natural history collections, though owing to want of space it has been impossible up to the present time to arrange or utilize them in a proper manner. The nucleus is formed by the collections previously in the possession of the Manchester Natural History and Geological Societies, which were transferred to the College in 1867 and 1869 respectively: to these, very valuable additions have since been made by gift, bequest, or purchase. The general Geological collection is a very good one; the Tertiary collections, including those made by Prof. Boyd Dawkins and by Mr. Waters, being of exceptional importance, and the Coal Measure series being one of the best in existence. In Mineralogy the David Forbes Collection, which was purchased by the College in 1877, is well known. In Zoology there is a good osteological series; and the collections of shells, including those presented by Mr. Cholmondeley and by Mr. Walton, and of insects are unusually complete, and in exceedingly good condition. The Botanical Museum contains a very fine British herbarium, and Prof. Williamson's unique collections illustrating the Carboniferous flora. The Museum will thus start very fairly equipped, and it may reasonably be hoped that the stimulus caused by the opening of the new buildings will lead to additional gifts and bequests, which will speedily render the collection one worthy in all respects of the College and of the city which has created it.

In the Laboratory block, which occupies the north side of the quadrangle, between the older buildings and the Museum, and is shown in the right-hand corner of the illustration, the ground floor contains on the inner side two lecture-theatres, seating 200 and 80 respectively, with convenient preparation and diagram rooms. On the other side, facing the street, are the mineralogical and petrological laboratories, geological laboratories, geological drawing room, a laboratory for applied geology, and private rooms for the professors and lecturers.

The Botanical Department is on the second floor, and comprises a large laboratory 42 feet by 28 feet, private rooms for the professor and for the demonstrator, and a dark room for physiological experiments. Provision is also made for a greenhouse 20 feet square, in direct connexion with the Laboratory.

The Zoological Laboratories occupy the third and part of the second floor.

The Junior and Senior Laboratories, which are in free communication with each other, measure 42 feet by 37 feet and 42 feet by 16 feet respectively; they are 29 feet high, and are extremely well lighted and equipped. In the Junior Laboratory the tables run north and south; each student has his own locker and drawer at his side, and gas- and water-supply in front of him; larger sinks with hot-water spirals are in the corners of the rooms; a large demonstration-table, with drawers and cupboards beneath, occupies the centre of the room; and a lecture-table and black-board are placed against the north wall. In the Senior Laboratory the tables face north. A gallery runs along the east and west walls of the laboratories, but has not yet been fitted up.

Besides these laboratories there are a Zoological Research Laboratory 42 feet by 16 feet; private rooms for the professor and for the demonstrators; a very convenient tank-room; and large storage space.

The building has concrete floors throughout; the heating is by hot water, and there is a very efficient system of ventilation. At each floor there is free communication between the Laboratory and Museum blocks, and the lift is placed midway between these two.

The Zoological and Botanical Laboratories have been in use since Christmas; the Museum will not be fitted up till October. An excellent opportunity for seeing the buildings is afforded by the meeting of the British Association. The ground-floor museum is being used for the reception-room and post-office, and the upper museum for reading- and writing-room, ladies' room, smoking-room, &c.; while the quadrangle is occupied by temporary luncheon-rooms and lavatories. The Section Rooms are partly in the College and partly in its immediate neighbourhood.

## THE BRITISH ASSOCIATION.

MANCHESTER, *Tuesday Evening*.

UP to the present the third Manchester meeting of the British Association promises to be as successful as everyone expected it would be. Probably no Local Committee has ever made more strenuous exertions to command success than that which for many months past has been busying itself with preparations for the present meeting. It would be difficult to suggest any improvements on the local preparations. The Reception Room in Owens College is spacious and is entirely confined to business. The Reading Rooms, Ladies' Rooms, Smoking Rooms, and Exhibition Galleries are all upstairs away from the crowd and noise. The Luncheon Rooms can accommodate hundreds, and the Sectional Rooms have had the special care of the Committee, several of whom know well the practical requirements of Sectional work. It is perhaps unfortunate that the rooms for D, E, F, and G are a long way from the Reception Room; but that has been unavoidable. The exhibition in the galleries of the Reading and Writing Room is of special interest. The anthropological collections contributed by Dr. Fritsch, Mr. Coutts Trotter, and others, are extensive and varied and highly instructive. Besides these there are collections of physical instruments by Sir William Thomson and Mr. W. H. Gee, and a fine series of models and apparatus for teaching practical physics in schools and colleges, exhibited by the Owens College Physical Department. In Section C, Prof. Boyd Dawkins exhibits several museum appliances, and Mr. J. H. Teall a series of specimens illustrating his paper on "The Origin of Certain Banded Gneisses." Other exhibits come under Sections D, G, and H, and the whole collection is likely to attract many visitors.

It is not expected that in numbers the present meeting will exceed the Newcastle meeting of 1863, when 3335 persons were present, or even the last meeting in this city in 1861, when the number reached 3138. But of course at present it is impossible to say. Some weeks ago the number who had taken tickets exceeded 2000, and to-day and to-morrow it is probable that at least another 1000 will be added. Whatever may be the number, it is certain that few past meetings of the Association will have surpassed the present in quality and weight. The marked feature is the number of foreign men of science who have promised to attend. The names of most of them have already appeared in NATURE. Their presence is entirely due to the exertions of the Local Committee, and especially, we believe, of Dr. Schuster. Nearly every man of any eminence in science abroad had a cordial letter of invitation to come to Manchester as a guest of the Local Committee, and the result is that over 100 have accepted. Among those who have arrived in Manchester to day are Prof. Riley, of Washington; Prof. Rowlands, of Baltimore; Prof. Langley, of Michigan; Prof. Dewalque, from Belgium; and Prof. Fittica, of Marburg. Among others who are expected to-morrow I need only mention such names as those of Cleveland Abbe, Neumayer, A. C. Young, Asa Gray, Mendeléeff, Pringsheim, G. Wiedemann, Wislicenus, F. Zirkel, De Bary, Cohn, His, and the two Saportas.

Several important discussions have been arranged for. One between Sections C and D on the arrangement of natural history museums, will be led off by Dr. Woodward on Friday morning. There will be then other discussions in Section D on questions of the greatest scientific interest, while electrolysis will come up again,

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I believe, in Sections A and B. A joint discussion on gold and silver has been arranged between Sections C and F. As these discussions will be real, and as several eminent foreigners are expected to take part in them, the meeting on the whole promises to be lively.

The social distractions—*conversaziones*, receptions, dinners, and excursions—are perplexingly numerous. The hand-books for the excursions have been got up with much care and thoughtfulness. There is, indeed, a separate little hand-book for each excursion, the whole set being done up in a case. Another hand-book of about one hundred pages gives an epitome of the history, antiquities, meteorology, physiography, flora and fauna of Manchester and the district.

Thus, so far as the officials are concerned, everything has been done to make the Manchester meeting a success. At the present moment the weather is not quite what could be wished; it is raining hard, and the weather is oppressively sultry. We can only hope it will improve before active operations begin.

INAUGURAL ADDRESS BY SIR HENRY E. ROSCOE, M.P., D.C.L., LL.D., PH.D., F.R.S., V.P.C.S., PRESIDENT.

MANCHESTER, distinguished as the birthplace of two of the greatest discoveries of modern science, heartily welcomes to-day, for the third time, the members and friends of the British Association for the Advancement of Science.

On the occasion of our first meeting in this city in the year 1842, the President, Lord Francis Egerton, commenced his address with a touching allusion to the veteran of science, John Dalton, the great chemist, the discoverer of the laws of chemical combination, the framer of the atomic theory upon which the modern science of chemistry may truly be said to be based. Lord Francis Egerton said:—"Manchester is still the residence of one whose name is uttered with respect wherever science is cultivated, who is here to-night to enjoy the honours due to a long career of persevering devotion to knowledge, and to receive from myself, if he will condescend to do so, the expression of my own deep personal regret that increase of years, which to him up to this hour has been but increase of wisdom, should have rendered him, in respect of mere bodily strength, unable to fill on this occasion an office which in his case would have received more honour than it could confer. I do regret that any cause should have prevented the present meeting in his native town from being associated with the name"—and here I must ask you to allow me to exchange the name of Dalton in 1842 for that of Joule in 1887, and to add, again in the words of the President of the former year, that I would gladly have served as a doorkeeper in any house where Joule, the father of science in Manchester, was enjoying his just pre-eminence.

For it is indeed true that the mantle of John Dalton has fallen on the shoulders of one well worthy to wear it, one to whom science owes a debt of gratitude not less than that which it willingly pays to the memory of the originator of the atomic theory. James Prescott Joule it was who, in his determination of the mechanical equivalent of heat, about the very year of our first Manchester meeting, gave to the world of science the results of experiments which placed beyond reach of doubt or cavil the greatest and most far-reaching scientific principle of modern times; namely, that of the conservation of energy. This, to use the words of Tyndall, is indeed a generalization of conspicuous grandeur fit to take rank with the principle of gravitation; more momentous, if that be possible, combining as it does the energies of the material universe into an organic whole, and enabling the eye of science to follow the flying shuttles of the universal power as it weaves what the Erdgeist in "Faust" calls "the living garment of God."

