

the steaming plant at two or three centres, from which power is distributed to the mines by electric transmission or in the form of compressed air. This has largely been the work of the Victoria Falls and Transvaal Power Company. Electrically transmitted power is rapidly supplanting independent steam power for mills, winders, sinking engines, underground hoists, pumps, &c., owing to the favourable rate at which it can be purchased from the power company. The price has been fixed by agreement at 0.561 pence per unit until October, 1912, and thereafter at 0.525 pence. Transmission is effected both by overhead lines (at 40,000 volts along the Rand, and at 80,000 volts from Vereeniging, a distance of 30 miles) and by underground cables (at 20,000 volts). The length of the overhead lines is 150 miles, that of the underground cables 35 miles. A portion of the power supplied by the company is in the form of compressed air for rock-drills.

A considerable economy will be effected by this centralisation of power generation, and the consequent reduction in the number of independent steaming plants. From the price per unit at which the Victoria Falls and Transvaal Company are supplying power, the cost of a horse-power per annum, utilised continuously day and night, can be calculated: it works out at 14*l.* It is not so easy to arrive at the average cost of a horse-power year on the mines prior to electrification, but it is stated to have been 28*l.* In any case, the saving due to the substitution of electric motive-power for steam-power is undoubted, and there is, moreover, the indirect advantage of greater flexibility and more perfect control.

The present position of the industry and the progress to be expected in the future may be illustrated by a few statistics.

Since the discovery of the Field in 1886 to the end of 1910 the Rand has milled 155 million tons of ore, and produced gold to the value of 276,000,000*l.*, this being an average of 35.6*s.*, or 8.4 dwt. of fine gold to the ton milled. During the same period dividends amounting to 72,416,550*l.* have been distributed, equivalent to 9.3*s.* per ton milled.

During 1910 gold to the value of close on 31,000,000*l.* was produced by crushing 21,500,000 tons of ore; this is equivalent to an average yield of 28*s.* 6*d.*, or 6.7 dwt. of fine gold per ton milled. The working costs averaged (from the returns of fifty-six companies) was 17*s.* 7*d.* per ton, giving an average profit of 10*s.* 9*d.* per ton milled.

Seven of the largest companies, crushing close on a quarter of the whole tonnage, are working at an average cost of 13.8*s.* This very remarkable result has been brought about by increasing, to their economic limit, the size of the units used in the various operations, such as trucks, stamps, tube-mills, vats, pumps, &c.; by the simplification of the methods of handling ore; and by replacement, so far as it is economy to do so, of hand-labour by mechanical appliances. Larger units of development and the centralisation of power plant have also contributed to this result; while the amalgamation of the properties into larger units has helped to lower working costs, by permitting a reorganisation of the transport and hoisting arrangements, and by reducing the standing charges.

*Future of the Goldfield.*—Working at a cost of 13.8*s.* per ton means that the cost of development, extraction, and reduction, including administration, is covered by a recovery of 3½ dwt. of fine gold per ton. On 5 dwt. ore, therefore, this would allow of a profit of nearly 7*s.* 6*d.* per ton; and over a considerable area of the Rand the average grade of the ore-bodies is not much above 5 dwt. The inclusion of large tonnages of relatively poor reef, which formerly were considered outside the range of practical mining, has been made possible by lower operating costs. The grade of the ore crushed has fallen in consequence. This does not necessarily imply that the increased depth of the mines has (*per se*) caused a falling-off in the actual value of the ore-deposit considered as a whole.

The effect of this increased tonnage and diminished grade on the life of the Rand goldfield as a whole is an interesting subject for speculation. From the data available the production of gold to be expected from the Main Reef series, if worked down to a vertical depth of 6000 feet, may be estimated.

