

THE GENERAL PHYSICAL CONSTANTS
AS OF AUGUST 1941 WITH DETAILS ON THE
VELOCITY OF LIGHT ONLY

BY

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INTRODUCTION

THIS paper is being written on request—and at *this time* on request. In spite of the delusive word "Constants" appearing in the title, it is the continual variation in the values of these quantities that furnishes most of the interest in the subject. It would indeed be disheartening to any real scientist to feel that an accepted value of any physical constant would never again be changed. The most characteristic feature of science—in general and in particular—is its never-ending change. But what one must not overlook is the fact that it is the *probable* value of any given constant that changes, not its *actual* value. A belief in any significant variability of the constants of nature is fatal to the spirit of science, as science is now understood.

It is, of course, a fact that the experimentally determined value of every "constant" varies in general with each determination, and just such variations often furnish invaluable clues to errors of experiment and of theory. It has always seemed to me that the chief value of the critical work that has been done on the general constants lies just in its potential exposure of such experimental or theoretical errors. In fact, possibly as a result of such critical work, the situation in respect to the general constants is now vastly improved over that obtaining in 1929, when I wrote my first general paper (Birge, 1929; hereinafter referred to as G.C. 1929) in the field; and for the first time since 1929 it seems possible to present with some confidence a new set of values.

Another most desirable object of a critical study of the general physical constants is to furnish a *consistent* set of values. This object, no matter how desirable, is really impossible of attainment, and the longer one works in the field, the more one is impressed with this rather discouraging fact. The general physical constants are woven together in a net of innumerable strands. It is scarcely an exaggeration to say that any change in the adopted value of a given constant affects, *to a greater or less degree*, the apparent best value of every other constant. Fortunately this degree of dependence varies very greatly from constant to constant, and in G.C. 1929 I attempted to reduce the difficulty to a minimum by starting with those least dependent on others, and working finally to those dependent on others.

The general physical constants

Because of the general interdependence of all constants, it is never possible to work up a strictly consistent set of values that at the same time represents the best experimental evidence. The work on any such paper as this necessarily extends over many months, if not years, and during the work new experimental results are constantly appearing—not to mention the *old* results that one, in some inexcusable way, has overlooked. Any one such new result, if accorded proper recognition, is capable of spoiling half the calculations of an entire paper. It may not be inappropriate to mention also the human element involved. One should, in principle, undertake such work very methodically, beginning, as just stated, with the constant least dependent on others, and working systematically through the remaining constants, in a carefully predetermined order. Actually, however, one's time is strictly limited, especially in these days of international stress, and when an exciting new result appears, one is apt immediately to test its effect on the most probable value of the constant concerned, without stopping to investigate whether any of the auxiliary constants have also recently changed in value. Such a calculation frequently develops into an extensive investigation, which one would prefer not to repeat too often.

A particularly unfortunate example of such a situation may be mentioned. The values of the electronic charge e and the specific electronic charge e/m appear more often in the numerous derived constants than possibly any other of the fundamental constants. Now the value of e depends on the value of the Faraday F , when e is calculated in the present most reliable way. Just one half of the present twelve most reliable values of e/m also involve F . But the value of F depends upon the adopted atomic weights of iodine and silver, and the most accurate determinations of I depend in turn on the value of Ag. Since 1925 the internationally accepted value of Ag has been 107.880, and there appears to be nothing in any recent report of the International Committee on Atomic Weights to indicate that any change is contemplated or needed in the adopted value. Hence, in working up the present list of constants, I merely accepted the value 107.880 ± 0.001 , which I had adopted in G.C. 1929. Then, after my calculations were completed, I happened to read something in my notes that aroused my suspicions about the value of Ag. As the result of a subsequent hasty and still incomplete investigation, it appears to me that the best value is far more likely to be 107.878 than 107.880. To get out of this difficulty with a minimum of recalculation, I have increased the probable error and now adopt $Ag = 107.880 \pm 0.002$, thus covering the possibility of 107.878. Since the atomic weight of silver is the basis for nearly all atomic weights, it is not to be changed without due consideration.

The moral of the incident is that one should take nothing for granted. Unfortunately no one can be an expert in every field, and I often feel that I should apologize for expressing critical opinions in many fields concerning which I know very little. It is true, however, that the experts in one field often use values of auxiliary constants definitely different from those used by experts in another

field. In fact, all too often the various experts in a given field are using simultaneously a bewildering variety of values. Strangely enough, such variations are often marked off by strict geographical boundaries, a state of affairs that has shown vast improvement in the past century, but is still open to a lot more improvement. Hence, in order to arrive at any *consistent* set of values, it is necessary to go far "behind the scenes" of any published result, and just because of the practical impossibility of doing this either thoroughly or simultaneously for all the constants, no published list of constants can ever be satisfactory to the author, regardless of how the reader may feel.

With this very personal, but I trust pertinent, explanation of the problems involved in critical work on physical constants, I proceed to the details. Within the limits placed on the present article, it is possible to set forth only a very small fraction of the relevant material. I hope later to publish elsewhere a far more extended account. Hence I have chosen just one of the general constants for a detailed discussion—namely, the velocity of light. This constant furnishes a good illustration of the sort of problems that arise in the critical evaluation of any constant, and because of the much publicized suggestion that the velocity of light may be changing in value, I trust that the discussion will be not without interest.

On the other hand, I have now practically finished a re-examination of all the general constants, and the newly recalculated and adopted values are given in tables at the end of this paper, together with the adopted values of all auxiliary constants used in the calculations, and the values of a few derived constants. A very brief statement is made concerning each of the general constants, and the lettering of sections is the same as that used in G.C. 1929. Incidentally, the introduction to that paper should be considered also as an additional introduction to the present paper.