It is well, therefore, for us to remember, in the midst of the turmoil of our active industrial and commercial life, that Manchester not only well represents the energy of England in these practical directions, but that it possesses even higher claims to our regard and respect as being the seat of discoveries of which the value not only to pure science is momentous, but which also lie at the foundation of all our material progress and all our industrial success. For without a knowledge of the laws of chemical combination all the marvellous results with which modern industrial chemistry has astonished the world could not have been achieved, whilst the knowledge of the quantitative relations existing between the several forms of energy, and the possibility of expressing their amount in terms of ordinary mechanics, are matters which now constitute the life-breath of every branch of applied science. For example, before Dalton's discovery every manufacturer of oil of vitriol—a substance now made each week in thousands of tons within a few miles of this spot—every manufacturer had his own notions of the quantity of sulphur which he ought to burn in order to make a certain weight of sulphuric acid, but he had no idea that only a given weight of sulphur can unite with a certain quantity of oxygen and of water to form the acid, and that an excess of any one of the component parts was not only useless but harmful. Thus, and in tens of thousands of other instances, Dalton replaced rule of thumb by scientific principle. In like manner the applications of Joule's determination of the mechanical equivalent of heat are even more general; the increase and measurement of the efficiency of our steam-engines and the power of our dynamos are only two of the numerous examples which might be adduced of the practical value of Joule's work.

If the place calls up these thoughts, the time of our meeting also awakens memories of no less interest, in the recollection that we this year celebrate the Jubilee of Her Most Gracious Majesty's accession to the throne. It is right that the members of the British Association for the Advancement of Science should do so with heart and voice, for, although science requires and demands no royal patronage, we thereby express the feeling which must be uppermost in the hearts of all men of science, the feeling of thankfulness that we have lived in an age which has witnessed an advance in our knowledge of Nature, and a consequent improvement in the physical, and let us trust also in the moral and intellectual, well-being of the people hitherto unknown; an age with which the name of Victoria will ever be associated.

To give even a sketch of this progress, to trace even in the merest outline the salient points of the general history of science during the fifty momentous years of Her Majesty's reign, is a task far beyond my limited powers. It must suffice for me to point out to you, to the best of my ability, some few of the steps of that progress as evidenced in the one branch of science with which I am most familiar, and with which I am more closely concerned, the science of chemistry.

In the year 1837 chemistry was a very different science from that existing at the present moment. Priestley, it is true, had discovered oxygen, Lavoisier had placed the phenomena of combustion on their true basis, Davy had decomposed the alkalies, Faraday had liquefied many of the gases, Dalton had enunciated the laws of chemical combination by weight, and Gay-Lussac had pointed out the fact that a simple volumetric relation governs the combination of the gases. But we then possessed no knowledge of chemical dynamics, we were then altogether unable to explain the meaning of the heat given off in the act of chemical combination. The atomic theory was indeed accepted, but we were as ignorant of the mode of action of the atoms and as incapable of explaining their mutual relationship as were the ancient Greek philosophers. Fifty years ago, too, the connexion existing between the laws of life, vegetable and animal, and the phenomena of inorganic chemistry, was ill understood. The idea that the functions of living beings are controlled by the same forces, chemical and physical, which regulate the changes occurring in the inanimate world, was then one held by only a very few of the foremost thinkers of the time. Vital force was a term in everyone's mouth, an expression useful, as Goethe says, to disguise our ignorance, for

"Wo die Begriffe fehlen,
Da tritt ein Wort zur rechten Zeit sich ein."

Indeed the pioneer of the chemistry of life, Liebig himself, cannot quite shake himself free from the bonds of orthodox opinion, and he who first placed the phenomena of life on a true basis cannot trust his chemical principles to conduct the affairs of the body, but makes an appeal to vital force to help him out of his difficulties; as when in the body politic an unruly mob requires the presence and action of physical force to restrain it and to bring its members under the saving influence of law and order, so too, according to Liebig's views, in the body corporeal a continual conflict between the chemical forces and the vital power occurs throughout life, in which the latter, when it prevails, insures health and a continuance of life, but of which defeat insures disease or death. The picture presented to the student of to-day is a very different one. We now believe that no such conflict is possible, but that life is governed by chemical and physical

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forces, even though we cannot in every case explain its phenomena in terms of these forces; that whether these tend to continue or to end existence depends upon their nature and amount, and that disease and death are as much a consequence of the operation of chemical and physical laws as are health and life.

Looking back again to our point of departure fifty years ago, let us for a moment glance at Dalton's labours, and compare his views and those of his contemporaries with the ideas which now prevail. In the first place it is well to remember that the keystone of his atomic theory lies not so much in the idea of the existence and the indivisible nature of the particles of matter—though this idea was so firmly implanted in his mind that, being questioned on one occasion on the subject, he said to his friend the late Mr. Ransome, "Thou knowst it must be so, for no man can split an atom"—as in the assumption that the weights of these particles are different. Thus whilst each of the ultimate particles of oxygen has the same weight as every other particle of oxygen, and each atom of hydrogen, for example, has the same weight as every other particle of hydrogen, the oxygen atom is sixteen times heavier than that of hydrogen, and so on for the atoms of every chemical element, each having its own special weight. It was this discovery of Dalton, together with the further one that the elements combine in the proportions indicated by the relative weights of their atoms or in multiples of these proportions, which at once changed chemistry from a qualitative to a quantitative science, making the old invocation prophetic, "Thou hast ordered all things in measure and number and weight."

The researches of chemists and physicists during the last fifty years have not only strengthened but broadened the foundations of the great Manchester philosopher's discoveries. It is true that his original numbers, obtained by crude and inaccurate methods, have been replaced by more exact figures, but his laws of combination and his atomic explanation of those laws stand as the great bulwarks of our science.

On the present occasion it is interesting to remember that within a stone's-throw of this place is the small room belonging to our Literary and Philosophical Society which served Dalton as his laboratory. Here, with the simplest of all possible apparatus—a few cups, penny ink-bottles, rough balances, and self-made thermometers and barometers—Dalton accomplished his great results. Here he patiently worked, marshalling facts to support his great theory, for as an explanation of his laborious experimental investigations the wise old man says: "Having been in my progress so often misled by taking for granted the results of others, I have determined to write as little as possible but what I can attest by my own experience." Nor ought we when here assembled to forget that the last three of Dalton's experimental essays—one of which, on a new method of measuring water of crystallization, contained more than the germ of a great discovery—were communicated to our Chemical Section in 1842, and that this was the last memorable act of his scientific life. In this last of his contributions to science, as in his first, his method of procedure was that which has been marked out as the most fruitful by almost all the great searchers after Nature's secrets; namely, the assumption of a certain view as a working hypothesis, and the subsequent institution of experiment to bring this hypothesis to a test of reality upon which a legitimate theory is afterwards to be based. "Dalton," as Henry well says, "valued detailed facts mainly, if not solely, as the stepping-stones to comprehensive generalizations."

Next let us ask what light the research of the last fifty years has thrown on the subject of the Daltonian atoms: first, as regards their size; secondly, in respect to their indivisibility and mutual relationships; and thirdly, as regards their motions.

As regards the size and shape of the atoms, Dalton offered no opinion, for he had no experimental grounds on which to form it, believing that they were inconceivably small and altogether beyond the grasp of our senses aided by the most powerful appliances of art. He was in the habit of representing his atoms and their combinations diagrammatically as round disks or spheres made of wood, by means of which he was fond of illustrating his theory. But such mechanical illustrations are not without their danger, for I well remember the answer given by a pupil to a question on the atomic theory: "Atoms are round balls of wood invented by Dr. Dalton." So determinedly indeed did he adhere to his mechanical method of representing the chemical atoms and their combinations that he could not be prevailed upon to adopt the system of chemical formulæ introduced by Berzelius and now universally employed. In a letter addressed to Graham in April 1837, he writes: "Berzelius's symbols are horrifying. A young student in chemistry might as soon learn Hebrew as make himself acquainted with them." And again: "They appear to me equally to perplex the adepts in science, to discourage the learner, as well as to cloud the beauty and simplicity of the atomic theory."

