

transmutation, however, could have been received with enthusiasm. The two contraries (*Yin* and *Yang*) might have been identified with the opposing Greek 'elements' of *water* and *fire* and hence with the two 'qualities' of *mercury* and *sulphur*. No understanding of *Taoism* would have been necessary. The fact that transmutation was alleged to have been accomplished by changing lower qualities or natures into higher ones might have been sufficient to arouse interest, particularly in the mind of the Egyptian artisan who was daily confronted in his workshop with curious and striking changes in the appearance (notably colour) of metals under suitable treatment. Consequently, may not the alleged fact of the practical transmutation of the metals in China as related by traders in Alexandria have been the inspiration for that marriage of Greek philosophy and Egyptian craftsmanship which Prof. Hopkins believes gave birth to Western alchemy?

The purpose of this note is to indicate that the apparent completeness of Prof. Hopkins's picture does not automatically preclude the possibility that reports of Chinese alchemy may have had some influence on the origin of the Egyptian art. Whether such an influence is considered to be 'improbable' or 'probable', present data still appear to indicate that it is 'possible'.

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¹ Hopkins, A. J., "Alchemy: Child of Greek Philosophy", Columbia Univ. Press, New York; 1934; *Isis*, 7, 58-76; 1925.
² Barnes, W. H., *J. Chem. Ed.*, 11, 655-658; 1934. Davis, T. L., *J. Chem. Ed.*, 11, 517-620, 635; 1934; 12, 1, 51; 1935. Davis, T. L. and Wu, L. C., *Sci. Monthly*, 31, 225-235; 1930; *J. Chem. Ed.*, 9, 859-862; 1932; *Isis*, 18, 210-289; 1932. Edkins, J., *Trans. China Branch, Roy. Asiatic Soc.*, Hong Kong, pt. 5, 83-89; 1855. Forke, A., *Archiv. Gesch. Philosophie*, 41, 115-126; 1932. Johnson, O. S., "A Study of Chinese Alchemy", Commercial Press, Shanghai; 1928. Martin, W. A. P., "The Lore of Cathay", Chap. iii, "Alchemy in China", pp. 44-71, Fleming H. Revell Co., New York, Chicago, Toronto; 1901. Partington, J. R., *NATURE*, 119, 11; 1927; 120, 158; 1927. Read, B. E., *NATURE*, 120, 877; 1927. Waley, A., *Bull. Sch. Orient. Studies*, 6, 1-24, 1102-3; 1930-32.

The Ratio 136/137 in Atomic Physics

In an earlier letter¹ I suggested that some (or all) of the determinations of the specific electronic charge, e/m , disagreed with the value deduced from Sir Arthur Eddington's $M/m = 1847.6$ theory (namely, 1.7703×10^7 E.M.U.) because they were really measurements of

$$\frac{136}{137} (1.770,3_1 \pm 0.000,1_4) \times 10^7 = \\ (1.757,4 \pm 0.000,14) \times 10^7 \text{ E.M.U.}$$

Prof. Birge² stated that the measurements agreed with my hypothesis even better than I had suggested; and Sir Arthur Eddington³ suggested that, on theoretical grounds, the ratio would be expected to be very nearly 136/137.

The hypothesis can now be tested with precision. The seven most recent determinations⁴ of e/m are:

$$\left\{ \begin{array}{l} 1.757,9 \pm 0.002,5 \\ 1.758,7 \pm 0.000,9 \\ 1.757 \pm 0.001,5 \\ 1.758 \\ 1.757 \pm 0.001 \\ 1.757,0 \pm 0.001,0 \\ 1.757,9 \pm 0.000,3 \end{array} \right. \times 10^7 \text{ E.M.U.}$$

The unweighted mean of these seven:

$$(1.757,6_4 \pm 0.000,2) \times 10^7 \text{ E.M.U.},$$

may be compared with the value given by my hypothesis:

$$(1.757,4 \pm 0.000,14) \times 10^7 \text{ E.M.U.}$$

Apparently the factor 136/137 is involved because we analyse a system, such as an atom, into 'separate parts' which 'interact'. It seems that the effective mass of the less massive portion may have one of two distinct values according to our point of view.

Prof. Birge⁵ has pointed out that the estimate of the electronic charge deduced from Millikan's experiment (4.768×10^{-10} E.S.U.) and the estimate obtained by the crystal-grating X-ray method (4.803×10^{-10}) are almost in the ratio 136/137. I wish to make a rather similar suggestion.

I have formerly⁶ given evidence that Sir Arthur Eddington's theoretical deductions $hc/2\pi e^2 = 137$ and $M/m = 1847.6$ are true. From these (with the help of Faraday's and Rydberg's constants) we can deduce⁷ what I believe to be the most reliable estimate of e , namely:

$$(4.775,9 \pm 0.000,4_3) \times 10^{-10} \text{ E.S.U.}$$

I would suggest that the crystal-grating X-ray estimate of e is really an estimate of

$$\frac{137}{136} (4.775,9 \pm 0.000,4_3) \times 10^{-10} = \\ (4.810,9 \pm 0.000,4_3) \times 10^{-10} \text{ E.S.U.}$$

This value could also be obtained by assuming $hc/2\pi e^2 = 137$, but using $e/m = 1.757_4$ in the Rydberg-Bohr equation.

The most recent determinations of e by the X-ray method⁸ give the values 4.806 ± 0.003 and 4.805 , which are in reasonably good accord with the 4.810 , suggested above. Ruark¹⁰ finds that certain discrepancies disappear if he uses $e = 4.806 \pm 0.003$, $e/m = 1.757,9 \pm 0.0003$, $hc/2\pi e^2 = 137.04$.

If this second hypothesis of mine is correct, it would appear that the discrepancy between the two methods of estimating the electronic charge (or the two methods of estimating the X-ray wave-lengths) is due to the faulty analysis of a 'system' into two 'parts'. I do not know where the error occurs. It may be due to our assuming that 'the mass' contained in each unit cell in a crystal can be deduced by multiplying 'the volume' of the unit cell by the density of the crystal. We naively contemplate the system as split up into parts.

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¹ W. N. Bond, *NATURE*, 133, 327; 1934.

² R. T. Birge, *NATURE*, 133, 648; 1934.

³ A. S. Eddington, *NATURE*, 133, 907; 1934.

⁴ C. D. Shane and F. H. Spedding, *Phys. Rev.*, 47, 33; 1935.

⁵ R. T. Birge, *loc. cit.*

⁶ W. N. Bond, *Proc. Phys. Soc.*, 44, 374; 1932.

⁷ R. T. Birge, *Phys. Rev.*, 40, 319; 1932. (Prof. Birge amended my calculation of e , but owing to a numerical mistake he obtained too small a value for the probable error, namely, $\pm 0.000,048$ in place of $\pm 0.000,4_3$.)

⁸ E. Bäcklin, *NATURE*, 135, 32, Jan. 5, 1935.

⁹ M. Söderman, *NATURE*, 135, 67, Jan. 12, 1935.

¹⁰ A. E. Ruark, *Phys. Rev.*, 47, 316; 1935.