SECTION A

THE VELOCITY OF LIGHT IN VACUUM (e)

The velocity of light is possibly the most fundamental of all constants. Certainly more time and money have been devoted to its measurement than in the case of any other general constant. But in spite of its long history, beginning with Römer in 1675, no measurement comparable in accuracy with recent work was made previous to 1900. Science moves forward at an ever increasing pace, and three-quarters of the papers to be cited in this article have been published since my previous account in 1929! But de Bray (1927-34) appears to take quite seriously certain older measurements, and as a result he obtains the truly alarming deduction that the velocity of light is decreasing at the rate of some four km./sec. per year. At that rate the velocity would go to zero in a mere 75,000 years. Now it is quite possible that the length of the day is increasing very slowly—let us say 0.01 sec. per century. This change would cause the apparent velocity of light to increase one part in 10^7 per century, whereas the de Bray equation

corresponds to a *decrease* of one part in 750 per century. Since this question of

- p. 90 last line. Insert "most" before "dependent".
- p. 98 line 4. Mercier (1925 a and b - 24)
- p. 103 line below Eq. (3). Insert "limiting" before "value".
- p. 107 10th line from bottom. The sentence is clarified by the substitution of "the same value" for "one same".
- p. 112 line 4. J' 15 in place of J15.
- p. 112 line 5. Footnote symbol should be a dagger.
- p. 112 line 20. Footnote symbol should be a star.
- p. 120 **Last line of Table.** Goedelke ^{at} place of Goedelke.
- p. 121 first line of first footnote. 1932c in place of 1932b.
- p. 122 insert a star before footnote.
- p. 125 15 lines from bottom. Birge, 1940b in place of Birge, 1940.
- p. 125 first line of last paragraph, $b/e = (1.3785 \text{ etc.})$
- p. 126 Table a. Unit of G ; cm.² in place of cm.
- p. 130 Radiation const^{tr} constant, $4\pi^2 k_0$, etc., in place of $4\pi k_0$.
- p. 135 Naujan, Uroff, etc. 1954 in place of 1955.

corresponds to a *decrease* of one part in 750 per century. Since this question of the variability of c has attracted much attention, among both scientists and laymen, it is advisable to consider the present evidence rather critically.

In G.C. 1929 I merely accepted Michelson's 1927 result of $c = 299796$ km./sec. (Michelson, 1927). This result was believed, at the time, to have a probable error of only 4 km./sec. But Michelson, Pease and Pearson (1935; to be denoted by M.P.P.) found irregular and completely mysterious variations in the measured value of c occurring daily, and even hourly, as well as over longer periods of time. A subsequent examination of Michelson's original notebooks, I am told, has shown that similar variations were present in his work, although he had failed to notice them at the time.

Michelson greatly overestimated his accuracy in many respects. Thus the distance from Mt. Wilson to Mt. San Antonio ("Baldy"), some 35 km., was believed to have been measured correctly to about one part in 10^6 (or 35 mm.). On the other hand, M.P.P. found that their carefully measured mile-long base line showed for two years a steady increase in length of 6.5 mm. per year (or 4 parts in 10^6 per year), and then a sudden *decrease* of 8 mm. after a mild earthquake in the vicinity! As the late D. C. Miller has noted, between the time that Michelson's 35-km. base line was measured and his actual measurements on c were made, there occurred the disastrous Santa Barbara earthquake, and his actual base line may well have suddenly changed by several feet! Furthermore, Michelson's measurements were made under a wide variety of temperatures and barometric pressures, but Michelson used only a single correction to vacuum for his measured velocity in air. For these, and other reasons, I am now increasing the probable error of Michelson's 1927 work to 15 km./sec. This new probable error may be slightly too large, and this point will be mentioned again later.

Michelson's published result of 299796 km./sec. is also in need of revision. To obtain it he used 67 km./sec. for the correction to vacuum from air at an assumed average temperature of 20° C. and at an average barometric pressure of 625 mm. Hg. It is easy to show that his correction results from the use of the *wave* index of refraction, instead of the correct *group* index of refraction. This is one of the most inexplicable errors that I have ever come across in the literature. It is well known that Michelson, about 1883, obtained $1.330 (\pm ?)$ for the index of refraction of water, and 1.76 ± 0.02 for the index of CS_2 , by a direct comparison of the velocities in air and in the two media mentioned.

Now the *wave* index of refraction of CS_2 is 1.624 for the "D" line, and 1.637 for the "E" line. To explain his high observed value for CS_2 , Michelson employed the concept of *group* velocity (and the corresponding *group* index of refraction), as discussed in connection with the velocity of light by Rayleigh (1881 a, 81 b), although the first correct formulation of group velocity should be credited to W. R. Hamilton (*c.* 1835), according to a review in *Nature* (Ramsey, 1940). For CS_2 the *group* index μ_g ($= \mu - \lambda_0 d\mu/d\lambda_0$, where μ is the wave index and λ_0 the wave-length in vacuum) is 1.722 and 1.767 for the "D" and "E"

lines, respectively, in good agreement with Michelson's observed result. This is, however, the *only* instance in which Michelson ever used the concept of group velocity! He never applied it to his work on the absolute value of c , and neither has anyone else working in the field, until I called Dr. Anderson's attention to the matter (see Anderson, 1941) several years ago. Michelson did not even apply group velocity to his observed index for water. If he had, it would have spoiled the apparent agreement, for the group index is 1.352 and 1.359 (for the D and E lines), whereas the wave index is 1.334 and 1.336, as compared to his observed 1.330.*

The correction of the velocity of light to vacuum from air, for an effective wave-length of $\lambda 5700$, is 82.97 km./sec., at 0° C., 76 cm. Hg. pressure, with the use of the wave index of refraction, and 85.64 km./sec. with the use of the correct group index. At Michelson's pressure and temperature the differential correction is slightly over 2 km./sec. Hence Michelson's actual 1927 result is 299798 km./sec., in place of his 299796. I first noted these facts regarding group velocity shortly after I had written my letter to *Nature* on the value of c (Birge, 1934 b). Since no such correction was needed in the work of M.P.P., and since their work had then shown that Michelson's 1927 work was far less reliable than had been thought, there seemed to be no proper excuse for publicizing the matter. But the group versus wave-index correction becomes more serious in the case of recent Kerr cell work on c , as will be discussed presently.

We come now to the work of M.P.P. This is one of two really extensive recent investigations on the value of c , carried out under controlled and accurately known conditions. The second investigation is that of Anderson in 1940, in which a Kerr cell was used (Anderson, 1941). Although Anderson's work will be mentioned in connection with other Kerr cell work, it is convenient to consider at this point certain aspects of the work of both M.P.P. and Anderson.