But modern research has accomplished, as regards the size of the atom, at any rate to a certain extent, what Dalton regarded as impossible. Thus in 1865, Loschmidt, of Vienna, by a train of reasoning which I cannot now stop to explain, came to the conclusion that the diameter of an atom of oxygen or nitrogen was 1/10,000,000 of a centimetre. With the highest known magnifying power we can distinguish the 1/40,000 part of a centimetre; if now we imagine a cubic box each of whose sides has the above length, such a box when filled with air will contain from 60 to 100 millions of atoms of oxygen and nitrogen. A few years later William Thomson extended the methods of atomic measurement, and came to the conclusion that the distance between the centres of contiguous molecules is less than 1/5,000,000 and greater than 1/1,000,000,000 of a centimetre; or, to put it in language more suited to the ordinary mind, Thomson asks us to imagine a drop of water magnified up to the size of the earth, and then tells us that the coarseness of the graining of such a mass would be something between a heap of small shot and a heap of cricket balls. Or again, to take Clifford's illustration, you know that our best microscopes magnify from 6000 to 8000 times; a microscope which would magnify that result as much again would show the molecular structure of water. Or again, to put it in another form, if we suppose that the minutest organism we can now see were provided with equally powerful microscopes, these beings would be able to see the atoms.

Next, as to the indivisibility of the atom, involving also the question as to the relationships between the atomic weights and properties of the several elementary bodies.

Taking Dalton's aphorism, "Thou knowst no man can split an atom," as expressing the view of the enunciator of the atomic theory, let us see how far this idea is borne out by subsequent work. In the first place, Thomas Thomson, the first exponent of Dalton's generalization, was torn by conflicting beliefs until he found peace in the hypothesis of Prout, that the atomic weights of all the so-called elements are multiples of a common unit, which doctrine he sought to establish, as Thorpe remarks, by some of the very worst quantitative determinations to be found in chemical literature, though here I may add that they were not so incorrect as Dalton's original numbers.

Coming down to a somewhat later date, Graham, whose life was devoted to finding what the motion of an atom was, freed himself from the bondage of the Daltonian aphorism, and defined the atom not as a thing which cannot be divided, but as one which has not been divided. With him, as with Lucretius, as Angus Smith remarks, the original atom may be far down.

But speculative ideas respecting the constitution of matter have been the scientific relaxation of many minds from olden time to the present. In the mind of the early Greek the action of the atom as one substance taking various forms by unlimited combinations was sufficient to account for all the phenomena of the world. And Dalton himself, though upholding the indivisibility of his ultimate particles, says: "We do not know that any of the bodies denominated elementary are absolutely indecomposable." Again, Boyle, treating of the origin of form and quality, says: "There is one universal matter common to all bodies—an extended divisible and impenetrable substance." Then Graham in another place expresses a similar thought when he writes: "It is conceivable that the various kinds of matter now recognized as different elementary substances may possess one and the same ultimate or atomic molecules existing in different conditions of movement. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies."

What experimental evidence is now before us bearing upon these interesting speculations? In the first place, then, the space of fifty years has completely changed the face of the inquiry. Not only has the number of distinct well-established elementary bodies increased from fifty-three in 1837 to seventy in 1887 (not including the *twenty* or more new elements recently said to have been discovered by Krüss and Nilson in certain rare Scandinavian minerals), but the properties of these elements have been studied, and are now known to us with a degree of precision then undreamt of. So that relationships existing between these

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bodies which fifty years ago were undiscernible are now clearly manifest, and it is to these relationships that I would for a moment ask your attention. I have already stated that Dalton measured the relative weights of the ultimate particles by assuming hydrogen as the unit, and that Prout believed that on this basis the atomic weights of all the other elements would be found to be multiples of the atomic weight of hydrogen, thus indicating that an intimate constitutional relation exists between hydrogen and all the other elements.

Since the days of Dalton and Prout the truth or otherwise of Prout's law has been keenly contested by the most eminent chemists of all countries. The inquiry is a purely experimental one, and only those who have a special knowledge of the difficulties which surround such inquiries can form an idea of the amount of labour and self-sacrifice borne by such men as Dumas, Stas, and Marignac in carrying out delicate researches on the atomic weights of the elements. What is, then, the result of these most laborious experiments? It is that, whilst the atomic weights of the elements are not exactly either multiples of the unit or of half the unit, many of the numbers expressing most accurately the weight of the atom approximate so closely to a multiple of that of hydrogen, that we are constrained to admit that these approximations cannot be a mere matter of chance, but that some reason must exist for them. What that reason is, and why a close approximation and yet something short of absolute identity exists, is as yet hidden behind the veil; but who is there that doubts that when this Association celebrates its centenary, this veil will have been lifted, and this occult but fundamental question of atomic philosophy shall have been brought into the clear light of day?

But these are by no means all the relationships which modern science has discovered with respect to the atoms of our chemical elements. So long ago as 1829 Döbereiner pointed out that certain groups of elements exist presenting in all their properties strongly marked family characteristics, and this was afterwards extended and insisted upon by Dumas. We find, for example, in the well-known group of chlorine, bromine, and iodine, these resemblances well developed, accompanied moreover by a proportional graduation in their chemical and physical properties. Thus, to take the most important of all their characters, the atomic weight of the middle term is the mean of the atomic weights of the two extremes. But these groups of triads appeared to be unconnected in any way with one another, nor did they seem to bear any relation to the far larger number of the elements not exhibiting these peculiarities.

Things remained in this condition until 1863, when Newlands threw fresh light upon the subject, showing a far-reaching series of relationships. For the first time we thus obtained a glance into the mode in which the elements are connected together, but, like so many new discoveries, this did not meet with the recognition which we now see it deserves. But whilst England thus had the honour of first opening up this new path, it is to Germany and to Russia that we must look for the consummation of the idea. Germany, in the person of Lothar Meyer, keeps, as it is wont to do, strictly within the limits of known facts. Russia, in the person of Mendelejeff, being of a somewhat more imaginative nature, not only seizes the facts which are proved, but ventures upon prophecy. These chemists, amongst whom Carnelley must be named, agree in placing all the elementary bodies in a certain regular sequence, thus bringing to light a periodic recurrence of analogous chemical and physical properties, on account of which the arrangement is termed the periodic system of the elements.

In order to endeavour to render this somewhat complicated matter clear to you, I may perhaps be allowed to employ a simile. Let us, if you please, imagine a series of human families: a French one, represented by Dumas; an English one, by name Newlands; a German one, the family of Lothar Meyer; and lastly, a Russian one, that of Mendelejeff. Let us next imagine the names of these chemists placed in a horizontal line in the order I have mentioned. Then let us write under each the name of his father, and again, in the next lower line, that of his grandfather, followed by that of his great-grandfather, and so on. Let us next write against each of these names the number of years which has elapsed since the birth of the individual. We shall then find that these numbers regularly increase by a definite amount, *i.e.* by the average age of a generation, which will be approximately the same in all the four families. Comparing the ages of the chemists themselves we shall observe certain differences, but these are small in comparison with the period which has elapsed since the birth of any of their ancestors. Now each individual in this series of family trees represents a chemical element; and just as each family is distinguished by certain idiosyncrasies, so each group of the elementary bodies thus arranged shows distinct signs of consanguinity.

But more than this, it not unfrequently happens that the history and peculiarities of some member of a family may have been lost, even if the memory of a more remote and more famous ancestor may be preserved, although it is clear that such an individual must have had an existence. In such a case Francis Galton would not hesitate from the characteristics of the other members to reproduce the physical and even the mental peculiarities of the missing member; and should genealogical research bring to light the true personal appearance and mental qualities of the man, these would be found to coincide with Galton's estimate.

Such predictions and such verifications have been made in the case of no less than three of our chemical elements. Thus, Mendelejeff pointed out that if, in the future, certain lacunæ in his table were to be filled, they must be filled by elements possessing chemical and physical properties which he accurately specified. Since that time these gaps have actually been stopped by the discovery of gallium by Lecoq de Boisbaudron, of scandium by Nilson, and of germanium by Winkler, and their properties, both physical and chemical, as determined by their discoverers, agree absolutely with those predicted by the Russian chemist. Nay, more than this, we not unfrequently have had to deal with chemical foundlings, elements whose parentage is quite unknown to us. A careful examination of the personality of such waifs has enabled us to restore them to the family from which they have been separated by an unkind fate, and to give them that position in chemical society to which they are entitled.

These remarkable results, though they by no means furnish a proof of the supposition already referred to, viz. that the elements are derived from a common source, clearly point in this direction, and lend some degree of colour to the speculations of those whose scientific imagination, wearying of dry facts, revels in picturing to itself an elemental Bathybius, and in applying to the inanimate, laws of evolution similar to those which rule the animate world. Nor is there wanting other evidence regarding this inquiry, for here heat, the great analyzer, is brought into court. The main portion of the evidence consists in the fact that distinct chemical individuals capable of existence at low temperatures are incapable of existence at high ones, but split up into new materials possessing a less complicated structure than the original. And here it may be well to emphasize the distinction which the chemist draws between the atom and the molecule, the latter being a more or less complicated aggregation of atoms, and especially to point out the fundamental difference between the question of separating the atoms in the molecule and that of splitting up the atom itself. The decompositions above referred to are, in fact, not confined to compound bodies, for Victor Meyer has proved in the case of iodine that the molecule at high temperatures is broken to atoms, and J. J. Thomson has added to our knowledge by showing that this breaking up of the molecule may be effected not only by heat vibrations, but likewise by the electrical discharge at a comparatively low temperature.

