LETTERS TO THE EDITOR.

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The Atomic Weight of "Thorium" Lead.

IN continuation of preliminary work published by Mr. H. Hyman and myself (Trans. Chem. Soc., 1914, cv., 1402) I gave an account in NATURE, February 4, 1915, p. 615, of the preparation of 80 grams of lead from Ceylon thorite and of the determination of its density in comparison with that of ordinary lead, which proved the thorite lead to be 0·26 per cent. denser. Taking the new international figure, 207·20, for the atomic weight of common lead, that for the thorite lead would be 207·74 on the assumption that the atomic volume of isotopic elements is constant.

This lead and the comparison sample were each distilled in three fractions, and the atomic weights of the two middle fractions were determined from the ratio, Pb : $PbCl_2$, by converting the metal into chloride, *via* nitrate, in a quartz vessel. Only single determinations were done, which gave the values 207·694 and 207·199 respectively, which are in the ratio of 100·24 to 100.

This result, which indicated clearly that the atomic volume of isotopic elements is constant, was communicated in a lecture to the Royal Institution, May 15, 1915, and to Section A of the British Association at Birmingham, 1915.

Since then Prof. T. W. Richards and Mr. Wadsworth at Harvard have shown that the density of lead derived from uranium minerals is less than that of common lead, but, as in the case of thorium lead, the atomic volume is constant. Varieties of lead of atomic weight from 206·08 to 207·18 varied in density from 11·273 to 11·337. The latter values refer to common lead.

Dr. R. W. Lawson, at present interned in Vienna but allowed full liberty to continue his investigations at the Radium Institut under Prof. Stefan Meyer, communicated to me in July last year the desire of the investigators in that institute to examine independently the atomic weight of some of my thorite lead, and I accordingly sent him the first fraction of the distilled lead, weighing some 12 grams. He has now written to me, and it is a pleasant duty first to mention that he speaks in the warmest terms of the utmost kindness and consideration shown him by the staff of the institute and of the courtesy and consideration of the police and other authorities during his internment. He reports that Prof. Hönigschmid has made four determinations of the atomic weight of my lead, according to the method of gravimetric titration and the relation of chloride to silver, and four by gravimetric analysis, whereby the weights of the chloride and silver chloride were determined. The complete mean of the eight results was $207{\cdot}77 \pm 0{\cdot}014$, which is in excellent agreement with my own figure, 207·74, found indirectly from the density, and shows that my single atomic-weight determination, 207·694, was not seriously in error.

It is especially gratifying to have the conclusion that the atomic weight of thorium lead is higher than that of common lead confirmed by an investigator of the training and experience of Prof. Hönigschmid. For, although the converse proposition that the atomic weight of lead from uranium minerals is lower than that of ordinary lead has, since the publication of the first paper by Mr. Hyman and myself on thorite lead, been thoroughly and conclusively established by the work of many investigators at Harvard, in Vienna, and in Paris, several of them famous for their atomic-weight determinations, doubt has lingered with regard to our results for the very much more difficult case of thorium lead. In the first place, no one but myself has been able to obtain a suitable material by which to test the question, and I, of course, can claim no previous experience of atomic-weight work. In the second place, there has been an unfortunate confusion between my material, Ceylon thorite, and thorianite, a totally distinct mixed thorium and uranium Ceylon mineral. Lastly, there has been the widespread view, due to Holmes and Lawson, Fajans, and others, mainly derived from geological evidence, that thorium-E, the isotope of lead resulting from the ultimate change of thorium, was not sufficiently stable to accumulate over geological periods of time. This confirmation from Vienna thus clears up many controversial matters, and we now know of varieties of lead differing from 206·08 to 207·77 in atomic weight, and from 11·273 to 11·376 in density, the atomic volume in all cases examined being constant.

According to analyses by Miss A. F. R. Hitchins and myself, the 20 kilos of selected thorite worked upon contained 0·4 per cent. of lead, 57 per cent. of thorium, 1·03 per cent. of uranium, and 0·5 c.c. of helium per gram. Taking the ratio of the period of thorium to that of uranium as 3·2, and assuming that *the whole of the lead is of radioactive origin and is stable,* 94·5 per cent. is derived from thorium, and 5·5 per cent. from uranium. If 206·0 is the true atomic weight of uranium lead, Prof. Hönigschmid's value, 207·77, for thorite lead gives the figure 207·87 for the atomic weight of thorium lead, whilst his figure, 232·12, for the atomic weight of thorium gives a total loss of 0·25 unit of mass in the six α- and four β-ray changes suffered by the thorium atom. From these data and from Silberstein's and his own theories of mutual electromagnetic mass, perhaps Prof. Nicholson may be able to give us further information as to the constitution of the nucleus of the thorium atom.

FREDERICK SODDY.

Marischal College, Aberdeen, February 1.

The Bursting of Bubbles.

PRESUMABLY all bubbles when they burst on the surface of a liquid commence to do so at the top and thus give rise to gaseous vortices. In the ordinary way these are not apparent, but recently we have accidentally discovered a neat way of making them easily visible. The method consists in creating the bubbles by sparking with a Ruhmkorff coil between two wires beneath the surface of some resin oil and thus gasifying the latter. As the bubbles are full of smoke, when they burst the vortex effect is clearly indicated by the formation of beautiful little smoke rings. The size of the bubbles and of the rings depends upon the viscosity of the oil, greater viscosity causing bubbles and rings to become larger. This can easily be shown by cooling or warming the oil.

A. A. CAMPBELL SWINTON.
EVELYN BEALE.

66 Victoria Street, London, S.W.
February 6.

A Plea for a Scientific Quadruple Entente.