It figures out at 1,046,000,000*l.*, which, on the basis of

an average output of 30,000,000*l.* per annum, is equivalent to a life of thirty-five years, *i.e.* down to a vertical depth of 6,000 feet. But, if at still greater depths the banket should contain sufficient gold to yield a profit, after deducting the cost of working, we may rest assured that it will be worked. What, then, are the limiting factors? They are generally considered to be (1) the mechanical difficulty of raising the ore to the surface from such great depths, and (2) the effect of the temperature gradient. With regard to the mechanical question, the electrical transmission of power applied to stage-winding has so modified the mining engineer's conception of the depth from which deep hoisting is practicable, that it is now generally assumed that there are no mechanical difficulties that cannot be overcome if it pays to do so. As to the temperature question, figures based on Mr. Marriott's careful experiments, which showed that the rise is only 1° F. for every 208 feet of depth, indicate that the rock-temperature at 7000 feet would not exceed 97.5° F., and with efficient ventilation the air temperature would of course be considerably lower. It follows, therefore, that for all practical purposes the whole question turns solely on the gold content, and what that may be at a vertical depth of 7000 or 8000 feet, no one can tell. This much, however, may be said: the geological structure of the country clearly points to the continuance of the conglomerate or banket beds to still greater depths than even 7000 or 8000 feet, before the bottom of the great synclinal basin of the Witwatersrand is reached; and, beyond that point, the beds must still continue until they rise to form the southern lip of the basin known to exist beyond the Vaal River.

#### THE FUNDAMENTAL PROPERTIES OF THE ELEMENTS.<sup>1</sup>

THE mystery that enshrouds the ultimate nature of the physical universe has always stimulated the curiosity of thinking man. Of old, philosophers sought to solve the cosmic problem by abstract reasoning, but to-day we agree that the only hope of penetrating into the closely guarded secret lies in the precise estimation of that which is tangible and visible. Knowledge of the actual behaviour of material and of energy provides the only safe basis for logical inference as to the real essence of things. Faraday was deeply imbued with this conviction; and it is widely recognised as the basis of all modern experimental science. The subject of my lecture to-night concerns the methods and general results of several extended series of investigations, planned with the hope of adding a little to the foundations of human knowledge by means of careful experiment.

At the outset let me remind you of an old saying of Plato's, for it sounds the keynote of the lecture:—"If arithmetic, mensuration, and weighing be taken away from any art, that which remains will not be much."<sup>2</sup> In other words, the soundness of all important conclusions of mankind depends on the definiteness of the data on which they are based.

Lord Kelvin said:—"Accurate and minute measurement seems to the non-scientific imagination a less lofty and dignified work than looking for something new. But nearly all the grandest discoveries of science have been the rewards of accurate measurement and patient, long-continued labour in the minute sifting of numerical results."<sup>3</sup> The more subtle and complicated the conclusions to be drawn, the more exactly quantitative must be the knowledge of the facts.

Measurement is a means, not an end. Through measurement we obtain data full of precise significance, about which to reason; but indiscriminate measurement will lead nowhere. We must choose wisely the quantities to be measured, or else our time may be wasted.

Among all quantities worthy of exact measurement, the properties of the chemical elements are surely some of the most fundamental, because the elements are the vehicles of

<sup>1</sup> Abridged from the Faraday lecture delivered before the Chemical Society by Prof. T. W. Richards on June 14.

<sup>2</sup> Plato, "Philebus" (trans. Jowett), 1875, vol. iv., p. 104.

<sup>3</sup> Sir W. Thomson (Lord Kelvin), address to British Association, August, 1871, "Life," ii., 600.



all the manifold phenomena within the range of our perception.

Weight is clearly one of the most significant of these properties. The eighty or more individual numbers which we call the atomic weights are perhaps the most striking of the physical records nature has given us concerning the earliest stages of the evolution of the universe. They are mute witnesses of the first beginnings of the cosmos out of the chaos, and their significance is one of the first concerns of the chemical philosopher.

Mankind is not yet in a position to predict any single atomic weight with exactness. Therefore the exact determination of atomic weights rests upon precise laboratory work; and in order to arrive at the real values of these fundamental constants, chemical methods must be improved and revised so as to free them from systematic or accidental errors.

What, now, are the most important precautions to be taken in such work? These are worthy of brief notice, because the value of the results inevitably depends upon them. Obvious although they may be, they are often disregarded.

In the first place, each portion of substance to be weighed must be free from the suspicion of containing unheeded impurities; otherwise its weight will mean little. This is an end not easily attained, for liquids often attack their containing vessels and absorb gases, crystals include and occlude solvents, precipitates carry down polluting impurities, dried substances cling to water, and solids, even at high temperatures, often fail to discharge their imprisoned contaminations.