How far, now, has this process of simplification been carried? Have the atoms of our present elements been made to yield? To this a negative answer must undoubtedly be given, for even the highest of terrestrial temperatures, that of the electric spark, has failed to shake any one of these atoms in two. That this is the case has been shown by the results with which spectrum analysis, that new and fascinating branch of science, has enriched our knowledge, for that spectrum analysis does give us most valuable aid in determining the varying molecular conditions of matter is admitted by all. Let us see how this bears on the question of the decomposition of the elements, and let us suppose for a moment that certain of our present elements, instead of being distinct substances, were made up of common ingredients, and that these compound elements, if I may be allowed to use so incongruous a term, are split up at the temperature of the electric spark into less complicated molecules. Then the spectroscopic examination of such a body must indicate the existence of these common ingredients by the appearance in the spark-spectra of these elements of identical bright lines. Coincidences of this kind have indeed been observed, but on careful examination these have been shown to be due either to the presence of

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some one of the other elements as an impurity, or to insufficient observational power. This absence of coincident lines admits, however, of two explanations—either that the elements are not decomposed at the temperature of the electric spark, or, what appears to me a much more improbable supposition, each one of the numbers of bright lines exhibited by every element indicates the existence of a separate constituent, no two of this enormous number being identical.

Terrestrial analysis having thus failed to furnish favourable evidence, we are compelled to see if any information is forthcoming from the chemistry of the sun and stars. And here I would remark that it is not my purpose now to dilate on the wonders which this branch of modern science has revealed. It is sufficient to remind you that chemists thus have the means placed at their disposal of ascertaining with certainty the presence of elements well known on this earth in fixed stars so far distant that we are now receiving the light which emanated from them perhaps even thousands of years ago.

Since Bunsen and Kirchhoff's original discovery in 1859, the labours of many men of science of all countries have largely increased our knowledge of the chemical constitution of the sun and stars, and to no one does science owe more in this direction than to Lockyer and Huggins in this country, and to Young in the New England beyond the seas. Lockyer has of late years devoted his attention chiefly to the varying nature of the bright lines seen under different conditions of time and place on the solar surface, and from these observations he has drawn the inference that the matching observed by Kirchhoff between, for instance, the iron lines as seen in our laboratories and those visible in the sun, has fallen to the ground. He further explains this want of uniformity by the fact that at the higher transcendental temperatures of the sun the substance which we know here as iron is resolved into separate components. Other experimentalists, however, while accepting Lockyer's facts as to the variations in the solar spectrum, do not admit his conclusions, and would rather explain the phenomena by the well-known differences which occur in the spectra of all the elements when their molecules are subject to change of temperature or change of position.

Further, arguments in favour of this idea of the evolution of the elements have been adduced from the phenomena presented by the spectra of the fixed stars. It is well known that some of these shine with a white, others with a red, and others again with a blue light; and the spectroscope, especially under the hands of Huggins, has shown that the chemical constitution of these stars is different. The white stars, of which Sirius may be taken as a type, exhibit a much less complicated spectrum than the orange and the red stars; the spectra of the latter remind us more of those of the metalloids and of chemical compounds than of the metals. Hence it has been argued that in the white, presumably the hottest, stars a celestial dissociation of our terrestrial elements may have taken place, whilst in the cooler stars, probably the red, combination even may occur. But even in the white stars we have no *direct* evidence that a decomposition of any terrestrial atom has taken place; indeed we learn that the hydrogen atom, as we know it here, can endure unscathed the inconceivably fierce temperature of stars presumably many times more fervent than our sun, as Sirius and Vega.

Taking all these matters into consideration, we need not be surprised if the earth-bound chemist should, in the absence of celestial evidence which is incontestable, continue, for the present at least, and until fresh evidence is forthcoming, to regard the elements as the unalterable foundation-stones upon which his science is based.

Pursuing another line of inquiry on this subject, Crookes has added a remarkable contribution to the question of the possibility of decomposing the elements. With his well-known experimental prowess, he has discovered a new and beautiful series of phenomena, and has shown that the phosphorescent lights emitted by certain chemical compounds, especially the rare earths, under an electric discharge in a high vacuum exhibit peculiar and characteristic lines. For the purpose of obtaining his material Crookes started from a substance believed by chemists to be homogeneous, such, for example, as the rare earth yttria, and succeeded by a long series of fractional precipitations in obtaining products which yield different phosphorescent spectra, although when tested by the ordinary methods of what we may term high temperature spectroscopy, they appear to be the one substance employed at the starting-point. The other touchstone by which the identity, or otherwise, of these various products might be ascertained, viz. the determination of their atomic weights, has not, as yet, engaged Crookes's attention. In explanation of these singular phenomena, the discoverer suggests two possibilities. First, that the bodies yielding the different phosphorescent spectra are different elementary constituents of the substance which we call yttria Or, if this be objected to because they all yield the same spark-spectrum, he adopts the very reasonable view that the Daltonian atom is probably, as we have seen, a system of chemical complexity; and adds to this the idea that these complex atoms are not all of exactly the same constitution and weight, the differences, however, being so slight that their detection has hitherto eluded our most delicate tests, with the exception of this one of phosphorescence in a vacuum. To these two explanations, Marignac, in a discussion of Crookes's results, adds a third. It having been shown by Crookes himself that the presence of the minutest traces of foreign bodies produce remarkable alterations in the phosphorescent spectra, Marignac suggests that in the course of the thousands of separations which must be made before these differences become manifest, traces of foreign bodies may have been accidently introduced, or, being present in the original material, may have accumulated to a different extent in the various fractions, their presence being indicated by the only test by which they can now be detected. Which of these three explanations is the true one must be left to future experiment to decide.

We must now pass from the statics to the dynamics of chemistry; that is, from the consideration of the atoms at rest to that of the atoms in motion. Here, again, we are indebted to John Dalton for the first step in this direction, for he showed that the particles of a gas are constantly flying about in all directions; that is, that gases diffuse into one another, as an escape of coal gas from a burner, for example, soon makes itself perceptible throughout the room. Dalton, whose mind was constantly engaged in studying the molecular condition of gases, first showed that a light gas cannot rest upon a heavier gas as oil upon water, but that an interpenetration of each gas by the other takes place. It is, however, to Graham's experiments, made rather more than half a century ago, that we are indebted for the discovery of the law regulating these molecular motions of gases, proving that their relative rates of diffusion are inversely proportional to the square roots of their densities, so that oxygen being 16 times heavier than hydrogen, their relative rates of diffusion are 1 and 4.

But whilst Dalton and Graham indicated that the atoms are in a continual state of motion, it is to Joule that we owe the first accurate determination of the rate of that motion. At the Swansea meeting, in 1848, Joule read a paper before Section A on the "Mechanical Equivalent of Heat and on the Constitution of Elastic Fluids." In this paper Joule remarks that whether we conceive the particles to be revolving round one another according to the hypothesis of Davy, or flying about in every direction according to Herapath's view, the pressure of the gas will be in proportion to the *vis viva* of its particles. "Thus it may be shown that the particles of hydrogen at the barometrical pressure of 30 inches at a temperature of 60° must move with a velocity of 6225·54 feet per second in order to produce a pressure of 14·714 lbs. on the square inch," or, to put it in other words, a molecular cannonade or hailstorm of particles, at the above rate—a rate, we must remember, far exceeding that of a cannon ball—is maintained against the bounding surface.

We can, however, go a step further and calculate with Clerk Maxwell the number of times in which this hydrogen molecule, moving at the rate of 70 miles per minute, strikes against others of the vibrating swarm, and we learn that in one second of time it must knock against others no less than 18 thousand million times.

And here we may pause and dwell for a moment on the reflection that in Nature there is no such thing as great or small, and that the structure of the smallest particle, invisible even to our most searching vision, may be as complicated as that of any one of the heavenly bodies which circle round our sun.

But how does this wonderful atomic motion affect our chemistry? Can chemical science or chemical phenomena throw light upon this motion, or can this motion explain any of the known phenomena of our science? I have already said that Lavoisier left untouched the dynamics of combustion. He could not explain why a fixed and unalterable amount of heat is in most cases emitted, but in some cases absorbed, when chemical combination takes place. What Lavoisier left unexplained Joule has made clear. On August 25, 1843, Joule read a short

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communication, I am glad to remember, before the Chemical Section of our Association, meeting that year at Cork, containing an announcement of a discovery which was to revolutionize modern science. This consisted in the determination of the mechanical equivalent of heat, in proving by accurate experiment that by the expenditure of energy equal to that developed by the weight of 772 pounds falling through 1 foot at Manchester, the temperature of 1 pound of water can be raised 1° F. In other words, every change in the arrangement of the particles is accompanied by a definite evolution or an absorption of heat. In all such cases the molecular energy leaves the potential to assume the kinetic form, or *vice versâ*. Heat is evolved by the clashing of the atoms, and this amount is fixed and definite.

