

Letters to the Editor.

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The Atomic Weight of Silver.

IN reply to Prof. Brauner's criticism (NATURE, Mar. 5, p. 348, and April 9, p. 526) of our determination of the atomic weight of silver (*J.C.S.*, 1926, 128, 2510), we have now investigated the volatility of silver and have most emphatically confirmed our original statement that no silver could possibly have escaped from the silica tube during the final melting in hydrogen.

Some finely divided silver, prepared by heating the oxide in air to 350°-400° C., was introduced into the silica tube shown in the diagram (Fig. 1). The tube was heated in the electric furnace for an hour at 700°-800° C., for a similar period at 800°-900° C., and finally for five hours at a temperature of 1000° C., whilst a slow stream of hydrogen passed through the

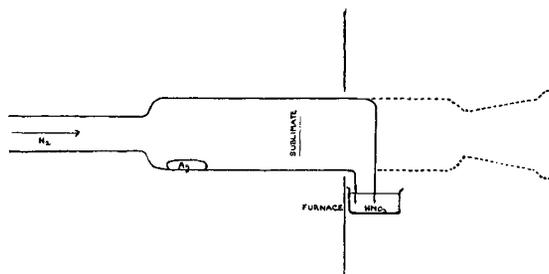


FIG. 1.

tube and bubbled through the dilute nitric acid (HNO₃:2H₂O). The nitric acid was then poured into a nephelometer tube and a little dilute potassium bromide solution added. On examination of the nephelometer no turbidity could be detected, even after standing for several hours. On the addition of 0.2 c.c. of N/1000 silver nitrate (0.02 mgm. silver) an immediate and comparatively dense turbidity developed.

Taking into consideration the relative sizes and positions of the silica tube as used in the atomic weight determination (shown by dotted lines in Fig. 1), and the silica tube used in this experiment, also the time of heating, which was only twenty minutes in the determinations, then it is quite conclusive that no silver could possibly have escaped from the original silica tube.

On examination of the tube after the experiment, an extremely minute sublimate was observed 3 cm. from the silver bead, showing that the silver is slightly volatile, but not to anything like the extent suggested by Prof. Brauner.

Our preliminary investigations on the decomposition of silver oxide showed that it was essential to melt the silver in hydrogen after the initial decomposition. Silver, prepared from silver oxide by heating at 400° C., is a coherent mass full of voids. It could almost be called sintered. The loss in weight on melting in hydrogen was between 2 mgm. and 3 mgm.

In estimating the weight which ought to be attached to any determination of a constant such as an atomic

weight, we would suggest that it is only the experimental facts which deserve consideration. Our determination of the atomic weight of silver has an advantage over others, in that it is the only one in which the direct ratio of silver to oxygen has been measured.

H. B. BAKER.

H. L. RILEY.

Imperial College of Science and Technology,
South Kensington, S.W.7.

Radioactivity and the Heat of the Earth.

IN a previous letter to NATURE (vol. 119, p. 277, 1926) I have discussed Dr. J. W. Evans's suggestion that the existence of pleochroic haloes in mica indicates that much of the energy of the rays from the radio-elements in rocks is used up in producing physical, chemical, or atomic rather than thermal changes in the surrounding minerals. I concluded that the close agreement between the calculated and measured heat productions of the radio-elements affords strong evidence for the view that "practically the whole of the energy associated with the radiations emitted by these elements is available for raising the temperature of the rocks in which they occur." In a later communication (NATURE, vol. 119, p. 424, 1926), Dr. Evans infers that if measurements of the heat production of radium were carried out in a mica container instead of in a glass tube, the results of experiment and theory might not show such good agreement, in spite of the fact that the fluorescence effects and colorations produced by the radiations and the thermo-luminescence resulting from subsequent heating of the coloured materials are quite analogous for the two substances. From the viewpoint of geothermal problems, the importance of an unequivocal decision on the question at issue is perhaps sufficient justification for my returning to the subject, for several independent lines of thought support the conclusion arrived at in my previous letter.

Since it can be obtained in very thin uniform sheets, mica has always been used extensively in absorption experiments with α -rays; its behaviour is perfectly normal. The calculated range of α -particles in mica, as in glass and aluminium, is in good agreement with that found by experiment. This we should not expect if the behaviour of mica as regards absorption were abnormal. In such a case, either each α -particle would lose an appreciable amount of its energy in promoting, say, chemical change and the observed range in mica would be smaller, or the α -particles would lose different amounts of energy in this way, when the range would be indefinite, in opposition to observations of pleochroic haloes where the range is sharply defined. Furthermore, the proportion of the energy of α -rays utilised in promoting atomic changes in mica must also be very small, for mica has been used extensively in absorption experiments on atomic disruption, and the number of H-rays observed, particularly when the mica has been outgassed, is insignificant for present purposes.

Dr. Evans mentions that Dr. Alfred Brammell found it necessary to keep biotite at a dull-red heat for about six hours to render the pleochroic haloes invisible; but here I think he has misunderstood my previous reference to the thermo-luminescent effects with minerals coloured by radioactive radiations. When coloured glass or minerals are heated appreciably, but below dull-red heat, the thermo-luminescent effect is of relatively short duration, and may often be designated a flash. My statement that the energy liberated during the process is "manifestly small"