The measurements of M.P.P. were made in a mile long evacuated tube, under definitely known and very steady conditions of temperature and pressure. A total of 2885 values of c were obtained from observations made on about 165 different days (actually nights), extending over 24 months' time. Anderson obtained 2895 values of c from observations made on 22 different days, extending over 18 months' time. The average value of all the M.P.P. observations is 299774 km./sec., and the average of Anderson's is 299776. The mean epoch of the M.P.P. work is 1932.5, that of Anderson is 1940.0.

It is not possible to make an entirely satisfactory comparison of the consistency

* Similarly Michelson found that "orange-red" light travels 1 to 2 per cent faster than "greenish-blue" light in CS_2 , a result that he considered, and that has since been regularly quoted as, in good agreement with theory. Now if we take 6000 Å. and 5000 Å. as reasonable values for the effective wave-lengths of the light used, the expected difference in velocity is 1.4 per cent and 3.95 per cent on the basis of the correct group index. Gutton (1911) has shown conclusively that the dispersion of the velocity of light in CS_2 does agree with the group-index equation, and *not* with that for the wave index. Gutton obtained no absolute values of the velocity.

of the two sets of measurements, partly because the form of the published material is not the same in the two cases. M.P.P. publish data from which one can plot a distribution curve of the 2885 residuals. They also show such a plot. Most fortunately the plot is quite symmetrical, so that the average value of $c = 299774$ km./sec. can be accepted with some confidence. On the other hand, the distribution of the residuals deviates appreciably from a normal error curve. But I find that a very good fit can be obtained if one takes the sum of two such curves, one with a standard deviation of 5 km./sec. (for a single observation) and the other of 15 km./sec.* Now 5 km./sec. is roughly the deviation to be expected for a single observation. Hence we can assume that in the case of one half the observations, the expected experimental conditions were attained, whereas in the case of the other half, a multitude of errors of one kind or another led to a standard deviation three times as large. Of course many other similar assumptions would doubtless give an equally satisfactory fit. The point is that if a large assortment of observations are *not* of equal reliability, their residuals cannot be expected to follow a normal error curve, and I am more and more convinced that the deviations from such a curve found so commonly in large groups of physical measurements are due usually just to this cause.†

M.P.P. tabulate their observations in 233 series. The average deviation *within* each series from the mean of that series is about 10.5 km./sec. A plot of their observations against time shows unmistakable evidence of irregular deviations from the general average of 299774 km./sec. Deviations of 10 km./sec., over a period of a week or more, are common, and deviations up to 30 km./sec. occasionally occur. The authors were unable to locate the precise origin of these variations. They are probably due in part to instrumental difficulties and in part to the fact that the earth must be considered as a dynamic rather than as a static body.

Anderson gives only the 22 average values for his 22 days of observing. The average deviation *within* each of the 22 series is 9 km./sec., comparing well with the M.P.P. figure of 10.5 km./sec. The average deviation of the 22 daily averages from the final average is 14 km./sec. A corresponding result for the M.P.P. data could be derived, by sufficient calculation, but thus far I have computed only the 13.37 km./sec. standard deviation ‡ of the entire 2885 observations, as already stated. It would appear, however, that the distribution of residuals for the two investigations has essentially the same average spread, although I have no information on the *shape* of Anderson's distribution curve.

Anderson himself believes that the irregular deviations with time occurring in his work are due chiefly to a certain instrumental difficulty, which he discusses

* The best single normal error curve that can be passed through the data has a standard deviation of 13.37 km./sec.

† But with carefully controlled experimental conditions, the writer (Birge, 1932 a) obtained an entirely satisfactory normal distribution of residuals.

‡ The average deviation is the arithmetic average, whereas the standard deviation—which gives a more reliable estimate of uncertainty—is the root-mean-square average.

in detail. Just because of the known existence of this difficulty, I shall assign a larger probable error to his final result than to that of M.P.P. With a standard deviation of 13.37 km./sec. for 2885 observations, the probable error of the average is, by formula, only 0.17 km./sec. Obviously, as a result of the systematic and instrumental errors that were evidently present, one must assume for the effective probable error of the average value of c a quantity far greater than this. I think, however, that 4 km./sec. is a reasonable estimate in the case of the M.P.P. work, and with this figure as a basis I take 6 km./sec. as the probable error of Anderson's 1940 work.*

Because of the low pressure of the air (0.5 to 5.5 mm. Hg) in the M.P.P. vacuum tube, the total correction from air to absolute vacuum is only 1.5 parts in 10^6 , on the average, and the distinction between wave and group velocity is obviously negligible. Anderson (1941) has, however, suggested that the M.P.P. result may need correction for group velocity, due to the fact that the light enters and leaves the vacuum tube through a glass window (0.75 inch thick). Now the change in their total optical path of ten miles, resulting from the use of group velocity in place of wave velocity through 1.5 inches of glass, is 0.073 inch, or only one part in nine million. Hence the final M.P.P. result is $c = 299774 \pm 4$ km./sec. Anderson used the group-velocity equation in getting his published average of 299776, and hence I adopt 299776 ± 6 km./sec.

We now consider as a group four investigations on the velocity of light, made by means of a Kerr cell, which replaces the toothed wheel in Fizeau's classic arrangement. The base line in each case was about 40 metres. One of the four investigations is Anderson's 1940 work, just discussed. In each of the four investigations a quite distinctive experimental arrangement was employed, but to present an adequate discussion of these different arrangements would require many pages. Due to limitations of space I am omitting, in general, all reference to experimental details. On the other hand, it is essential to consider the corrections to the various published values, required by the substitution of group velocity for wave velocity.

