which are now more appropriately called *molecular mixtures*. The same fundamental cause further gives rise to the phenomena of cohesion, adhesion, and capillary attraction, and it seems therefore as if the supposition of special molecular forces is in no way necessary any longer.

Now as the attraction of atoms depends on their *quality*, it is also clear that the molecular attraction caused by such atomic attraction must, under favourable conditions, produce an *orientation* of all molecules combining with one another, and must thus lead to bodies of a regular molecular structure, therefore to *crystals*.

Lastly, the question whether the properties of atoms are dependent on their *weight* has much occupied the chemists of modern times. Positive results which could be rendered clear in a few words have not yet been obtained, but it seems, according to the observations made by Lothar Meyer and Mendelejeff, as if not only the *chemical* properties and specially the chemical quantivalence of atoms and the intensity of their mutual combination, but also the *physical* properties, which at present are still treated as *constants* for materially different objects, were a function and indeed a *periodic function* of the atomic weight. The mathematical form of this function is no doubt of a peculiar nature, but one thing seems certain, viz., *that the numerical value of the atomic weight is the variable by which the substantial nature and all properties dependent on this are determined.*

Thus there again seems hope that it will be possible to reduce *all* properties of matter, including gravity, to *one* and the same force.

The right of introducing all such speculations into the domain of exact science, has been questioned very much. It is generally conceded, indeed, that the setting-up of hypotheses on the domain accessible to exact investigation, as a method of investigation, is useful, inasmuch as it often may accelerate the progress of exact knowledge. But it is at the same time often believed that speculations beyond a certain limit are not admissible. The scientific value of all atomistic considerations particularly has ever, and also in the most recent time, been very much doubted. It has been pretended specially that the supposition of atoms did not explain any properties of bodies which had not first been ascribed to the atoms themselves.

We must own that such remarks contain many truths, but just for that reason it seems necessary that we examine the limit of their correctness.

It is generally acknowledged that the results of exact observation have the value of facts, therefore possess that degree of certainty which human knowledge can attain at all. It is further not contested that to all those laws which, independent of hypotheses on the nature of matter, are deduced from facts, nearly the same certainty must be ascribed as to facts themselves. It is just as incontestable, however, that the human mind in the positive understanding of facts does not find complete satisfaction, and that therefore natural sciences have to follow a yet further and higher aim, *that of the knowledge of the essence of matter and of the original connection of all phenomena*.

But the essence of matter is not accessible to any direct investigation. We can only draw conclusions regarding it from the phenomena which are accessible to our observation. And thus it is evident that there is a certain limit which, moreover, is influenced by the state of our knowledge at any given time, beyond which positive investigation loses ground and where the path is only open for speculation.

If, therefore, the single investigator, following the inclinations of his nature, rests satisfied with positive investigations and renounces all speculations, it is yet clear that to science as such this is not permitted.

By way of hypothesis, based upon what is known as facts, ideas must be formed on the nature of matter; the consequences of these ideas must be developed logically, and, if necessary, by the aid of calculation, and the results of these theories must be compared with the phenomena accessible to observation.

Of course, the *complete* truth will never be reached in this way, or there will, at least, never exist complete certainty that our conceptions are really identical with truth. But that conception which is simplest in itself, and which in the simplest manner accounts for the greatest number of phenomena, and finally for all, will have to be considered not only as the best and most probable one, but we shall have to designate it as relatively, and we may say, humanly, true.

By this the scientific right of existence of speculative investigation is no doubt proved, also for the so-called exact sciences, because beyond a certain limit these indeed cease to be exact.

Simultaneously, however, the scientific value of the present atomic theory is also proved, because it has not been contested that, even in its present and still extremely incomplete form, it accounts satisfactorily for an uncommonly large number of facts, better than any other conception.

It will certainly require further extension, and also a deeper fundamental structure; but at present there is very little probability that it will be completely superseded by essentially different conceptions.

There are other reproaches which have been made to chemistry specially, and still more to chemists, now and since the time of Lord Bacon; and even chemists cannot deny that they were not altogether undeserved.

It has been said that chemistry wilfully makes innumerable single hypotheses which are neither in connection with one another nor with the whole; that the value of hypotheses is over-rated by her disciples, far too great certainty being ascribed even to such as are only little justifiable, and that they are treated as if they had been actually proved; and finally, that her hypotheses are always gradually raised to articles of faith, and that everybody who sins against such dogmas is prosecuted as a heretic.

Recent times have also, in this direction, brought about a considerable improvement. The justification and the value of hypotheses are now recognised in chemistry, but at the same time the true value of hypotheses is also understood by chemists.