In the next place, after an analysis has once begun, stance into the balance case.<sup>1</sup> Every substance must be collected and find its way in due course to the scale-pan. The trouble here lies in the difficulty in estimating, or even detecting, minute traces of substances remaining in solution, or minute losses by vaporisation at high temperatures.

In brief, "the whole truth and nothing but the truth" is the aim. The chemical side of the question is far more intricate and uncertain than the physical operation of weighing. For this reason it is neither necessary nor advisable to use extraordinarily large amounts of material; from 5 to 20 grams in each experiment is usually enough. The exclamation, "What wonderfully fine scales you must have to weigh atoms," simply indicates ignorance; the real difficulties precede the introduction of the substance into the balance case.<sup>1</sup> Every substance must be assumed to be impure, every reaction must be assumed to be incomplete, every measurement must be assumed to contain error, until proof to the contrary can be obtained. Only by means of the utmost care, applied with ever-watchful judgment, may the unexpected snares which always lurk in complicated processes be detected and rendered powerless for evil.

That the atomic weights may be connected by precise mathematical equations seems highly probable; but although many interesting attempts have been made to solve the problem,<sup>2</sup> the exact nature of such relationships has not yet been discovered. No attempt which takes liberties with the more certain of the observed values is worthy of much respect. It seems to me that the discovery of the ultimate generalisation is not likely to occur until many atomic weights have been determined with the greatest accuracy. No trouble being too great to attain this end, the Harvard work will be continued indefinitely, and attempts will be made to improve its quality, for the discovery of an exact mathematical relationship between atomic weights would afford us an immeasurably precious insight into the ultimate nature of things.

But weight is only one of the fundamental properties of an element. Volume is almost, if not quite, as important in its own way, although far more variable and confusing. All gases, indeed, approach closely to a simple relationship of volumes, defined by the law of Gay Lussac and the rule of Avogadro, and well known to you all. In the liquid and solid state, however, great irregularities are

manifest, and very little system as regards volume is generally recognised.

About twelve years ago, the study of such small irregularities as exist among gases led me to the suspicion of a possible cause for the greater irregularities in liquids and solids.<sup>1</sup> On applying van der Waals's well-known equation to several gases, in some tentative and unpublished computations, it seemed clear that the quantity  $b$  is not really a constant quantity, but is subject to change under the influence of both pressure and temperature. This conclusion has also been reached independently by van der Waals himself.<sup>2</sup> But if the quantity  $b$  (supposed to be dependent upon the space actually occupied by the molecules) is changeable, are not the molecules themselves compressible?<sup>3</sup>

The next step in the train of thought is perhaps equally obvious. If changes in the bulk of molecules are to be inferred even from gases, may not the expansion and contraction of solids and liquids afford a much better clue to the relative expansion and contraction of these molecules?

Most physical chemists refer all changes in volume to changes in the extent of the *empty space* between the molecules. But are there, after all, any such empty spaces in solids and liquids? Solids do not behave as if the atoms were far apart within them; porosity is often conspicuous by its absence. Take, for instance, the case of glass; the careful experiments of Landolt on the conservation of weight<sup>4</sup> show that glass is highly impermeable to oxygen, nitrogen, and water for long periods. Such porosity as occurs in rigid, compact solids usually permits the passage only of substances which enter into the chemical structure of the solids themselves. Thus nitrogen cannot free itself from imprisonment within hot cupric oxide, although oxygen can escape;<sup>5</sup> again, water cannot evaporate into even the driest of atmospheres from accidental incarceration in crystals lacking water of crystallisation.<sup>6</sup> Palladium, on occluding hydrogen, is obliged to expand its bulk in order to make room for even this small addition to its substance. The behaviour of platinum, nickel, and iron is probably analogous, although less marked.<sup>7</sup> Fused quartz, impermeable when cold, allows of the passage of helium and hydrogen at high temperatures;<sup>8</sup> but most other gases seem to be refused admission, and very many solid substances appear to act as effective barriers to the passage of even hydrogen and helium, especially when cold. In these cases, as in so many others, the so-called "sphere of influence" of the atom is the actual boundary by which we know the atom and measure its behaviour.<sup>9</sup> Why not call this the actual bulk of the atom?