The first precision Kerr cell work is that by Mittelstaedt (Karolus and Mittelstaedt, 1928; Mittelstaedt, 1929 a, 29 b). His light path of 250 metres included a portion of the nitrobenzene in his Kerr cells, as well as glass lenses and, of course, air (at 75 cm. Hg average pressure). Mittelstaedt states that in order to get the true optical path length, the geometric length should be increased by 68 mm. for the air, 21 mm. for the glass and 12 mm. for the nitrobenzene, a

* There are two reasons for assigning a probable error. The first is to give an actual estimate of the size of the region within which the odds are even that the true value lies. The other, and more important reason for our present purpose, is to get a basis for the relative weights to be assigned the various observed values, when calculating a final weighted average. For this second purpose only the relative probable errors are of concern. In the case of the velocity of light, I take ± 4 km./sec. for the M.P.P. work as my basic probable error, and I have then attempted to assign such probable errors to other results as seem reasonably to express the relative reliability of the various investigations. This general principle holds throughout the present paper.

total of 101 mm. These results, as usual, correspond to the wave index, for which I find that the exact correction is 68.36 mm. for 250 metres of air, at 75 cm. Hg pressure and for $\lambda 5500 \text{ \AA}$, the effective wave-length used by Anderson and by Mittelstaedt. The needed dispersion constants are listed by Anderson (1941), and I find for the proper corrections, using the group index of refraction, 70.75 mm. for air, 22.82 mm. for glass, and 13.96 mm. for nitrobenzene, a total of 107.53 mm.* The total correction to c is, therefore, $(101/250000) \times 299776 = 121.11$ km./sec., according to Mittelstaedt, but is correctly $(107.5/250000) \times 299776 = 128.94$ km./sec., an increase of 7.83 km./sec. Thus Mittelstaedt's true result is 299786 km./sec., in place of the figure 299778 that he publishes. Mittelstaedt gives 20 km./sec. for his "limit of error". I still believe, as I did in 1934 (Birge, 1934 b), that 10 km./sec. is a reasonable value for his probable error.

The second Kerr cell result to be discussed is that of Hüttel (1940). In his experimental set-up, which represents a great improvement over that used by Mittelstaedt, there is only air in the light path. The necessary correction to his published result is therefore presumably about 3 km./sec. (2.9 km./sec. for $\lambda 5500 \text{ \AA}$, or 2.67 km./sec. for $\lambda 5700 \text{ \AA}$). His published result is 299768, and I correct this to 299771 km./sec. This result is the average of 135 observations, listed in 8 groups that show a maximum variation of 35 km./sec. Hüttel adopts 10 km./sec. for his final probable error, and since his work is certainly as reliable as that of Mittelstaedt, I accept his estimate.

The Kerr cell work of Anderson (1937, 41) has been carried on over more than three years. In his first experimental arrangement (1937), a certain amount of glass (lenses) was included in the light path. In his second paper (1941) he gives the group-velocity correction, to which I had meanwhile called his attention. The correction is 7 km./sec. for his first investigation, making his result 299771 km./sec., in place of 299764. To obtain this average value Anderson made 651 measurements of c , and his plot of their distribution shows approximately the same "spread" as in the case of the M.P.P. work. The observations were taken on 12 different days, distributed over 6 months of time. The average deviation within each of the 12 daily averages is 12 km./sec., and these 12 averages have an average deviation of 8 km./sec. from the final average. Because of the small number of observations in Anderson's first investigation, as compared to that in his second, I assign a probable error of 10 km./sec. to his first average, just as I have done in the case of Mittelstaedt and of Hüttel. Anderson's first result is, then, 299771 ± 10 km./sec., and his second, as already discussed in detail, is 299776 ± 6 km./sec. In the second investigation, all glass lenses were eliminated from the light path, and the electrical arrangements were quite different.

There are two indirect methods for measuring c , although neither method is* more indirect than any method for measuring h/e , or any other atomic constant.†

* Anderson in some way calculates only 13.4 mm. for the nitrobenzene.

† de Bray appears to ignore such indirect methods in his discussions of the value of c .

it is a value that has been used rather widely since that time, it is fortunate that the most recent work furnishes no grounds for suggesting a change.

If the Michelson, 1926, result is given a probable error of 10 km./sec., which I now think is the *minimum* permissible, the new weighted average is 299777.28; if his result is entirely ignored, the average is 299775.7 km./sec. It seems to me, therefore, that the presumption of evidence is in favour of 299776 rather than 299777, and in any case, a change of only one km./sec. would be devoid of significance.

The weighted average of the five older results is $299873.8 \pm 8.6_8 (\pm 22.6_1)$ km./sec., and the ratio r_e/r_i is 0.384. Thus these older results are entirely consistent *among themselves*, but their average is nearly 100 km./sec. greater than that given by the eight more recent results. The cause of the sudden change in the experimentally determined values of c , at the opening of the 20th century, might be an interesting subject for investigation, but I would hesitate to believe that this 100 km./sec. change is real.

Using all thirteen results we get, as the weighted average, $299777.8 \pm 2.5_{78}$ ($\pm 2.6_{94}$) km./sec., and the ratio r_e/r_i is 0.957. This last figure has considerable significance in connection with de Bray's hypothesis. It shows that the assumption of a *constant* value of c (for the period 1875 to 1940) is consistent with all the experimental evidence, when the probable error of each result is taken into consideration. Now my own philosophy of science—a philosophy that has been stated most eloquently by H. Poincaré (1913)—is that the simplest satisfactory explanation of a given set of phenomena should always be chosen. If the assumption of a constant value of c satisfies the data, it is gratuitous to postulate a systematically changing value.

Another, and more precise, method of testing the hypothesis of a linear variation of c with time is to calculate, by least squares, the best straight line passing through the data, plotted with the observed values of c as ordinates and the epoch (D), given in table 1, as abscissæ. For all 13 points the result is

$$c = 299779.18 \pm 1.97 - (0.901 \pm 0.192) (D - 1930) \text{ km./sec.}$$

The ratio $r_e/r_i = 0.723$, so that the internal probable errors are 2.72 and 0.265 respectively, and hence, by internal consistency, the slope is 3.4 times its probable error. Statistically, this is a barely significant result. Incidentally, the calculated rate of change is less than one km./sec. per year, in contrast to the four km./sec. per year postulated by de Bray.