In chemistry also, as in all domains of science, blind faith in authorities has been crushed, and by this alone the danger of dogmatising is lessened. And should perhaps any one, who holds antiquated views, try to attach his dogma upon progressing science as a restraint, he will always find the striving young generation, the representatives of the future, ready to remove unjustified impediments. If others, in the fiery zeal of youth, should be inclined to look upon daring flights of fancy as scientific hypotheses and to give them out as such, then those who are more moderate by themselves or by the riper experience of age, will always feel it a duty to step in as regulators.

The school of independent, and at the same time quiet thinkers, is now so numerously represented also among chemists, that a constant development of the science may be confidently expected, and an overgrowth of weeds need no longer be feared. Also in chemistry we are now well aware of the continuity of human mental work; the present generation no longer looks with despising contempt upon the work of their predecessors; far from thinking themselves infallible, they know that at any time it remains to the future to continue the work of preceding generations.

ON THE CAUSES OF THE ASCENT OF SAP IN TREES[1]

THE question as to what forces cause water to rise to such a remarkable height (frequently) in trees has had very various answers given to it. But these have mostly failed to account adequately for the phenomenon.

Capillary action is perhaps the oldest cause adduced. The view was long popular that water rose in trees like oil in a wick, the connected vessels of the wood forming capillary tubes. This view lost force when it was known that the wood of coniferæ was without vessels; and it did not explain the weakening or stoppage of the rise of sap produced by amputation of the roots, nor the presence of air in the columns of sap.

Shortly after Dutrochet's thorough study of diffusion, this phenomenon was called in to account for the rise of sap. One grave objection to such a theory is the rapidity of the ascent of sap (it has been carefully measured) as compared with the slowness of diffusion, which depends simply on molecular motion; another is the inevitable consumption of the osmotic force of tension. So that other problematical forces had to be called in.

When Jamin found that the imbibition of water through fine porous substances (*e.g.* blocks of gypsum) took place with great force, and that the air could thus be compressed to several atmospheres, an effect of this nature was affirmed to occur in living plants, the cell membrane being considered a porous substance. But in fact the natural saturated cell membrane has no air-filled pores, but only pores already filled with water, and even the hollow spaces, bounded by the cell membrane, are partly filled with water; besides, the fact that a branch, immediately after being cut off, loses in great measure the power of raising water, is against this theory.

[1] Abstract of a recent paper in *Der Naturforscher*.

A few years ago yet another theory was started, based on M. Quincke's discovery of the tendency of liquid films to expand rapidly upon wetable surfaces. The only advantage of this lay in accounting for the rapidity of the rise of sap; otherwise it was open to all the objections of the Jamin theory.

A theory has lately been propounded, and thoroughly worked out, by M. Joseph Böhm, which is characterised by good consistency, and offers perhaps a more satisfactory explanation of the phenomenon than any that have been referred to. It is based, like the osmotic theory, on the cellular structure of all sap-conducting plants, and it attributes an important *rôle* to the *elasticity of the cells*. "When the surface-cells of a plant," says M. Böhm, "have lost a portion of their water through evaporation, they are somewhat compressed by the air-pressure. Like elastic bladders, however, they tend to take their original form. This of course is only possible by their drawing in either air or water from without. Since, however, moist membranes are little penetrable by air, the cells draw from cells further in a portion of their liquid contents. These again borrow from their neighbours further down, which contain more water, and so on, either to the extreme root-cells or to those parts of the stem which are supplied with water from below through root-pressure."

To illustrate the action M. Böhm constructed an artificial cell-chain. A funnel closed by a bladder represented the evaporating leaf; to it were connected below several glass tubes about two ctm. wide, closed at one end with a bladder, and joined together in series by means of thick-walled caoutchouc-tubing. In consequence of the evaporation, the membrane which closes the funnel-mouth is bent inwards, and when it has reached a certain tension water is sucked into the funnel out of the next lower cell, which covers its loss in like manner. Manometers, connected with certain cells of the apparatus, indicate the amount of suction at different heights. To avoid fouling of the membranes carbolic acid was mixed with the distilled water in the cells. Since bladder membranes, with a not very great height of liquid column over them, admit passage of water by filtration, these artificial cell-chains (it is pointed out) must act much more imperfectly than the sap-conducting cells placed over one another in living plants, which cells, by reason of their narrow aperture, retain their liquid column by capillary attraction.

It is shown that this theory is in harmony with sundry phenomena which are contradictory of the imbibition theory.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

It will be proposed to confer the degree of D.C.L. *honoris causa* at the ensuing Oxford commemoration, upon Dr. William Spottiswoode, M.A., of Balliol College, F.R.S.