Thus it is to Joule we owe the foundation of chemical dynamics and the basis of thermal chemistry. As the conservation of mass or the principle of the indestructibility of matter forms the basis of chemical statics, so the principle of the conservation of energy[1] constitutes the foundation of chemical dynamics. Change in the form of matter and change in the form of energy are the universal accompaniments of every chemical operation. Here again it is to Joule we owe the proof of the truth of this principle in another direction, viz. that when electrical energy is developed by chemical change a corresponding quantity of chemical energy disappears. Energy, as defined by Maxwell, is the power of doing work, and work is the act of producing a configuration in a system in opposition to a force which resists that change. Chemical action produces such a change of configuration in the molecules. Hence, as Maxwell says, 'A complete knowledge of the mode in which the potential energy of a system varies with the configuration would enable us to predict every possible motion of the system under the action of given external forces, provided we were able to overcome the purely mathematical difficulties of the calculation." The object of thermal chemistry is to measure these changes of energy by thermal methods, and to connect these with chemical changes, to estimate the attractions of the atoms and molecules to which the name of chemical affinity has been applied, and thus to solve the most fundamental problem of chemical science. How far has modern research approached the solution of this most difficult problem? How far can we answer the question, What is the amount of the forces at work in these chemical changes? What laws govern these forces? Well, even in spite of the results with which recent researches, especially the remarkable ones of the Danish philosopher Thomsen have enriched us, we must acknowledge that we are yet scarcely in sight of Maxwell's position of successful prediction. Thermal chemistry, we must acknowledge, is even yet in its infancy; it is, however, an infant of sturdy growth, likely to do good work in the world, and to be a credit to him who is its acknowledged father, as well as to those who have so carefully tended it in its early years.

But recent investigation in another direction bids fair even to eclipse the results which have been obtained by the examination of thermal phenomena. And this lies in the direction of electrical chemistry. Faraday's work relating to conductivity of chemical substances has been already referred to, and this has been since substantiated and extended to pure substances by Kohlrausch. It has been shown, for example, that the resistance of absolutely pure water is almost an infinite quantity. But a small quantity of an acid, such as acetic or butyric acid, greatly increases the conductivity; but more than this, it is possible by determination of the conductivity of a mixture of water with these two acids to arrive at a conclusion as to the partition of the molecules of the water between the acids. Such a partition, however, implies a change of position, and therefore we are furnished with a means of recognizing the motion of the molecules in a liquid, and of determining its amount. Thus it has been found that the hindrance to molecular motion is more affected by the chemical character of the liquid than by physical characters such as viscosity. We have seen that chemical change is always accompanied by molecular motion, and further evidence of the truth of this is gained from the extraordinary chemical inactivity of pure unmixed substances. Thus pure anhydrous hydrochloric acid does not act upon lime, whereas the addition of even a trace of moisture sets up a most active chemical change, and hundreds of other examples of a similar kind might be stated. Bearing in mind that these pure anhydrous compounds do not conduct, we are led to the conclusion that an intimate relation exists between chemical activity and conductivity. And we need not stop here; for a method is indicated indeed by which it will be possible to arrive at a measure of chemical affinity from determination of conductivity. It has indeed been already shown that the rate of change in the saponification of acetic ether is directly proportional to the conductivity of the liquid employed.

Such wide-reaching inquiries into new and fertile fields, in which we seem to come into nearer touch with the molecular state of matter, and within a measurable distance of accurate mathematical expression, leads to confident hope that Lord Rayleigh's pregnant words at Montreal may ere long be realized: "It is from the further study of electrolysis that we may expect to gain improved views as to the nature of chemical reactions, and of the forces concerned in bringing them about; and I cannot help thinking that the next great advance, of which we already have some foreshadowing, will come on this side."

There is, perhaps, no branch of our science in which the doctrine of the Daltonian atom plays a more conspicuous part than in organic chemistry or the chemistry of the carbon compounds, as there is certainly none in which such wonderful progress has been made during the last fifty years. One of the most striking and perplexing discoveries made rather more than half a century ago was that chemical compounds could exist which, whilst possessing an identical chemical composition, that is containing the same percentage quantity of their constituents, are essentially distinct chemical substances exhibiting different properties. Dalton was the first to point out the existence of such substances, and to suggest that the difference was to be ascribed to a different or to a multiple arrangement of the constituent atoms. Faraday soon afterwards proved that this supposition was correct, and the research of Liebig and Wöhler on the identity of composition of the salts of fulminic and cyanic acid gave further confirmation to the conclusion, leading Faraday to remark that "now we are taught to look for bodies composed of the same elements in the same proportion, but differing in their qualities, they may probably multiply upon us." How true this prophecy has become we may gather from the fact that we now know of thousands of cases of this kind, and that we are able not only to explain the reason of their difference by virtue of the varying position of the atoms within the molecule, but even to predict the number of distinct variations in which any given chemical compound can possibly exist. How large this number may become may be understood from the fact that, for example, one chemical compound, a hydrocarbon containing thirteen atoms of carbon combined with twenty-eight atoms of hydrogen, can be shown to be capable of existing in no less than 802 distinct forms.

Experiment in every case in which it has been applied has proved the truth of such a prediction, so that the chemist has no need to apply the cogent argument sometimes said to be used by experimentalists enamoured of pet theories, "When facts do not agree with theory, so much the worse for the facts"! This power of successful prediction constitutes a high-water mark in science, for it indicates that the theory upon which such a power is based is a true one.

But if the Daltonian atom forms the foundation of this theory, it is upon a knowledge of the mode of arrangement of these atoms and on a recognition of their distinctive properties that the superstructure of modern organic chemistry rests. Certainly it does appear almost to verge on the miraculous that chemists should now be able to ascertain with certainty the relative position of atoms in a molecule so minute that millions upon millions, like the angels in the schoolmen's discussion, can stand on a needle's point. And yet this process of orientation is one which is accomplished every day in our laboratories, and one which more than any other has led to results of a startling character. Still, this sword to open the oyster of science would have been wanting to us if we had not taken a step farther than Dalton did, in the recognition of the distinctive nature of the elemental atoms. We now assume on good grounds that the atom of each element possesses distinct capabilities of combination: some a single capability, others a double, others a triple, and others again a fourfold combining capacity. The germs of this theory of valency, one of the most fruitful of modern chemical ideas, were enunciated by Frankland in 1852, but the definite explanation of the linking of atoms, of the tetrad nature of the carbon atoms, their power of combination, and of the difference in structure between the fatty and aromatic series of compounds,

[1] "The total energy of any material system is a quantity which can neither be increased nor diminished by any action between the parts of the system, though it may be transformed into any of the forms of which energy is susceptible."—MAXWELL.

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was first pointed out by Kekulé in 1857; though we must not forget that this great principle was foreshadowed so long ago as 1833 from a physical point of view by Faraday in his well-known laws of electrolysis, and that it is to Helmholtz, in his celebrated Faraday Lecture, that we owe the complete elucidation of the subject; for, whilst Faraday has shown that the number of the atoms electrolytically deposited is in the inverse ratio of their valencies, Helmholtz has explained this by the fact that the quantity of electricity with which each atom is associated is directly proportional to its valency.

Amongst the tetrad class of elements, carbon, the distinctive element of organic compounds, finds its place; and the remarkable fact that the number of carbon compounds far exceeds that of all the other elements put together receives its explanation. For these carbon atoms not only possess four means of grasping other atoms, but these four-handed carbon atoms have a strong partiality for each other's company, and readily attach themselves hand in hand to form open chains or closed rings, to which the atoms of other elements join to grasp the unoccupied carbon hand, and thus to yield a dancing company in which all hands are locked together. Such a group, each individual occupying a given position with reference to the others, constitutes the organic molecule. When, in such a company, the individual members change hands, a new combination is formed. And as in such an assembly the eye can follow the changing positions of the individual members, so the chemist can recognize in his molecule the position of the several atoms, and explain by this the fact that each arrangement constitutes a new chemical compound possessing different properties, and account in this way for the decompositions which each differently constituted molecule is found to undergo.

Chemists are, however, not content with representing the arrangement of the atoms in one plane, as on a sheet of paper, but attempt to express the position of the atoms in space. In this way it is possible to explain certain observed differences in isomeric bodies, which otherwise baffled our efforts. To Van t'Hoff, in the first instance, and more recently to Wislicenus, chemistry is indebted for work in this direction, which throws light on hitherto obscure phenomena, and points the way to still further and more important advances.