If, now, we use only the eight really reliable values, covering the period 1906 to 1940, the result is

$$c = 299777.27 \pm 1.37 - (0.381 \pm 0.158) (D - 1930) \text{ km./sec.}$$

and $r_e/r_i = 0.490$. Hence the probable error of the slope, by internal consistency, is ± 0.323 km./sec. per year, and the calculated slope of 0.381 km./sec. per year has no significance whatever. This conclusion is fairly obvious from an inspection of table 1, but it is well to put it in quantitative form. I believe, then,

that there is now no experimental evidence of a linear variation in c , with time, although recent investigators have found continuously occurring irregular variations (from hour to hour, as well as day to day), due doubtless to a lack of complete stability in the experimental arrangements. Edmondson's (1934) suggested harmonic variation, as given by the equation *

$$c = 299885 + 115 \sin(2\pi/40)(D - 1901),$$

is already completely disproved, since it predicts $c = 299867$ km./sec. for 1940.0, contrasted with Anderson's observed $c = 299776 \pm 6$ km./sec. Thus, after a long and, at times, hectic history, the value of c has at last settled down into a fairly satisfactory "steady" state. The same thing can now be said of other important constants. Otherwise I would not be writing this paper!

SECTION B

THE NEWTONIAN CONSTANT OF GRAVITATION (G) AND THE MEAN DENSITY OF THE EARTH

In G.C. 1929 I adopted for G Heyl's final value at that time, namely, $G = (6.664 \pm 0.002) \times 10^{-8}$ dyne.cm² g.⁻² (Heyl, 1927). The adopted probable error is actually his average deviation from the average, for five determinations ranging from 6.667 to 6.661. The small masses used in the vibrating pendulum were made of platinum. Later Heyl continued his experiments, using gold and glass masses. His final averages (Heyl, 1930) for the three different materials differ considerably more than expected on the basis of their internal consistency, and he was not able to trace the origin of the discrepancies. Thus the final values for platinum, gold and glass are 6.664, 6.678 and 6.674 respectively, whereas the average deviations from the average for each material are only 0.002, 0.003 and 0.002 respectively. Heyl assigns weights of 3, 1 and 3, and thus obtains a weighted average of $6.670 \pm 0.002_7$, on the basis of external consistency. Because of the unexpected discrepancies between the three results, I increase the probable error to 0.005, and adopt

$$G = (6.670 \pm 0.005) \times 10^{-8} \text{ dyne.cm}^2 \text{ g.}^{-2}$$

In G.C. 1929 I used $G \cdot \delta(\text{earth}) = 36.797 \times 10^{-8} \text{ sec.}^{-2}$, where $\delta(\text{earth})$ is the average density of the earth. The figure is taken from Henning and Jaeger (1926 b). With the present adopted value of G we get

$$\delta(\text{earth}) = 5.517 \pm 0.004 \text{ g.cm.}^{-3}$$

SECTION C

RELATION OF THE LITRE (1000 ml.) TO THE CUBIC DECIMETER (1000 cm³)

The litre is defined as the volume of a kilogram of air-free water at its maximum density. The kilogram is the mass of the "International Prototype Kilogram", preserved at Paris. It has been customary to use "c.c." for the designation of

* This equation has been discussed (Birge, 1934 b).

and Dole (1939) and Dole and Slobod (1940). The facts, briefly stated, are as follows.

"Ordinary water" (from fresh-water lakes as well as the tap-water of various cities) shows an exceedingly uniform density. Water from the ocean (after distillation) has a density one to two gamma units in excess of ordinary water.* The water prepared from the oxygen in geologically old iron ores does not differ significantly from ordinary water. But water from the oxygen of carbonate rocks of all kinds is heavier by about eight gamma units. Furthermore, it is known that in the case of CO_2 in contact with water, there is an excess of ^{18}O in the CO_2 , equivalent to about ten gamma units. Finally, water prepared from atmospheric oxygen is heavier than ordinary water by 6·6 gamma units. This last difference is the so-called "Dole Effect", and the 6·6 gamma unit value is due to Swartout and Dole (1939), whose investigation is the latest and most accurate on this subject.

We can now draw the following conclusions. The oxygen in most rocks, regardless of geologic age, has a very uniform isotopic composition, which is the same as that of oxygen in ordinary water. But carbonates, which come from animal life, have a distinctly higher ^{18}O abundance, and the oxygen of the atmosphere, which is believed to come almost wholly from the photosynthesis of plants, has a similar higher abundance. In both cases the differential effect is presumably due to the natural higher abundance of ^{18}O in CO_2 , which has been explained on theoretical grounds. From the foregoing evidence we can also feel quite certain that the variations in the isotopic abundance found by Manian, Urey and Bleakney (1934) are almost entirely experimental in origin.

It should be noted, in this connection, that the work just cited on the density of water also leads to the abundance ratio $^1\text{H}/^2\text{H} = 6900 \pm 100$, given in table *a'*. The involved experimental procedures designed to isolate a possible changing $^1\text{H}/^2\text{H}$ ratio from a possible changing $^{18}\text{O}/^{16}\text{O}$ ratio are described in detail by Swartout and Dole (1939). Furthermore, if we now assume that the adopted $^{16}\text{O}/^{18}\text{O}$ ratio = 506 applies to the oxygen of ordinary water, and hence to that in most rocks (*excluding* carbonates), then the ratio for atmospheric oxygen is about 492.†

All of the atomic weight values that have been adopted for the purposes of the present investigation are listed in table *a'*, at the end of the paper. No real details will be given here. The adopted values of H, He, C and N on both the physical and chemical scales are based on the mass spectrograph values of the physical scale, with the indicated isotopic abundance. In all cases I have used what appears to be the best average of recent determinations. In the case of nitrogen there is given also the present average result (14.0086 ± 0.0007) derived from limiting gas-density investigations.

* It is this excess that is mentioned in connection with the litre, in Section C.

† A gamma value of 6·6 equals a change of 13·5 in the $^{18}\text{O}/^{16}\text{O}$ ratio.

One of the best determined of all chemical ratios, based on gravimetric work, is $\text{AgNO}_3/\text{Ag} = 1.574790$, which appears to have a probable error of 0.000005 or less.* From $N = 14.00740 \pm 0.00012$, as determined from mass spectrograph work (see table *a'*), one gets immediately $\text{Ag} = 107.8783 \pm 0.0009$, and this result is the basis for my remarks about the atomic weight of silver, in the introduction to this paper. From the limiting density value $N = 14.0086 \pm 0.0007$, and the same AgNO_3/Ag ratio, one gets $\text{Ag} = 107.880 \pm 0.001$. As already noted, I adopt $\text{Ag} = 107.880 \pm 0.002$, pending a more comprehensive examination of this important question.