The following awards for proficiency in Natural Science have been made at St. John's College, Cambridge:—Foundation Scholarships to F. J. Allen, Marr, Slater, C. M. Stuart; Exhibitions to Fleming, Hart; the Open Exhibition to C. H. O. Curtis, from the Royal School of Mines.

The plans for the new University edifices at Strasburg have just been completed. They provide for over 100 rooms to serve as auditoriums, museums, the inevitable German singing hall and fencing hall, &c., and will meet the needs of all sections of the university, with the exception of the medical faculty, which retains its old quarters, on account of the propinquity to the hospital. The attendance, which has fallen off during the past year, is now greater than ever before, the number of students for the present semester being 710.

SCIENTIFIC SERIALS

Journal de Physique, April.—In this number M. Vincent recommends chloride of methyl as a frigorific agent, and indicates an abundant source of it. He employs a cylindrical copper vessel having double walls, between which the liquid is admitted through a peculiar cock from an adjoining vessel. In the central part is put an uncongealable liquid such as alcohol. The outer wall is enveloped in cork. On opening the cock the chloride of methyl enters into ebullition, and the temperature of the alcohol bath sinks to $-23°$. By connecting with an air pump and making vacuum, a much lower temperature may be obtained. One pretty experiment with this apparatus is the crystallisation of mercury.—M. Gariel explains the new system of numbering glasses of spectacles, in which a unit called the *dioptrie* is used, this being the power of a convergent lens of 1m. focal distance. The number of dioptries for a particular lens is got by dividing 1m. by the focal distance reckoned in metres and decimal fractions of a metre, since the power varies in inverse ratio of the focal distance. Let N_D be the number of a lens reckoned in dioptries and f_m the focal distance in metres, then $N_D f_m = 1$m., which gives one of the quantities when the other is known.—M. Pellat contributes a mathematical paper on the specific heats of vapours, and the phonograph occupies some attention.

Memorie della Società degli Spettroscopisti Italiani, January, 1878.—Prof. Tacchini contributes a long paper on the appearance and constitution of the sun, based on the photographs of M. Jansen taken at Meudon; there is also another by the same author, giving the observations of the positions in which the magnesium and 1474 lines appeared on the limb of the sun in June, 1877. The appendix contains a paper by L. Gruber on the falling stars of the first part of last November.

February.—Notice of the death of Father Secchi, by the editor.—A paper by Prof. Rosetti on the temperature of the sun; a description of the thermopile and the necessary accessories, together with the results, is given at length.—A table showing the number of spots and protuberances, and the heights of the latter during the first half of the year 1877, and drawings of the chromosphere for the months of November and December made at Rome, by Prof. Tacchini.

March.—A note and table by Prof. Tacchini showing the position on the sun's limb when the magnesium and 1474 lines were seen during June, 1877. Also a summary of the positions of the same during the first half of the year 1877.

SOCIETIES AND ACADEMIES

LONDON

Royal Society.—"Note on the Specific Gravity of the Vapours of the Chlorides of Thallium and Lead," by Henry E. Roscoe, F.R.S., Professor of Chemistry in Owens College, Manchester.

Experimental difficulties of so serious a nature surround the attempt to ascertain the specific gravity of vapours at a high temperature that, in spite of the interest which attaches to this subject, but few additions have been made in our knowledge in this direction since the researches of Deville and Troost.

The present experiments, of which this notice contains the first results, have been made with the object of so simplifying the process as to render it easy to determine the specific gravity of the vapours of bodies possessing high boiling points with a degree of accuracy sufficient for the purpose of controlling their molecular weights.

The method consists in vaporising the substance under examination in long-necked glazed porcelain globes of known capacity placed in a muffle raised to bright redness. The temperature of the globe is ascertained by a calorimetric determination made with heavy platinum weights placed in the muffle, this determination being checked by the simultaneous insertion in the muffle of a second globe containing mercury.

The porcelain globes having a capacity of about 300 cub. centims., and containing from three to nine grams of substance, are closed by loosely-fitting stoppers of baked clay, and then gradually introduced in the muffle. After remaining there until no further escape of vapour is observed, and until the temperature has become constant, the globes are quickly withdrawn from the muffle and their contents removed and analysed, the temperature being in each case ascertained by the calorimetric method at the time of withdrawal of the globe. The following determinations of the specific gravity of mercury vapour serve to show the reliability of the method:—

	Temperature determined calorimetrically.	Specific gravity of mercury vapour.
	°	
Experiment I.	1019	6·92
,, II.	894	6·75
,, III.	815	6·91
,, IV.	972	5·77
,, V.	1047	7·05

the calculated specific gravity (Hg = 198·8) being 6·728.

Before determining the specific gravity of the vapour of thallium chloride it was ascertained that this compound does not