LETTERS TO THE EDITOR

Prompt publication of brief reports of important discoveries in physics may be secured by addressing them to this department. Closing dates for this department are, for the first issue of the month, the eighteenth of the preceding month, for the second issue, the third of the month. Because of the late closing dates for the section no proof can be shown to authors. The Board of Editors does not hold itself responsible for the opinions expressed by the correspondents.

Communications should not in general exceed 600 words in length.

On the Values of Fundamental Atomic Constants

A recent article by von Friesen,[1] with the above title, seems to call for certain comments. In the first place von Friesen writes his adopted values in the form $x \pm t$, where $2t$ indicates "the narrowest region within which one is sure to find the true value." He also argues that the concept of probable error can be applied only to accidental errors.

In my article on the general physical constants,[2] I gave reasons why it is preferable to publish the value of a physical constant with its estimated probable error, rather than with an estimated "limit of error." The situation is essentially as follows. When a person states that he is "sure" that the value of a physical constant lies within a certain region, he does not really mean this. One can never be absolutely sure. What one has in mind is that it seems reasonable to bet some very high odds, such as 100 to 1, or 1000 to 1, that the value in question lies within the stated limits. Now the concept of "probable error" can equally well be applied in any such situation, since it merely defines a region for which the odds are *even* that the true value lies within it. It is entirely unnecessary to base the estimated probable error merely on accidental errors. In fact one of my main efforts during the past ten years has been to emphasize the necessity of considering also systematic errors in any published estimate of probable error. Hence when I write $x \pm r$ for a physical quantity, I mean merely that, in the light of *all* the facts available to me, it appears an even bet that the true value lies within an interval $2r$, centered on x.

In considering experimental values of physical constants, it is not unreasonable to assume a normal distribution of errors, since in general actual distributions found in practice seem to approximate the normal error curve more closely than any other convenient mathematical distribution. Now for a normal distribution there is one chance in 100 of getting an error greater than $3.82r$ (where r is probable error), and 1 in 1000 of exceeding $4.90r$. Hence if a writer chooses to give what he terms a "limit of error," it is necessary merely to divide this result by, let us say, 4 or 5, to get a corresponding estimate of probable error. It is obvious that the reverse process is equally possible. It would, however, be very advantageous to have a uniform procedure, and mainly for historical reasons I have advocated, and still advocate, the publication of values of constants with an estimate of the region of the "even bet" (i.e., probable error), rather than of the region corresponding to other arbitrarily adopted odds.

The experimental data given by von Friesen for each of the constants e, h and e/m appear very consistent, but a fundamental discrepancy, which he fails to mention, exists between his finally adopted values of these three constants. These values are $e = (4.800 \pm 0.005) \times 10^{-10}$ e.s.u., $h = (6.610 \pm 0.015) \times 10^{-27}$ erg·sec., $e/m = (1.7585 \pm 0.002) \times 10^{7}$ e.m.u., where his uncertainties, as noted, are intended to represent "limits of error." Now these three adopted values do *not* satisfy the Bohr formula for the Rydberg constant, a formula which von Friesen accepts as correct. In fact, with his adopted values of e and h, one gets, from this formula, $e/m = 1.766$, a value far outside his stated limit of error.

The very disconcerting discrepancy that exists between the directly measured values of e, h/e, and e/m has already been emphasized.[3] The experimental evidence that has since appeared has served only to sharpen the inconsistency in question. I have discussed this situation with many persons and it is the present consensus of opinion that the "grating value" of e is substantially correct (I gave 4.8029 ± 0.0005 for this, in 1936), and that the Bohr formula for the Rydberg constant should be at least very closely correct. When one substitutes in this formula the value of e just quoted, and von Friesen's adopted value of e/m (1.7585), which is certainly close to the present best observed value, the resulting value of h/e is 1.3796×10^{-17} e.s.u., and this is *greater* than *any* directly observed value. The recent beautiful work on h/e by DuMond and Bollman[4] brings into even greater prominence the serious discrepancy between the precise directly observed values of h/e, and the value calculated indirectly from e and e/m.

In spite of the excellence of the recent experimental work on h/e, of which no serious criticism has yet been voiced, I have, in common with many others, reluctantly been forced to the conclusion that *all* of the present experimental values of h/e are low, and that the true value will eventually be found to be in the neighborhood of that just calculated. With $e = 4.8029$ this value requires that $h = 6.626 \times 10^{-27}$ erg·sec., and that $1/\alpha = 137.044$, in close agreement with Eddington's predicted value of 137. It is, however, only fair to admit that, until a reasonable source of error has been suggested for the present experimental values of h/e, the true values of h and of α remain very uncertain.

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[1] Sten von Friesen, Proc. Roy. Soc. **A160**, 424 (1937).
[2] R. T. Birge, Rev. Mod. Phys. **1**, 1 (1929).
[3] R. T. Birge, Nature **137**, 187 (1936).
[4] J. DuMond and V. Bollman, Phys. Rev. **51**, 400 (1937).