darby, Cort, Neilson, and Bell in iron, of Bessemer, Thomas, Gilchrist, and Snelus in steel, of Elkington, and Matthey in the noble metals, show that in these branches the discoveries which have revolutionized processes have been made by Englishmen; while Young, the father of paraffin, Spence the alum-maker, and Abel of gun-cotton fame, are some among many of our countrymen whose names may be honorably mentioned as having founded new chemical industries.

Hence, while there is much to stimulate us to action in the energy and zeal shown by our Continental brethren in the pursuit both of pure and applied chemistry, there is nothing to lead us to think that the chemistry of the English-speaking nations in the next fifty years will be less worthy than that of the past half-century of standing side by side with that of her friendly rivals elsewhere.

CONGELATION OF WATER BY MEANS OF SULPHURIC ACID.

A CONVENIENT and useful apparatus for the conversion of water into ice has been devised by K. Berberich, which consists of a flask, A, of about 15 cm. diameter, and two receptacles for sulphuric acid, each of which measures 10 cm. diametrically. The latter two are connected by a glass tubing 4 cm. wide and 15 cm. in length. The neck of the globe, B, to the left, is elongated, double bent, and closes hermetically the flask, A, while the bent tubing, C, is accurately ground into the globe, and sealed by means of sealing wax at the center of the metallic disk, L. On filling the globes to one-half of their capacity with concentrated sulphuric acid, and charging the flask partly with water, ebullition of the latter, after actuating the air pump, commences almost immediately, and is converted into ice within four or five minutes. The aqueous water rising from the water is avidiously absorbed by the acid in the first globe, causing a central depression of the surface of the liquid in the globe. When once charged with sulphuric acid, the apparatus can be used for four or five experiments.—*Annalen der Physik*.

AGAINST WROUGHT IRON FOR WATER PIPES.

To the Editor of the Scientific American:

Referring to your article on "Chemical Obstructions in Iron Water Pipes," I beg leave to say that some eighteen or twenty years since I laid down about 2,400 feet of "welded tube" or gas pipe of $1\frac{1}{2}$ inches diameter, for the purpose of supplying my establishment with water—a neighbor of mine having used such pipe successfully for twenty-two years preceding, and it being still in good order at this time. For a year or two I had an abundant flow of water—7 gallons per minute or upward of 10,000 gallons in twenty-four hours—but after eighteen months the flow of water was sensibly diminished, and at the end of three years it had dwindled to a mere trickle. When I took up one of the pipes, I found it impossible to see through it or force a stick through it. Closer examination disclosed the fact that small nodules had been formed on the inner circumference of the pipe, which by gradual accretion from the oxide (or sesquioxide) of iron had grown (analogous to the formation of stalactites in caves) until the whole space was nearly filled up by vertical formations, and not longitudinal ones, as cited by Col. Ludlow. I had to resort to the slow and tedious process of working in and through a very small wire, which, by churning around, broke off some of these iron "stalactites." Then I introduced a larger wire, and so gradually cleared the pipe; but many of the "stalactites" were so large that in coming

out they left a hole in the pipe, much of which was thus rendered worthless.

In striking contrast to these welded tubes was the condition of about 300 feet of cast iron pipes which had been in the same service for twenty years! Not a single nodule or stalactite had been formed in the cast iron in twenty years, while in about three years the welded tube was substantially filled with them!

I have from time to time submitted this remarkable difference to men experienced in the laying of water and gas pipes, but most of them declared themselves unable to solve the difficulty. A friend in Richmond, Virginia, suggested that in welding the tube it had become decarbonized, and hence (perhaps) its affinity for the oxide of iron, with which our water is frequently surcharged. Our soil is a deep red clay, its coloring matter being due (according to Prof. Rogers) to oxide of iron. Can you or some of your readers solve the problem?

B. JOHNSON BARBOUR.
Barboursville, Orange County, Va., Sept. 10, 1884.

DETERMINATION OF NITRIC ACID.

By M. ARNAUD.

CINCHONAMINE nitrate is almost insoluble in water acidified with from 10 to 15 per cent. of hydrochloric acid. It is therefore very easy to detect the nitrates qualitatively by bringing the solution to this condition. But for an exact determination it is impossible to employ this means, as, in drying, the acid becomes concentrated, and finally attacks not only the paper of the filter, but the cinchonamine nitrate itself.

After many attempts the author has succeeded in obtaining very good results by operating as follows:

The liquid containing the nitrates is neutralized with soda if acid, and with sulphuric acid if alkaline, the essential point being to obtain neutrality. The chlorine of the chlorides, if any are present, is eliminated with silver acetate, any excess of the latter being removed by the addition of a few drops of sodium phosphate. The filtrate is then evaporated almost to dryness and filtered anew if it is not absolutely clear, acidulated very slightly with a drop of dilute acetic acid. The liquid is then precipitated at a boil with a hot solution of cinchonamine sulphate. Cinchonamine nitrate falls down immediately in a crystalline state. It is let stand for twelve hours in a cool place. It is then brought upon a filter and washed with an aqueous solution of cinchonamine nitrate, saturated at the temperature of the atmosphere so as to remove any cinchonamine sulphate.

The object of this method of washing is to avoid dissolving any trace of the cinchonamine nitrate, however slight. Pure water, employed instead, would dissolve about 0.002 of its