THE letter of Prof. Eugenio Rignano in NATURE of January 25 may have recalled to some a discussion on literature which took place in Section D at the British Association meeting at Manchester in 1915, a discussion which was introduced almost precisely from the same point of view as that now given by Prof. Rignano. It is the fact that Germany, by welcoming and publishing papers in French, Italian, and English, as well as in German, and by printing the material practically as fast as it arrived, had gained an ascendancy and even an actual or presumptive international position with regard to scientific publication. For example, the *Internationale Revue der gesammten Hydrobiologie und Hydrographie* is published in Leipzig, and it must be confessed that it is conducted with considerable ability and adequately fulfils its function. Such works, too, as the "Nordisches Plankton" show how keen our present enemies have been in publishing monographs which have become indispensable. It is not necessary, however, to look for sinister intentions in the progress which Germany has made in publication. In all countries scientific literature has had a similar history, and its evolution may be said to mark the progress made by each with respect to science. In all countries, societies, museums, and laboratories have sought an outlet for their investigations by publications which have the primary advantage of securing by exchange similar publications from institutions at home and abroad.

The result is somewhat chaotic, and for this reason we are thankful to the societies and publications and agencies which attempt to give us periodical lists of literature and summaries of papers. I confess I do not see how the national output of scientific papers is to be controlled, or, indeed, if it is desirable that it should be controlled. This is a matter which it appears must be left to the editor and the writer. But now that our attention has been directed to the subject, the opportunity should be taken to discuss whether we should be content with pre-war conditions, or if improvements could be suggested so far as the nation is concerned, and especially with regard to the control of such publications as are meant to be international in character. We have to recognise that each country must necessarily have an output of material for publication for which provision must be made. The fact that so many English papers have been published in Germany raises the question whether the provision is adequate. It might be suggested that each country should publish its own material, and that the desirability of publishing year-books bringing together summaries of the important papers and discoveries in each subject should be considered. If this were done by each country, probably the necessity for international journals would disappear, and better so, for it would be difficult to determine in each case which country and which language to choose.

An improvement of great value would result if a suggestion which has been made before were carried into effect, viz. that a size of page for octavo and for quarto periodicals should be decided upon. If this were agreed to by each country, and the effort made to issue the more monographic papers separately, it would be possible to bind in any way suitable to the institution or worker.

A. MEEK.

Armstrong College, Newcastle-upon-Tyne,
January 27.

Science in Education.

ON p. 432 of your issue for February 1 it is stated in an unsigned article that "in the early fifties of the nineteenth century a little experimental science crept in almost shamefacedly, introduced by the peripatetic teacher with his box of tricks." Then after mentioning Queenwood in 1847 it goes on: "But it was not until twenty years later that this example was followed in other schools. Then Clifton took the lead in 1867, and was followed immediately by the Manchester Grammar School."

Your correspondent is not very well informed.

At Rugby in 1849 William Sharp, F.R.S., was appointed by Dr. Tait reader in natural philosophy, and gave systematic teaching to classes of boys. He was succeeded by Henry Highton, a distinguished electrician (see Arago's "Tonnerre"), who taught chemistry and physics to about forty boys. He became headmaster of Cheltenham College in 1859, and Dr. Temple appointed me to succeed him. A chemical laboratory was built, and I taught physics, chemistry, and a little geology to somewhat larger classes. Then in 1865, after the Public Schools Commission, a great increase in science teaching took place; the Rev. T. N. Hutchinson was appointed to take chemistry, and Mr. Kitchener took botany with large classes. I went on with physics and geology. All this happened before 1867.

Clifton College is an excellent school, but it need not be exalted at the expense of its mother.

JAMES M. WILSON,
Sometime mathematics and science master at Rugby, and afterwards headmaster of Clifton College.

OBVIOUSLY the article referred to did not profess to give a complete history of the introduction of science teaching into schools. That would be a long story, and would necessitate reference to several schools besides Queenwood, such as the Friends' School at Ackworth, in Yorkshire, where for many years some teaching of science subjects had been established long before Rugby and other public schools had made a beginning. The claim for Clifton is based on the fact that it is believed to have been the first great school in which natural science was introduced as a constituent of the curriculum imposed on *the whole school* (except the Classical VI.), and not as a voluntary subject taken up by a small number of boys as an alternative to modern languages or other subjects, as was the case at Rugby in 1859.

THE WRITER OF THE ARTICLE.

"Frost Thistles."

IN NATURE of January 11 Dr. R. T. Gunther describes a very beautiful freezing effect. I recently obtained a similar effect on a much larger scale. One of the large bottles, holding several litres, used for collecting rainfall in the London parks, in connection with the investigation of atmospheric pollution, was found, when brought into the laboratory for examination, to contain a hollow cylinder or annulus of ice, perforated with silvery air-tubes arranged, as Dr. Gunther remarks, for all the world like lines of force round a magnetic pole. We were unable to detect any visible specks of solid matter at the peripheral ends of the tubes; but these ends were pointed, whereas the inner ends were rounded and expanded, probably terminating thus where ice and the central core of water met. The surface of this central portion was covered with a scum of air-bubbles. One feature which was noticed particularly was that, in addition to the air-tubes, other channels filled with something (probably liquid water containing dissolved matter) were arranged radially throughout the mass.

The water represented the accumulated rainfall of January, and, in addition to matters in solution, contained an appreciable deposit. It is, therefore, quite probable that small particles were present on the sides, and thus formed collecting points for the air first liberated when congelation began.

The various points observed agreed entirely with those already described, except that the very much larger quantity of water had not solidified throughout its mass. If this had occurred it would have probably been a slow action and the opaque central mass would not have been seen.

J. H. COSTE.

Teddington, February.

Note.—Since writing the above I have seen another mass of ice solid throughout, which had broken the bottle; a small opaque central cylinder, or inverted cone, was observed.—J. H. C.