The value for iodine (126.915 ± 0.004) results from a study of the latest data, much of which is discussed by Baxter.† The present adopted value deviates considerably from the value (126.932 ± 0.002) adopted in G.C. 1929, but we now have almost identical results yielded by several quite different experimental methods, and except for the remaining uncertainty due to its dependence on the value of Ag, the atomic weight of iodine seems well established.

The atomic weights of calcium, chlorine and sodium are based on chemical determinations. The first is needed in the evaluation of the electronic charge. The last two are fundamental atomic weights, used in some of the results for iodine and for many other elements.

SECTION G THE STANDARD ATMOSPHERE (A_0)

The standard atmosphere is defined as the pressure exerted by a column of mercury 76 cm. high, of standard density (i.e., at 0°C , A_0), under standard gravity (g_0). Although A_0 is itself used in the definition, the very slight variation of the density of mercury with pressure makes this inclusion of A_0 possible. The standard atmosphere is then defined by

$$A_0 = H_0 \cdot \rho_0(\text{Hg}) \cdot \delta_m(\text{H}_2\text{O}) \cdot g_0, \text{ where } H_0 = 76.000 \text{ cm.} = \text{height of standard barometer;}$$

$$\rho_0 = \text{standard specific gravity of Hg (at } 0^\circ \text{ C., } A_0\text{), referred to air-free water of maximum density;}$$

$$\delta_m(\text{H}_2\text{O}) = \text{maximum density of water} = 0.999972 \pm 0.000002 \text{ g.cm.}^{-3} \quad (\text{Section C});$$

$$g_0 = \text{standard gravity} = 980.665 \text{ cm.sec.}^{-2} \quad (\text{Section B}).$$

The only quantity that remains to be evaluated is thus ρ_0 . In G.C. 1929 I adopted $\rho_0 = 13.59546$, which is the unweighted average of four determinations listed by Henning and Jaeger (1926 a). The discussion by these authors indicates, however, that only the last investigation of the four—that by Scheel and Blankenstein (1925)—is worthy of serious consideration, and even their result of 13.59549 is open to some question, for the following reason. Their work, carried out by the hydrostatic method, gave two results. One result, 13.59540, was obtained with mercury "E", a quantity of mercury used at the *Reichsanstalt* in standard electrical resistances. It had often been distilled. The other result, 13.59558,

* See page 24 of G.C. 1929.

† Baxter, Guichard, Höning schmid and Whytlaw-Gray, 1941.

was obtained with mercury "III", a quantity of mercury used in the measurement of small pressures. For the present investigation it was distilled in vacuum and a trifling amount of impurity was found. Since this second sample had seldom been distilled, the difference of 13 parts in 10^6 might be real, due to some difference in isotopic composition brought about by the distillation. But Jaeger and Steinwehr (1926), using the same two samples of mercury, found later, by direct observation of relative density (a more accurate process), that the two densities agreed within a very few parts in 10^6 . Hence the previously found difference must be purely experimental, and under the circumstances one can only adopt the average value, with half the difference as its probable error, i.e., $13 \cdot 59549 \pm 0 \cdot 00009$.

Meanwhile a new investigation has been carried out by Batuecas and Casedo (1936). They used mercury from the Almaden mines, and with the pycnometer method obtained $\rho_0 = 13 \cdot 59539$. Their published probable error is only $0 \cdot 00001$, but a study of their work indicates that $0 \cdot 00006$ is a more reasonable estimate. With this latter figure, the weighted average of the two investigations is $\rho_0 = 13 \cdot 59542 \pm 0 \cdot 00003$, ($\pm 0 \cdot 00005_0$). As usual I adopt the larger of the two probable errors, in this case that based on internal consistency.

Thus D_0 , the density of mercury at 0°C , A_0 ($= \rho_0 \cdot \delta_m$), is $D_0 = 13 \cdot 59504_0 \pm 0 \cdot 00005$, g.cm.⁻³, and finally *

$$A_0 = D_0 \times 76 \times 980 \cdot 665 = (1 \cdot 013246 \pm 0 \cdot 000004) \times 10^6 \text{ dyne.cm.}^{-2}$$

or

$$A_{45} = D_0 \times 76 \times 980 \cdot 616 = (1 \cdot 013195 \pm 0 \cdot 000004) \times 10^6 \text{ dyne.cm.}^{-2}$$

SECTION H

THE ABSOLUTE TEMPERATURE OF THE ICE-POINT (T_0)

In G.C. 1929 the adopted value of the ice-point was $T_0 = 273 \cdot 18 \pm 0 \cdot 03^\circ\text{K}$. This result was derived from Roebrick's observed value $273 \cdot 15^\circ\text{K}$, and Henning and Heuse's value † $273 \cdot 19^\circ\text{K}$. Since then very extensive work has been carried out, and fortunately this work has been thoroughly reviewed in two very recent summary papers. Hence all details may be omitted here. Both of the papers in question were read at the recent Symposium on Temperature, organized by the American Institute of Physics. Roebrick and Murrill (1941) give an extensive table of determinations of T_0 , with an average value of $273 \cdot 170^\circ\text{K}$. They advocate $273 \cdot 17 \pm 0 \cdot 02^\circ\text{K}$ as the present best value. Beattie (1941) gives a similar table with an average of $273 \cdot 165^\circ\text{K}$. He also describes his own recent work, which is the latest and possibly the most accurate that has been done in this field. Two different methods of handling his own data lead to $273 \cdot 165^\circ\text{K}$. and $273 \cdot 167^\circ\text{K}$, respectively.

* Had it not been for my premature rounding of data, the value $A_0 = 1 \cdot 013249 \times 10^6$ given in G.C. 1929 would have been $1 \cdot 013250$, and hence the present adopted A_0 and A_{45} values are each smaller by 4 dyne.cm.⁻²

† See G.C. 1929, pp. 28-29, for all references.