From another point of view, the ordinary conception of a solid has always seemed to me little short of an absurdity. A gas may very properly be imagined with moving particles far apart; but what could give the rigidity of steel to such an unstable structure? The most reasonable conclusion, from all the evidence taken together, seems to be that the interstices between atoms in solids and liquids must usually be small even in proportion to the size of the atoms themselves, if, indeed, there are any interstices at all.

Very direct and convincing evidence of another sort is at hand. The idea that atoms may be compressible

<sup>1</sup> Richards, "The Significance of Changing Atomic Volume," Proc. Amer. Acad., 1901, xxxvii., 1; 1902, xxxvii., 300; 1902, xxxviii., 293; 1904, xxxix., 581; *Zeitsch. physikal. Chem.*, 1902, xl., 169, 597; 1903, xlii., 129; 1904, xlix., 15.

<sup>2</sup> Van der Waals, *Zeitsch. physikal. Chem.*, 1903, xxviii., 257. His earlier publication on this topic (Proc. R. Akad. Wetensch. Amsterdam, 1895, xxix., 138) was unknown to me at that time. See also Lewis, Proc. Amer. Acad., 1899, xxxv., 21.

<sup>3</sup> Van der Waals speaks cautiously, but with some conviction, as to the probable compressibility of the molecules on p. 283 of the paper cited above.

<sup>4</sup> H. Landolt, "Über die Erhaltung der Masse bei Chem. Umwandlungen," *Abhandlung der königl. preuss. Akad. der Wissenschaften*, 1910.

<sup>5</sup> Richards, *Zeitsch. anorg. Chem.*, 1892, i., 196; Proc. Amer. Acad., 1893, xxviii., 200.

<sup>6</sup> Baker and Adlam, *Trans.*, 1911, xcix., 507.

<sup>7</sup> Richards and Behr, *Publ. Carnegie Inst.*, 1906, lxi.

<sup>8</sup> Jacques and Perrot, *Compt. rend.*, 1907, cxliv., 135.

<sup>9</sup> Since these ideas were first advanced, Parlow and Pepe have brought forward much interesting evidence concerning the significance of the volumes of solids and liquids, which supports the idea that the atoms are closely in contact with one another (*Trans.*, 1906, lxxxix., 1675; 1907, xci., 1750; 1908, xciii., 1528; 1910, xcvi., 2308).

<sup>1</sup> Richards, "Methods Used in Precise Chemical Investigation," published by the Carnegie Inst. of Washington, 1910, No. 125, p. 97.

<sup>2</sup> See especially Rydberg, *Zeitsch. anorg. Chem.*, 1897, xiv., 66.



receives striking confirmation from a recent interesting investigation of Grüneisen<sup>1</sup> concerning the small effect of low temperatures on the compressibility of metals. The average compressibility of aluminium, iron, copper, silver, and platinum falls off only 7 per cent. between the temperature of the room and that of liquid air. Extrapolation of the curves indicates that at the absolute zero very little further diminution should occur. So far as we can guess, therefore, the hard metals are almost as compressible at the absolute zero as at room temperatures. But at the absolute zero all heat-vibration is supposed to stop; hence this remaining compressibility must needs be ascribed to the atoms themselves.

If the atoms are compressible, all mathematical reasoning which assumes them to be incompressible rests upon a false basis. The kinetic theory of *gases* remains unmolested by these considerations, except as they indicate the changeability of *b* in the equation of van der Waals, but the new views affect seriously the application of this equation to solids and liquids.

Let us proceed to trace a few of the outcomes of our hypothesis. If atoms may really be packed closely together, the volumes of solids and liquids should afford valuable knowledge concerning the relative spaces occupied by the atoms themselves under varying conditions. The densities of solids and liquids then assume a significance far more interesting to the chemical philosopher than before, because they have a more definite connection with the fundamental nature of things.

An apparent objection at once suggests itself; if the particles in condensed material are really touching one another, how can we account for heat within the material? Would such closely packed atoms be able to vibrate?

The theory of compressible atoms supplies as one of its own corollaries the immediate answer to this question. If atoms are compressible throughout their whole substance, they may contract and expand, or vibrate within themselves, even when their surfaces are prevented from moving by being closely packed together. It is thus possible to conceive of a vibrational effect, even in contiguous atoms, provided we can conceive of these atoms as being elastic throughout all their substance. Agitation sufficient to produce even the Brownian movement might easily exist in such a system.