It is this knowledge of the mode in which the atoms in the molecule are arranged, this power of determining the nature of this arrangement, which has given to organic chemistry the impetus which has overcome so many experimental obstacles, and given rise to such unlooked-for results. Organic chemistry has now become synthetic. In 1837 we were able to build up but very few and very simple organic compounds from their elements; indeed the views of chemists were much divided as to the possibility of such a thing. Both Gmelin and Berzelius argued that organic compounds, unlike inorganic bodies, cannot be built up from their elements. Organic compounds were generally believed to be special products of the so-called vital force, and it was only intuitive minds like those of Liebig and Wöhler who foresaw what was coming, and wrote in 1837 strongly against this view, asserting that the artificial production in our laboratories of all organic substances, so far as they do not constitute a living organism, is not only probable but certain. Indeed, they went a step farther, and predicted that sugar, morphia, salicine, will all thus be prepared; a prophecy which, I need scarcely remind you, has been after fifty years fulfilled, for at the present time we can prepare an artificial sweetening principle, an artificial alkaloid, and salicine.

In spite of these predictions, and in spite of Wöhler's memorable discovery in 1828 of the artificial production of urea, which did in reality break down for ever the barrier of essential chemical difference between the products of the inanimate and the animate world, still, even up to a much later date, contrary opinions were held, and the synthesis of urea was looked upon as the exception which proves the rule. So it came to pass that for many years the artificial production of any of the more complicated organic substances was believed to be impossible. Now the belief in a special vital force has disappeared like the *ignis fatuus*, and no longer lures us in the wrong direction. We know now that the same laws regulate the formation of chemical compounds in both animate and inanimate nature, and the chemist only asks for a knowledge of the constitution of any definite chemical compound found in the organic world in order to be able to promise to prepare it artificially.

But the progress of synthetic organic chemistry, which has of late been so rapid, was made in the early days of the half-century only by feeble steps and slow. Seventeen long years elapsed between Wöhler's discovery and the next real synthesis. This was accomplished by Kolbe, who in 1845 prepared acetic acid from its elements. But then a splendid harvest of results gathered in by chemists of all nations quickly followed, a harvest so rich and so varied that we are apt to be overpowered by its wealth, and amidst so much that is alluring and striking we may well find it difficult to choose the most appropriate examples for illustrating the power and the extent of modern chemical synthesis.

Next, as a contrast to our picture, let us for a moment glance back again to the state of things fifty years ago, and then notice the chief steps by which we have arrived at our present position. In 1837 organic chemistry possessed no scientific basis, and therefore no classification of a character worthy of the name. Writing to Berzelius in that year, Wöhler describes the condition of organic chemistry as one enough to drive a man mad. "It seems to me," says he, "like the tropical forest primæval, full of the strangest growths, an endless and pathless thicket in which a man may well dread to wander." Still clearances had already been made in this wilderness of facts. Berzelius in 1832 welcomed the results of Liebig and Wöhler's research on benzoic acid as the dawn of a new era; and such it really was, inasmuch as it introduced a novel and fruitful idea—namely, the possibility of a group of atoms acting like an element by pointing out the existence of organic radicals. This theory was strengthened and confirmed by Bunsen's classical researches on the cacodyl compounds, in which he showed that a common group of elements which acts exactly as a metal can exist in the free state, and this was followed soon afterwards by isolation of the so-called alcohol radicals by Frankland and Kolbe. It is, however, to Schorlemmer that we owe our knowledge of the true constitution of these bodies, a matter which proved to be of vital importance for the further development of the science.

Turning our glance in another direction we find that Dumas in 1834 by this law of substitution threw light upon a whole series of singular and unexplained phenomena by showing that an exchange can take place between the constituent atoms in a molecule. Laurent indeed went farther, and assumed that a chlorine atom, for example, took up the position vacated by an atom of hydrogen and played the part of its displaced rival, so that the chemical and physical properties of the substitution-product were thought to remain substantially the same as those of the original body. A singular story is connected with this discovery. At a *soirée* in the Tuileries in the time of Charles X. the guests were almost suffocated by acrid vapours which were evidently emitted by the burning wax candles, and the great chemist Dumas was called in to examine into the cause of the annoyance. He found that the wax of which the candles were made had been bleached by chlorine, that a replacement of some of the hydrogen atoms of the wax by chlorine had occurred, and that the suffocating vapours consisted of hydrochloric acid given off during the combustion. The wax was as white and as odourless as before, and the fact of the substitution of chlorine for hydrogen could only be recognized when the candles were destroyed by burning. This incident induced Dumas to investigate more closely this class of phenomena, and the results of this investigation are embodied in his law of substitution. So far indeed did the interest of the French school of chemists lead them that some assumed that not only the hydrogen but also the carbon of organic bodies could be replaced by substitution. Against this idea Liebig protested, and in a satirical vein he informs the chemical public, writing from Paris under the *nom de plume* of S. Windler, that he has succeeded in substituting not only the hydrogen but the oxygen and carbon in cotton cloth by chlorine, and he adds that the London shops are now selling nightcaps and other articles of apparel made entirely of chlorine, goods which meet with much favour, especially for hospital use!

But the debt which chemistry, both inorganic and organic, thus owes to Dumas' law of substitution is serious enough, for it proved to be the germ of Williamson's classical researches on etherification, as well as of those of Wurtz and Hofmann on the compound ammonias, investigations which lie at the base of the structure of modern chemistry. Its influence has been, however, still more far-reaching, inasmuch as upon it depends in great measure the astounding progress made in the wide field of organic synthesis.

It may here be permitted to me to sketch in rough outline the

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principles upon which all organic syntheses have been effected. We have already seen that as soon as the chemical structure of a body has been ascertained its artificial preparation may be certainly anticipated, so that the first step to be taken is the study of the structure of the naturally occurring substance which it is desired to prepare artificially by resolving it into simpler constituents, the constitution of which is already known. In this way, for example, Hofmann discovered that the alkaloid coniine, the poisonous principle of hemlock, may be decomposed into a simpler substance well known to chemists under the name of pyridine. This fact having been established by Hofmann, and the grouping of the atoms approximately determined, it was then necessary to reverse the process, and, starting with pyridine, to build up a compound of the required constitution and properties, a result recently achieved by Ladenburg in a series of brilliant researches. The well-known synthesis of the colouring matter of madder by Graebe and Liebermann, preceded by the important researches of Schunck, and that of indigo by Baeyer, are other striking examples in which this method has been successfully followed.

Not only has this intimate acquaintance with the changes which occur within the molecules of organic compounds been utilized, as we have seen, in the synthesis of naturally occurring substances, but it has also led to the discovery of many new ones. Of these perhaps the most remarkable instance is the production of an artificial sweetening agent termed saccharin, 250 times sweeter than sugar, prepared by a complicated series of reactions from coal-tar. Nor must we imagine that these discoveries are of scientific interest only, for they have given rise to the industry of the coal-tar colours, the value of which is measured by millions sterling annually, an industry which Englishmen may be proud to remember was founded by our countryman Perkin.

Another interesting application of synthetic chemistry to the needs of every-day life is the discovery of a series of valuable febrifuges, amongst which I may mention antipyrin as the most useful. An important aspect in connexion with the study of these bodies is the physiological value which has been found to attach to the introduction of certain organic radicals, so that an indication is given of the possibility of preparing a compound which will possess certain desired physiological properties, or even to foretell the kind of action which such bodies may exert on the animal economy.

But it is not only the physiological properties of chemical compounds which stand in intimate relation with their constitution, for we find that this is the case with all their physical properties. It is true that at the beginning of our period any such relation was almost unsuspected, whilst at the present time the number of instances in which this connexion has been ascertained is almost infinite. Amongst these perhaps the most striking is the relationship which has been pointed out between the optical properties and chemical composition. This was in the first place recognized by Pasteur in his classical researches on racemic and tartaric acids in 1848; but the first to indicate a quantitative relationship and a connexion between chemical structure and optical properties was Gladstone in 1863. Great instrumental precision has been brought to bear on this question, and consequently most important practical applications have resulted. I need only refer to the well-known accurate methods now in every-day use for the determination of sugar by the polariscope, equally valuable to the physician and to the manufacturer.

But now the question may well be put, is any limit set to this synthetic power of the chemist? Although the danger of dogmatizing as to the progress of science has already been shown in too many instances, yet one cannot help feeling that the barrier which exists between the organized and unorganized worlds is one which the chemist at present sees no chance of breaking down.

It is true that there are those who profess to foresee that the day will arrive when the chemist, by a succession of constructive efforts, may pass beyond albumen, and gather the elements of lifeless matter into a living structure. Whatever may be said regarding this from other standpoints, the chemist can only say that at present no such problem lies within his province. Protoplasm, with which the simplest manifestations of life are associated, is not a compound, but a structure built up of compounds. The chemist may successfully synthetize any of its component molecules, but he has no more reason to look forward to the synthetic production of the structure than to imagine that the synthesis of gallic acid leads to the artificial production of gall-nuts.