Meanwhile W. M. Keesom and W. Tuyn, acting as a sub-committee for the *Institut International du Froid*, issued in 1937 a mimeographed report in which they advocate $273 \cdot 15^\circ\text{K}$. as the best value of T_0 .* It may be noted, in this connection, that the Leiden value of $273 \cdot 144^\circ\text{K}$. is definitely lower than most recent results. So far as I am aware, this report has never been acted on, but according to *Nature*,† one of the resolutions which was to be presented at a projected, but never held, meeting of the *Institut International du Froid* was that $T_0 = 273 \cdot 15 \pm 0 \cdot 02^\circ\text{K}$. I believe that Keesom and Tuyn did not have available Beattie's recent result of $273 \cdot 165^\circ$ or $273 \cdot 167^\circ\text{K}$.

In view of the foregoing evidence, it appears that the best value is now

$$T_0 = 273 \cdot 16 \pm 0 \cdot 01^\circ\text{K},$$

and Dr. F. G. Brickwedde informs me that this is the opinion of the National Bureau of Standards. It would also appear that the value of the ice-point is now well established and that one can, with some confidence, adopt $\pm 0 \cdot 01^\circ\text{K}$. as its probable error.

SECTION I

THE JOULE EQUIVALENT (J) AND THE ELECTRICAL JOULE EQUIVALENT (J')

At the time G.C. 1929 was written there was available one outstanding piece of work on the electrical Joule equivalent and one on the mechanical Joule equivalent. Since then there has appeared another, and even more precise, evaluation of the electrical equivalent. Hence there are three results from which to get a weighted average.

The older work on the electrical equivalent is that by Jaeger and Steinwehr (1921; referred to in the rest of this section as "J. and S.") They obtained data over the range 5° to 50°C . As stated briefly in G.C. 1929, their published second degree power series equation, from which their adopted value of $J'_{15} = 4 \cdot 18420$ int. joules is derived, does not properly represent their data. In particular, the residuals show a serious trend in the vicinity of 15°C , just where it is most important that an accurate representation of the data be secured. I found, however, that a very satisfactory fit could be secured by means of a fourth degree power series, and from the new equation I obtained $J'_{15} = 4 \cdot 1832_7$ int. joules, with an estimated probable error of $\pm 0 \cdot 0008_0$ int. joule. This correction by the writer has been generally ignored in later German papers, although there is not the slightest question as to its validity.

Quite recently Osborne, Stimson and Ginnings (1939; referred to in the rest of this section as "O.S.G.") have made a new and extremely careful determination of J'_{15} . In their paper they discuss critically all of the older work. They too ignore the G.C. 1929 correction to the J. and S. result, but like the

* See also the lecture by H. B. G. Casimir (1939), in which $T_0 = 273 \cdot 15^\circ\text{K}$. is advocated.

† *Nature, Lond.*, 145, 597 (1940).

through the cell. To measure this latter quantity, Vinal and Bates (1914), who have carried out the only really precise determination, attached a silver voltameter in series to the iodine voltameter, so that the same current passed through both cells. They also, in the case of six runs, measured the current by means of a standard cell and standard resistance. The two values of Q thus obtained differ for the six runs by a maximum of 4 parts in 10^6 , with an algebraic average difference of only 1.5 parts in 10^6 .

Thus Q , in int. coulombs, is measured by $m_{\text{Ag}}/E_{\text{Ag}}^*$, where m_{Ag} is the measured mass of silver deposited in the silver voltameter. Then, from its definition, $E_i^* = m_i/Q$, where m_i is the corresponding mass of iodine released at the anode of the iodine voltameter. Hence we get

$$E_i^* = \frac{m_i}{(m_{\text{Ag}}/E_{\text{Ag}}^*)} = \frac{E_{\text{Ag}}^*}{(m_{\text{Ag}}/m_i)} \quad \dots \dots \dots (6)$$

Vinal and Bates' experimental result is $m_{\text{Ag}}/m_i = 0.850173_4 \pm 0.000016$,† and from equation (6)

$$E_i^* = \frac{0.00111800}{0.850173_4 \pm 0.000016} = (131.5026 \pm 0.0025) \times 10^{-5} \text{ g.} \quad \dots \dots \dots (7)$$

Finally, from equation (4),

$$F_i^* = \frac{126.915 \pm 0.004}{(131.5026 \pm 0.0025) \times 10^{-5}} = 96511.4 \pm 3.4 \text{ int. coulombs.} \quad \dots \dots \dots (8)$$

The difference between the results of equations (5) and (8) is 17.7 ± 3.9 int. coulombs, and it is this serious discrepancy (4.5 times its probable error) that has caused so much confusion and controversy.

My present solution of the difficulty, with all detailed explanation omitted, is as follows. It is known (Vinal and Bouvard, 1916) that inclusions exist in the silver deposit, to the average amount of 4 parts in 10^6 . Correction for such inclusions raises F_{Ag}^* by 3.9 coulombs to 96497.6 int. coulombs. Three different types of systematic error in the iodine voltameter are listed by Vinal and Bates (1914). Each of these errors tends to make the observed F_i^* too high, and the discussion of Vinal and Bates indicates that these three errors may well aggregate 5 parts in 10^6 , or 4.8 coulombs. With this assumption F_i^* becomes 96506.6 int. coulombs.

There are, however, various other possible sources of systematic error in both voltameters. Most of those mentioned for the silver voltameter tend to make F_{Ag}^* too high, and that is just the reason why, in 1929, I did not make the adopted value of F still higher, by using some weighted average of F_{Ag}^* and F_i^* . But Dr. J. W. M. DuMond has recently suggested‡ that there may be a preferential deposition of the two isotopes of silver, in the silver voltameter.

† This is my own calculated weighted average of their ten runs, with the probable error based on external consistency.

‡ Private communication.

The sign of this error, if it exists, is not known. Since the remaining types of possible systematic error in the iodine voltameter are partly positive and partly negative, I think it not unreasonable to include a probable error of 4 parts in 10^6 , in the case of each of the voltameters, to cover various additional sources of errors. On combining the new probable errors with those already given, we obtain

$$F_{\text{Ag}}^* = 96497.6 \pm 4.25 \text{ int. coulombs, } F_i^* = 96506.6 \pm 5.18 \text{ int. coulombs.}$$

The discrepancy thus becomes 9.0 ± 6.7 int. coulombs, which is not unreasonable. Hence it is now legitimate to weight the two results according to their probable errors, and we thus obtain $F^* = 96501.22 \pm 2.98 (\pm 3.29)$ int. coulombs, which we consider as our most probable value of the true F .