Clearly there is nothing impossible or obviously contradictory to experimental knowledge in the notion that atoms are compressible; indeed, the old idea of small, hard particles far apart is really more arbitrary and hypothetical than the new conception. The obvious simplicity of the latter is rather in its favour than otherwise, as in Dalton's atomic theory. In general, the more simply a hypothesis interprets the phenomena of nature, the more useful the hypothesis is likely to be, provided, of course, that the interpretation is adequate. The modern philosophy of pragmatism is a good guide in such matters; a theory not obviously illogical should be judged by its usefulness. Let us, then, test the new hypothesis by applying it to other aspects of physical chemistry.

If pressure produces a change in the sizes of the atoms and molecules themselves, may not the actual volumes of liquids and solids be used as a guide to the unknown internal pressures within them? Cannot we thus discover whether or not chemical affinity exerts pressure in its action? To follow this clue, the simplest possible case was chosen at first, namely, the comparison of the contractions taking place on combining several elements in succession with a single very compressible one. The changes of volume occurring during the formation of oxides were first computed; later, chlorides and bromides were studied. According to the theory of compressible atoms, we should expect to find greater contraction in cases of greater affinity. A diagram depicting typical data concerning certain nearly related chlorides strongly supports this inference.<sup>2</sup> One line shows the total change of volume which occurs when a grain-molecule of chlorine combines with the equivalent weight of metal; the other

gives the heat evolved during combination. The lines show distinct parallelism; that is to say, reactions evolving much heat manifest great contraction. In cases of this kind, the heat of reaction is usually not very different from the change of free energy; therefore we may infer that greater *affinity* is associated with greater contraction; and it is but a small leap in the dark to guess that the change of volume is *caused* by the pressure of affinity. Since chemical affinity holds two elements firmly together, why should it not exert pressure? And if it exerts pressure, why should not the volume of the system be diminished by this pressure?

Evidently the change of volume in any case must depend not only on the intensity of the pressure exerted by the affinity, but also, among other things, on the compressibility of the substances concerned. The greater the compressibility, the greater should be the change of volume caused by a given pressure of affinity. Before any definite conclusion can be drawn, the differences in compressibility must be taken into account.

These thoughts led to the measuring of the compressibilities of a large number of elements and simple compounds. The previously employed methods for solids and liquids being unsatisfactory, a new and highly satisfactory method was devised for the work done at Harvard. The compressibilities of thirty-five elements and many single compounds were studied by this method with sufficient care to leave no doubt as to their relative values. It became at once manifest that the formation of a compound of a compressible element was attended with greater decrease of volume than the formation of a similar compound of a less compressible element, other things being equal.<sup>1</sup> This is just what the theory leads us to expect, and is a fact inexplicable by any other hypothesis as yet known to me.

Another essential aspect of the theory of compressible atoms is that which concerns cohesion.<sup>3</sup> If the pressure of chemical affinity causes atomic compression, may not the pressure of cohesive affinity also have the same effect? Traube suggested this possibility, but looked at the whole question from a different point of view.<sup>3</sup> The affinity which prevents solids and liquids from vaporising is generally admitted to produce great internal pressure; must it not tend to compress the molecules into smaller space? Molecules with high cohesive affinity (those of substances hard to volatilise) should be much compressed and possess small volume, whereas molecules with a slight cohesive affinity should be more bulky. Moreover, those molecules already much compressed by their own self-affinity would naturally be but little affected by additional pressure. Thus, as regards two substances otherwise similar, the less volatile one would be less compressible, denser, and possess greater surface tension.<sup>4</sup> These outcomes of the theory correspond with the facts in a majority of cases thus far studied; for example, *o*-xylene is denser, less volatile, less compressible, and possesses a greater surface tension than either *m*-xylene or *p*-xylene. Differences of structure and differences of chemical nature sometimes conceal these relations; the parallelism appears most strikingly among isomeric compounds. In brief, the bulk of evidence strongly indicates that cohesiveness as well as chemical affinity exerts pressure in its action, and hence that each plays a part in determining the volumes occupied by molecules.

Thus the computation of the space occupied by either a solid or a liquid becomes a very complex matter. Not only must the various chemical affinities at work be taken into account, but also the cohesive attraction of both factors and products, and the compressibilities over a very wide range of all the substances concerned. Discoverable parallelism in volume changes is to be expected only when one alone of these forces is the chief variable.