Although there is thus no prospect of our effecting a synthesis of organized material, yet the progress made in our knowledge of the chemistry of life during the last fifty years has been very great, and so much so indeed that the sciences of physiological and of pathological chemistry may be said to have entirely arisen within this period.

In the introductory portion of this address I have already referred to the relations supposed to exist fifty years ago between vital phenomena and those of the inorganic world. Let me now briefly trace a few of the more important steps which have marked the progress of this branch of science during this period. Certainly no portion of our science is of greater interest, nor, I may add, of greater complexity, than that which, bearing on the vital functions both of plants and of animals, endeavours to unravel the tangled skein of the chemistry of life, and to explain the principles according to which our bodies live, and move, and have their being. If, therefore, in the less complicated problems with which other portions of our science have to deal, we find ourselves, as we have seen, often far from possessing satisfactory solutions, we cannot be surprised to learn that with regard to the chemistry of the living body—whether vegetable or animal—in health or disease we are still farther from a complete knowledge of phenomena, even those of fundamental importance.

It is of interest here to recall the fact that nearly fifty years ago Liebig presented to the Chemical Section of this Association a communication in which, for the first time, an attempt was made to explain the phenomena of life on chemical and physical lines, for in this paper he admits the applicability of the great principle of the conservation of energy to the functions of animals, pointing out that the animal cannot generate more heat than is produced by the combustion of the carbon and hydrogen of his food.

"The source of animal heat," says Liebig, "has previously been ascribed to nervous action or to the contraction of the muscles, or even to the mechanical motions of the body, as if these motions could exist without an expenditure of force [equal to that] consumed in producing them." Again he compares the living body to a laboratory furnace in which a complicated series of changes occur in the fuel, but in which the end-products are carbonic acid and water, the amount of heat evolved being dependent, not upon the intermediate, but upon the final products. Liebig asked himself the question, Does every kind of food go to the production of heat; or can we distinguish, on the one hand, between the kind of food which goes to create warmth, and, on the other, that by the oxidation of which the motions and mechanical energy of the body are kept up? He thought that he was able to do this, and he divided food into two categories. The starchy or carbohydrate food is that, said he, which by its combustion provides the warmth necessary for the existence and life of the body. The albuminous or nitrogenous constituents of our food, the flesh meat, the gluten, the casein out of which our muscles are built up, are not available for the purposes of creating warmth, but it is by the waste of those muscles that the mechanical energy, the activity, the motions of the animal are supplied. We see, said Liebig, that the Esquimaux feeds on fat and tallow, and this burning in his body keeps out the cold. The Gaucho, riding on the pampas, lives entirely on dried meat, and the rowing man and pugilist, trained on beefsteaks and porter, require little food to keep up the temperature of their bodies, but much to enable them to meet the demand for fresh muscular tissue, and for this purpose they need to live on a strongly nitrogenous diet.

Thus far Liebig. Now let us turn to the present state of our knowledge. The question of the source of muscular power is one of the greatest interest, for, as Frankland observes, it is the corner-stone of the physiological edifice and the key to the nutrition of animals.

Let us examine by the light of modern science the truth of Liebig's view—even now not uncommonly held—as to the functions of the two kinds of food, and as to the cause of muscular exercise being the oxidation of the muscular tissue. Soon after the promulgation of these views, J. R. Mayer, whose name as the first expositor of the idea of the conservation of energy is so well known, warmly attacked them, throwing out the hypothesis that all muscular action is due to the combustion of food, and not to the destruction of muscle, proving his case by showing that if the muscles of the heart be destroyed in doing

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mechanical work the heart would be burnt up in eight days! What does modern research say to this question? Can it be brought to the crucial test of experiment? It can; but how? Well, in the first place we can ascertain the work done by a man or any other animal; we can measure this work in terms of our mechanical standard, in kilogramme-metres or foot-pounds. We can next determine what is the destruction of nitrogenous tissue at rest and under exercise by the amount of nitrogenous material thrown off by the body. And here we must remember that these tissues are never completely burnt, so that free nitrogen is never eliminated. If now we know the heat-value of the burnt muscle, it is easy to convert this into its mechanical equivalent, and thus measure the energy generated. What is the result? Is the weight of muscle destroyed by ascending the Faulhorn or by working on the treadmill sufficient to produce on combustion heat enough when transformed into mechanical exercise to lift the body up to the summit of the Faulhorn or to do the work on the treadmill? Careful experiment has shown that this is so far from being the case that the actual energy developed is twice as great as that which could possibly be produced by the oxidation of the nitrogenous constituents eliminated from the body during twenty-four hours. That is to say, taking the amount of nitrogenous substance cast off from the body, not only whilst the work was being done but during twenty-four hours, the mechanical effect capable of being produced by the muscular tissue from which this cast-off material is derived would only raise the body half-way up the Faulhorn, or enable the prisoner to work half his time on the treadmill.

Hence it is clear that Liebig's proposition is not true. The nitrogenous constituents of the food do doubtless go to repair the waste of muscle, which, like every other portion of the body, needs renewal, whilst the function of the non-nitrogenous food is not only to supply the animal heat, but also to furnish, by its oxidation, the muscular energy of the body.

We thus come to the conclusion that it is the potential energy of the food which furnishes the actual energy of the body, expressed in terms either of heat or of mechanical work.

But there is one other factor which comes into play in this question of mechanical energy, and must be taken into account; and this factor we are as yet unable to estimate in our usual terms. It concerns the action of the mind upon the body, and, although incapable of exact expression, exerts none the less an important influence on the physics and chemistry of the body, so that a connexion undoubtedly exists between intellectual activity or mental work and bodily nutrition. In proof that there is a marked difference between voluntary and involuntary work, we need only compare the mechanical action of the heart, which never causes fatigue, with that of the voluntary muscles, which become fatigued by continued exertion. So, too, we know well that an amount of drill which is fatiguing to the recruit is not felt by the old soldier, who goes through the evolutions automatically. What is the expenditure of mechanical energy which accompanies mental effort, is a question which science is probably far removed from answering. But that the body experiences exhaustion as the result of mental activity is a well-recognized fact. Indeed, whilst the second law of thermodynamics teaches that in none of the mechanical contrivances for the conversion of heat into actual energy can such a conversion be complete, it is perhaps possible, as Helmholtz has suggested, that such a complete conversion may take place in the subtle mechanism of the animal organism.

The phenomena of vegetation, no less than those of the animal world, have, however, during the last fifty years been placed by the chemist on an entirely new basis. Although before the publication of Liebig's celebrated report on chemistry and its application to agriculture, presented to the British Association in 1840, much had been done, many fundamental facts had been established, still Liebig's report marks an era in the progress of this branch of our science. He not only gathered up in a masterly fashion the results of previous workers, but put forward his own original views with a boldness and frequently with a sagacity which gave a vast stimulus and interest to the questions at issue. As a proof of this I may remind you of the attack which he made on, and the complete victory which he gained over, the humus theory. Although Saussure and others had already done much to destroy the basis of this theory, yet the fact remained that vegetable physiologists up to 1840 continued to hold to the opinion that humus, or decayed vegetable matter, was the only source of the carbon of vegetation. Liebig, giving due consideration to the labours of Saussure, came to the conclusion that it was absolutely impossible that the carbon deposited as vegetable tissue over a given area, as for instance over an area of forest land, could be derived from humus, which is itself the result of the decay of vegetable matter. He asserted that the whole of the carbon of vegetation is obtained from the atmospheric carbonic acid, which, though only present in the small relative proportion of 4 parts in 10,000 of air, is contained in such absolutely large quantity that if all the vegetation on the earth's surface were burnt, the proportion of carbonic acid which would thus be thrown into the air would not be sufficient to double the present amount.

That this conclusion of Liebig's is correct needed experimental proof, but such proof could only be given by long-continued and laborious experiment, and this serves to show that chemical research is not now confined to laboratory experiments lasting perhaps a few minutes, but that it has invaded the domain of agriculture as well as of physiology, and reckons the periods of her observations in the field not by minutes, but by years. It is to our English agricultural chemists Lawes and Gilbert that we owe the complete experimental proof required. And it is true that this experiment was a long and tedious one, for it has taken forty-four years to give the definite reply. At Rothamsted a plot was set apart for the growth of wheat. For forty-four successive years that field has grown wheat without addition of any carbonized manure; so that the only possible source from which the plant could obtain the carbon for its growth is the atmospheric carbonic acid. Now, the quantity of carbon which on an average was removed in the form of wheat and straw from a plot manured only with mineral matter was 1000 pounds, whilst on another plot, for which a nitrogenous manure was employed, 1500 pounds more carbon was annually removed; or 2500 pounds of carbon are removed by this crop annually without the addition of any carbonaceous manure. So that Liebig's prevision has received a complete experimental verification.

