Does the Formula for the Rydberg Constant Require Revision?

Values of h/e with estimated error limits as determined by eight different methods are shown in the accompanying chart.[1] My purposes here are (1) to re-emphasize the inescapability of the discrepancy[2] between results III and VIII and (2) to consider which of the two is more likely to be correct. Recently von Friesen,[3] emphasizing method V, has ignored under generous error limits this discrepancy in an article which, I have learned from conversations and correspondence, gave many physicists a false sense of security.

To assign uncritically such discrepancies to errors of measurement as von Friesen seems to do is to ignore the possibility of obtaining precise information for the revision or refinement of theories and concepts. These, one cannot too often insist, are no more secure than their experimental foundations.

(1) Since the assignment of error limits is so controversial we refer the reader directly to our experimental results,[4] for the "isochromats" determining the threshold voltages of excitation of two different x-ray wave-lengths (continuous spectrum). These wave-lengths refer to the peak of the spectral "window" selected by the two-crystal monochromator, the window shapes being shown to correct voltage scale for both cases. I wish to emphasize the positions of the points W_1 and W_2 where the thresholds would have to be to make this experiment agree with VIII. Even with von Friesen's liberal error limits on e, the positions W_1 and W_2 cannot have uncertainties exceeding ± 12 and ± 6 volts, respectively (smallest divisions of voltage scale, Fig. 14, 10 volts).

The observed isochromat is a resultant of the true shape of the continuous spectrum and the shape of the "window" curve of the monochromator. If the former were an oblique featureless straight line terminating at the axis of abscissae then the integrated effect of its intenser regions shining through the remote "tail" of the "window" might indeed give the apparent shift of the isochromat toward lower voltages. *The humps K_2 on both isochromats make this explanation untenable and furnish the most clinching evidence for the discrepancy.*[5] These can only correspond to similar prominences on the true continuous spectrum and they must appear on the isochromat at that voltage at which such features of the continuous spectrum coincide with the
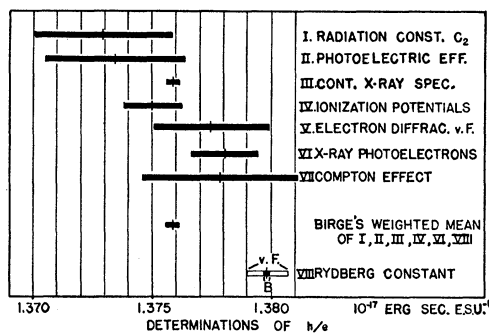

FIG. 1.

peak of the "window" of the monochromator. Both conclusions follow because the window curve is everywhere so smooth save at its peak. The positions of K_2 and W therefore establish the reality of the discrepancy quite objectively.

(2) Five of the six less precise methods plotted above have error limits overlapping III while only two overlap VIII. Any reasonably weighted mean of the six lies far closer to III than VIII. *This suggests that the Rydberg formula may require revision.* Such revision of theory must of course leave the series formula $(1/n^2 - 1/m^2)$ intact. It may be only a coincidence that if a constant multiplier $(1 - \alpha^{-1})$ is joined to the present Rydberg formula the discrepancy vanishes (α being of order 137). Or perhaps the action quantum (like mean free path in kinetic theory) may be at present too vaguely defined and may require different values for different purposes.

JESSE W. M. DuMOND

California Institute of Technology,
Pasadena, California,
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[1] I have to thank Dr. K. Overhage who plotted all of this diagram save point V from Birge's most recent data. I have added V from von Friesen's article, reference 3.
[2] R. T. Birge, Phys. Rev. 52, 241 (1937); DuMond and Bollman, Phys. Rev. 51, 400 (1937).
[3] S. von Friesen, Proc. Roy. Soc. A160, 424 (1937).
[4] Fig. 14, Phys. Rev. 51, 416 (1937).
[5] Humps K_2 were not previously observed for reasons discussed in our paper, reference 2. The substantial agreement of our results with those of others strongly supports the reality of the discrepancy. For this see Kirkpatrick and Ross, Phys. Rev. 51, 529 (1937).

Stability of Neon and Carbon with Respect to α-Particle Disintegration

Kalckar, Oppenheimer and Serber in a recent article[1] have proposed as an explanation for the experimentally observed stability of compound nuclei Ne^{20}, C^{12} against α-decay by small $l-s$ coupling. The smallness of coupling would mean that the energy of coupling is small compared with the separation of energy levels, but it is well known that the distance between energy levels in heavy nuclei is very small and in the above elements is surely much less than the energy of $l-s$ coupling. Therefore such an explanation of the anomalous stability seems untenable.

I should like to point out that there is no need to look for special explanations, because the observed phenomenon can be explained by the fundamental conservation laws which, as is well known in wave mechanics, include also the conservation of parity. As already pointed out by Fermi for atoms it can sometimes occur that a decay which is energetically possible cannot take place because of this conservation law. Let us assume for instance that the state in question is an odd state with angular momentum zero and the states of the disintegration particles are even states with momenta also zero. Then it is easy to see that no states of relative motion can satisfy both the law of conservation of angular momentum and parity.

L. LANDAU

Moscow Institute for Physical Problems,
Academie of Sciences of USSR,
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[1] Phys. Rev. 52, 279 (1937).