The exact mathematical working out of the consequences

<sup>1</sup> Richards, Proc. Amer. Acad., 1904, xxxix., 581.

<sup>2</sup> *Ibid.*

<sup>3</sup> See especially Traube, *Ann. Physik.*, 1897, [iii], lxi., 383; 1901, [iv], v., 548; 1902, viii., 267; 1902, xxii., 519; *Zeitsch. physikal. Chem.*, 1910, lxxviii., 289; also Walden, *Zeitsch. physikal. Chem.*, 1909, lxxvi., 385. Their interpretation depends largely on the application of van der Waals's equation and the complicating assumption of a *co-volume*; however, Walden's very recent paper presents a number of interesting and important relations concerning internal pressure, which seem to demand the assumption of atomic compressibility for their explanation.

<sup>4</sup> Richards and Mathews, *Zeitsch. physikal. Chem.*, 1908, lxi., 449.

<sup>1</sup> E. Grüneisen, *Ann. Physik.*, 1910, [iv], xxxiii., 1239. The relative values for the compressibilities recorded in this investigation are doubtless trustworthy, although the absolute magnitudes are somewhat uncertain because they depend on the rather inadequate theory of elasticity.

<sup>2</sup> Richards, Proc. Amer. Acad., 1902, xxxvii., 399; also especially J. Amer. Chem. Soc., 1909, xxxi., 188.



is very far in the distance, if, indeed, it can ever be attained. This fact does not, however, militate in the least against the plausibility of the idea. Although mankind has not yet been able to devise a method of mathematical analysis which will solve at one stroke the gravitational relations of three bodies, nature is not on that account prevented from causing three or more bodies to act on one another with the force of gravity, or astronomers from calculating as nearly as may be the consequences by a process of approximation.

Carried through to its logical conclusion, the idea that atoms are compressible gives one quite a new conception of the molecular mechanics of the universe. The influence of atomic compressibilities may be perceived everywhere, and in most cases each fact seems to fit easily and without constraint into its place in the hypothesis. Even apparent exceptions, such as the abnormal bulk of ice, may be ascribed in a reasonable fashion to superposed effects. A detailed discussion of many applications of the theory is impossible here, but a few may be suggested in order to make clearer its possibilities.

The satisfying of each valence of an atom would cause a depression on the atomic surface, owing to the pressure exerted by the affinity in that spot. The stronger the affinity, the greater should be this distortion. Evidently this conception gives a new picture of the asymmetric carbon atom, which, combined with four other different atoms, would have upon its surface depressions of four unequal magnitudes, and be twisted into an unsymmetrical tetrahedron. The combining atoms would be held on the faces of the tetrahedron thus formed, instead of impossibly perching upon the several peaks. According to this hypothesis, the carbon atom need not be imagined as a tetrahedron in the first place; it would assume the tetrahedral shape when combined with the other four atoms. One can easily imagine that the development of each new valence would change the affinities previously exercised, somewhat as a second depression in the side of a rubber ball will modify a forcibly caused dimple in some other part. Thus a part of the effect which each new atom has on the affinities of the other atoms already present may be explained.

Many other physico-chemical phenomena assume a new aspect when viewed from the point of view of this idea. New notions of the mechanism of the critical phenomena, surface tension, ductility, malleability, tenacity, and coefficient of expansion are gained. The peculiar relations of material and light, such as magnetic rotation, fluorescence, partial absorption, and so forth, may be referred to the modified vibrations of distorted atoms. The deviations from the exact fulfilment of many older generalisations concerning volume (such as the equation of van der Waals already cited, the comparative volumes of aqueous solutions, especially of electrolytically dissociated substances,<sup>1</sup> and the variations in the crystal forms of isomorphous substances) are seen to be a foregone conclusion. Moreover, the theory, although not necessarily dependent on the modern belief that atoms are built up of numbers of much smaller corpuscles, is consistent with that belief; for would not such an entity be compressible?

The more closely the actual data are studied, the more plausible the hypothesis of compressible atoms appears. Ten years' experience with its interpretations leads me to feel that the idea is highly suggestive and helpful in stimulating new search after truth and in correlating and codifying diverse facts. By such fruit are hypotheses justified.

#### UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The following is the text of the speech delivered by Prof. Love in presenting Sir W. T. Thiselton-Dyer for the degree of D.Sc., *honoris causa*, at the Encaenia on June 28:—

Adest nobis orandus Willelmus Turner Thiselton-Dyer, vir magnam in Botanica laudem adeptus, huius Academiae olim alumnus. Qui cum Dublinii, Coriniii, Londonii hanc

<sup>1</sup> Baxter has very recently discussed this matter from the point of view of the theory of compressible atoms (*J. Am. Chem. Soc.*, June, 1911).

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scientiam profiteretur, docendi rationes ita novavit ut florentissimis totius Europae scholis schola Britannica par fieret. Idem postea regalibus hortis Kewensibus praefectus varia negotiorum genera ad Botanicam pertinentia promovit. Testis est India, Cinchonae Peruvianae, in medendo potentissimae, nunc ferax: testes etiam Taprobane insula et Chryse Chersonesus Heveae Brasiliensis cultrices, cuius arboris virtutes omnibus notissimae sunt. Adeo non solum salutis, sed etiam rei familiari civium hic noster sua opera inservivit.

MR. J. H. PRIESTLEY, lecturer in botany at the University of Bristol, has been appointed professor of botany at the University of Leeds.

MR. JOHN BLAKEMAN, head of the mathematical department of the Leicester Technical School, has been appointed as principal of the Northampton Technical School.

AN endowment fund of 200,000*l.* has been presented to University College, Reading, with a view to enable it to apply for a charter as an independent university. Of the amount mentioned, Lady Wantage has given 50,000*l.*, Mr. and Mrs. George W. Palmer 100,000*l.*, and Mr. Alfred Palmer 50,000*l.* Mr. Alfred Palmer has, in addition, presented to the college the freehold of four acres of land contiguous to the college site, at present held and used for horticultural purposes by the college under lease from him.

We learn from *Science* that a gift of 4000*l.* to aid general research in the study of diseases at the Yale Medical School has been announced from an old student. Further gifts of 2000*l.* toward the endowment of the University clinic, and to the Peruvian exploration fund, for the Yale expedition under Prof. Hiram Bingham, have also been announced. From the same source we find that gifts of 20,000*l.* in lands by Messrs. James B. and Benjamin N. Duke, of 10,000*l.* for a new building by Mr. James B. Duke, and of 2000*l.* by Mr. B. N. Duke for improvements, were announced recently at Trinity College, Durham, N.C.

THE General Assembly of the State of Illinois has granted to the University of Illinois for the next two years the sum of 703,860*l.* *Science* says that this is the largest grant ever made by a State legislature to a State educational institution. The General Assembly has not only recognised the immediate needs of the University, but has looked ahead and made provision for the future by levying a one mill tax for the continued support of the University. It is estimated that this tax will yield an income to the University, two years hence, of about 450,000*l.* a year. In addition, the University will receive from the Federal Government and other sources funds that will bring its income to about 400,000*l.* per annum for the next biennium.

#### SOCIETIES AND ACADEMIES.

LONDON.

**Mineralogical Society**, June 13.—Prof. W. J. Lewis, F.R.S., president, in the chair.—G. S. Blake: Zirkelite from Ceylon. The results of five analyses made on fragments grouped together according to their specific gravity, which ranged from 5.2 to 4.4, showed remarkable variation in the percentage composition, the densest containing about 20 per cent. thoria and little uranium, and the lightest 14 per cent. U<sub>3</sub>O<sub>8</sub> and little thorium; the precise formula is uncertain. A few crystals, some simple and some twinned, were met with; they apparently belong to the hexagonal system ( $cr=53^{\circ} 22'$ ), the observed forms being  $c(0001)$ ,  $m(10\bar{1}0)$ ,  $r(10\bar{1}1)$ ,  $s(20\bar{2}1)$ ,  $d(10\bar{1}2)$ ,  $e(20\bar{2}3)$ , and  $r$  the plane of twinning; they were opaque in mass, but translucent and isotropic in splinters.—Rev. Mark Fletcher: Note on some crystals of artificial gypsum. The crystals, which were formed in the condensing plant of a distillery at Burton-on-Trent, were twinned about 101, and the forms 100, 110, 230, 111 were observed.—L. J.