May I without wearying you with experimental details refer for a moment to Liebig's views as to the assimilation of nitrogen by plants—a much more complicated and difficult question than the one we have just considered—and compare these with the most modern results of agricultural chemistry? We find that in this case his views have not been substantiated. He imagined that the whole of the nitrogen required by the plant was derived from atmospheric ammonia; whereas Lawes and Gilbert have shown by experiments of a similar nature to those just described, and extending over a nearly equal length of time, that this source is wholly insufficient to account for the nitrogen removed in the crop, and have come to the conclusion that the nitrogen must have been obtained either from a store of nitrogenous material in the soil or by absorption of free nitrogen from the air. These two apparently contradictory alternatives may perhaps be reconciled by the recent observations of Warington and of Berthelot, which have thrown light upon the changes which the so-called nitrogenous capital of the soil undergoes, as well as upon its chemical nature, for the latter has shown that under certain conditions the soil has the power of absorbing the nitrogen of the air, forming compounds which can subsequently be assimilated by the plant.

Touching us as human beings even still more closely than the foregoing, is the influence which chemistry has exerted on the science of pathology, and in no direction has greater progress been made than in the study of micro-organisms in relation to health and disease. In the complicated chemical changes to which we give the names of fermentation and putrefaction, the views of Liebig, according to which these phenomena are of a purely chemical character, have given way under the searching investigations of Pasteur, who established the fundamental principle that these processes are inseparably connected with the life of certain low forms of organisms. Thus was founded the science of bacteriology, which in Lister's hands has yielded such splendid results in the treatment of surgical cases; and in those of Klebs, Koch, William Roberts, and others, has been the means of detecting the cause of many diseases both in man and animals; the latest and not the least important of which is the remarkable series of successful researches by Pasteur into the nature and mode of cure of that most dreadful of maladies, hydrophobia. And here I may be allowed to refer with satisfaction to the results of the labours on this subject of a Committee the formation of which I had the honour of moving for in the House of Commons. These results confirm in every respect Pasteur's assertions, and prove beyond a doubt that the adoption of his method has prevented the occurrence of hydrophobia in a large proportion of persons

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bitten by rabid animals, who, if they had not been subjected to this treatment would have died of that disease. The value of his discovery is, however, greater than can be estimated by its present utility, for it shows that it may be possible to avert other diseases besides hydrophobia by the adoption of a somewhat similar method of investigation and of treatment. This, though the last, is certainly not the least of the debts which humanity owes to the great French experimentalist. Here it might seem as if we had outstepped the boundaries of chemistry, and have to do with phenomena purely vital. But recent research indicates that this is not the case, and points to the conclusion that the microscopist must again give way to the chemist, and that it is by chemical rather than by biological investigation that the causes of diseases will be discovered, and the power of removing them obtained. For we learn that the symptoms of infective diseases are no more due to the microbes which constitute the infection than alcoholic intoxication is produced by the yeast-cell, but that these symptoms are due to the presence of definite chemical compounds, the result of the life of these microscopic organisms. So it is to the action of these poisonous substances formed during the life of the organism, rather than to that of the organism itself, that the special characteristics of the disease are to be traced; for it has been shown that the disease can be communicated by such poisons in entire absence of living organisms.

If I have thus far dwelt on the progress made in certain branches of pure science it is not because I undervalue the other methods by which the advancement of science is accomplished, viz. that of the application and of the diffusion of a knowledge of Nature, but rather because the British Association has always held, and wisely held, that original investigation lies at the root of all application, so that to foster its growth and encourage its development has for more than fifty years been our chief aim and wish.

Had time permitted I should have wished to have illustrated this dependence of industrial success upon original investigation, and to have pointed out the prodigious strides which chemical industry in this country has made during the fifty years of Her Majesty's reign. As it is I must be content to remind you how much our modern life, both in its artistic and useful aspects, owes to chemistry, and, therefore, how essential a knowledge of the principles of the science is to all who have the industrial progress of the country at heart.

This leads me to refer to what has been accomplished in this country of ours towards the diffusion of scientific knowledge amongst the people during the Victorian era. It is true that the English people do not possess, as yet, that appreciation of the value of science so characteristic of some other nations. Up to very recent years our educational system, handed down to us from the Middle Ages, has systematically ignored science, and we are only just beginning, thanks in a great degree to the prevision of the late Prince Consort, to give it a place, and that but an unimportant one, in our primary and secondary schools or in our Universities. The country is, however, now awakening to the necessity of placing its house in order in this respect, and is beginning to see that if she is to maintain her commercial and industrial supremacy the education of her people from top to bottom must be carried out on new lines. The question as to how this can be most safely and surely accomplished is one of transcendent national importance, and the statesman who solves this educational problem will earn the gratitude of generations yet to come.

In conclusion, may I be allowed to welcome the unprecedentedly large number of foreign men of science who have on this occasion honoured the British Association by their presence, and to express the hope that this meeting may be the commencement of an international scientific organization, the only means nowadays existing, to use the words of one of the most distinguished of our guests, of establishing that fraternity among nations from which politics appear to remove us further and further by absorbing human powers and human work, and directing them to purposes of destruction. It would indeed be well if Great Britain, which has hitherto taken the lead in so many things that are great and good, should now direct her attention to the furthering of international organizations of a scientific nature. A more appropriate occasion than the present meeting could perhaps hardly be found for the inauguration of such a movement.

But whether this hope be realized or not, we all unite in that one great object, the search after truth for its own sake, and we all, therefore, may join in re-echoing the words of Lessing:—"The worth of man lies not in the truth which he possesses, or believes that he possesses, but in the honest endeavour which he puts forth to secure that truth; for not by the possession of truth, but by the search after it are the faculties of man enlarged, and in this alone consists his ever-growing perfection. Possession fosters content, indolence, and pride. If God should hold in His right hand all truth, and in His left hand the ever-active desire to seek truth, though with the condition of perpetual error, I would humbly ask for the contents of the left hand, saying, 'Father, give me this; pure truth is only for Thee.'"

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## SECTION A.

### MATHEMATICAL AND PHYSICAL SCIENCE.

OPENING ADDRESS BY SIR ROBERT S. BALL, LL.D., F.R.S., PRESIDENT OF THE SECTION.

### A Dynamical Parable.

THE subject I have chosen for my address to you to-day has been to me a favourite topic of meditation for many years. It is that part of the science of theoretical mechanics which is usually known as the "Theory of Screws."

A good deal has been already written on this theory, but I may say with some confidence that the aspect in which I shall invite you now to look at it is a novel one. I propose to give an account of the proceedings of a committee appointed to investigate and experiment upon certain dynamical phenomena. It may appear to you that the experiments I shall describe have not as yet been made, that even the committee itself has not as yet been called together. I have accordingly ventured to call this address "A Dynamical Parable."

There was once a rigid body which lay peacefully at rest. A committee of natural philosophers was appointed to make an experimental and rational inquiry into the dynamics of that body. The committee received special instructions. They were to find out why the body remained at rest, notwithstanding that certain forces were in action. They were to apply impulsive forces and observe how the body would begin to move. They were also to investigate the small oscillations. These being settled, they were then to—— But here the chairman interposed; he considered that for the present, at least, there was sufficient work in prospect. He pointed out how the questions already proposed just completed a natural group. "Let it suffice for us," he said, "to experiment upon the dynamics of this body so long as it remains in or near to the position it now occupies. We may leave to some more ambitious committee the task of following the body in all conceivable gyrations through the universe."

The committee was judiciously chosen. Mr. Anharmonic undertook the geometry. He was found to be of the utmost value in the more delicate parts of the work, though his colleagues thought him rather prosy at times. He was much aided by his two friends, Mr. One-to-One, who had charge of the homographic department, and Mr. Helix, whose labours will be seen to be of much importance. As a most respectable if rather old-fashioned member, Mr. Cartesian was added to the committee, but his antiquated tactics were quite out-manœuvred by those of Helix and One-to-One. I need only mention two more names. Mr. Commonsense was, of course, present as an *ex-officio* member, and valuable service was even rendered by Mr. Querulous, who objected at first to serve on the committee at all. He said that the inquiry was all nonsense, because everybody knew as much as they wished to know about the dynamics of a rigid body. The subject was as old as the hills, and had all been settled long ago. He was persuaded, however, to look in occasionally. It will appear that a remarkable result of the labours of the committee was the conversion of Mr. Querulous himself.

The committee assembled in the presence of the rigid body to commence their memorable labours. There was the body at rest, a huge amorphous mass, with no regularity in its shape—no uniformity in its texture. But what chiefly alarmed the committee was the bewildering nature of the constraints by which the movements of the body were hampered. They had been accustomed to nice mechanical problems, in which a smooth body lay on a smooth table, or a wheel rotated on an axle, or a body rotated around a point. In all these cases the constraints

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