The probable errors just given make no allowance for the uncertainty in the deliberate corrections that have been made to the values of F , and because of the numerous possible sources of error, both known and unknown, it seems best to adopt a considerably larger probable error for the final weighted average. For some years past I have suggested a probable error of 10 coulombs, and I renew the suggestion. Hence I adopt, for the value of F on the *chemical scale*,

$$F = 96501.2 \pm 10 \text{ int. coulombs.}$$

Then, from the adopted value of q (Section E)

$$F = 96487.7 \pm 10 \text{ abs. coulombs}$$

$$= 9648.77 \pm 1.0 \text{ abs. e.m.u.,}$$

$$F' = Fc = (2.89247 \pm 0.00030) \times 10^{14} \text{ abs. e.s.u.}$$

Finally, from the adopted value of r (Section F), we obtain, for the values of F on the *physical scale*,

$$F = 96514.0 \pm 10 \text{ abs. coulombs}$$

$$= 9651.40 \pm 1.0 \text{ abs. e.m.u.,}$$

$$F' = Fc = (2.89326 \pm 0.00030) \times 10^{14} \text{ abs. e.s.u.}$$

The adopted value of 96501.2 int. coulombs, on the chemical scale, is 7.2 coulombs higher than the F_{Ag}^* value 96494, which I have advocated from 1929 up till now. On the other hand, the final reduced value $F = 96514.0$ abs. coulombs, on the physical scale, is very close to the value 96513.1 that I used in 1938 (Birge, 1938). The close coincidence is due to two nearly compensating changes: (1) the change of q from 0.99993 to 0.99986, and (2) the change of F , in int. coulombs, from 96494 to 96501.2 .

On the basis of the adopted value of F , the *true* electrochemical equivalents are, by equation (2),

$$E_{\text{Ag}} = (1.11807 \pm 0.00012) \times 10^{-3} \text{ g.abs.-coulombs}^{-1} \text{ (chemical scale), and}$$

$$E_i = (1.31535 - 0.00014) \times 10^{-3} \text{ g.abs.-coulombs}^{-1} \text{ (chemical scale).}$$

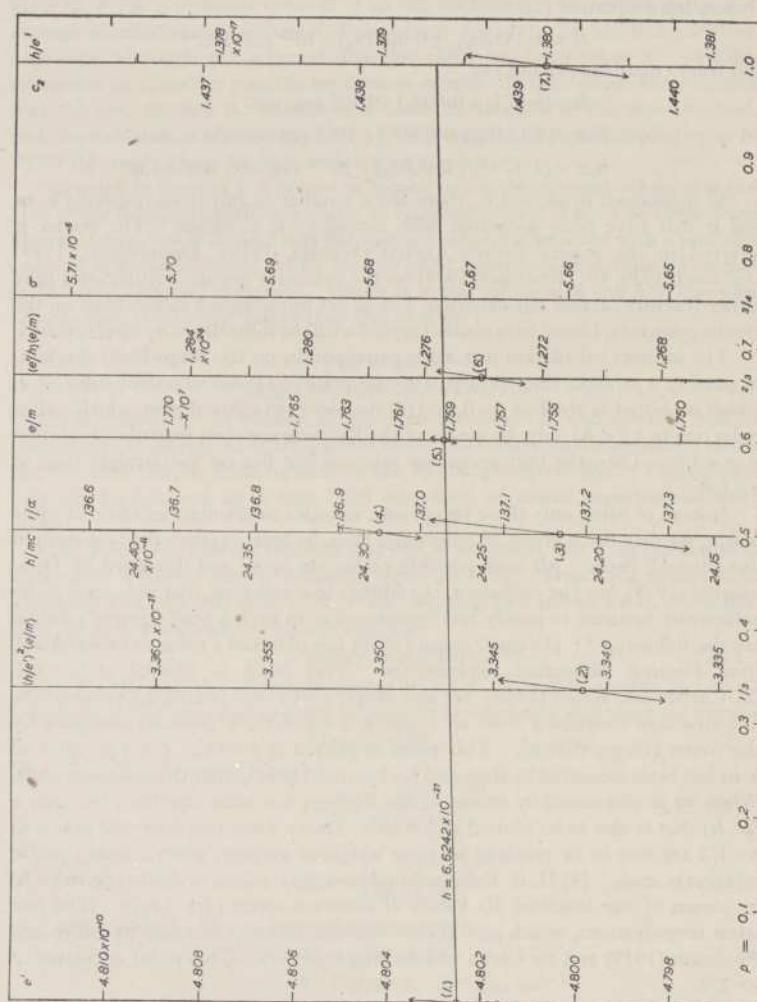


Figure 1. Nomographic chart (Birge-Bond diagram) showing relations between various $f(e, m, h)$. The ordinate scale is so highly magnified (only $\frac{1}{2}\%$ total variation in e') that many less accurately determined $f(e, m, h)$ fall outside the limits shown. The values of the constants adopted in this paper are given by the straight line through points (1) and (5), whose slope corresponds to $h = 6.6242 \times 10^{-34}$. The diagram is based on $h = 6.625$ for a horizontal line.

obtained for the least-squares solution a straight line that almost coincides with that passing exactly through the e and e/m points at $p=0$ and $3/5$. Furthermore, no point deviates from this line by an amount appreciably greater than its apparent probable error. Hence all of the present most accurate data involving functions of e , m and h —with the exception of the direct measurements of h/e —are very consistent, and they collectively yield a value of h/e almost identical with the value just adopted.

When, however, we consider the various direct measurements of h/e the picture is less satisfactory. One can determine h/e from (1) photoelectric effect, (2) ionization and excitation potentials of atoms, (3) the radiation constant $c_2 = (h/e)(F\epsilon/R_0)$, and (4) the Duane-Hunt limit of the continuous x-ray spectrum. A new and interesting method, involving a comparison of the heat of dissociation of iodine measured in cm^{-1} units per atom with that measured in calories per mole, has recently been used by Perlman and Rollefson (1941).

Of these five methods for determining h/e , only the fourth appears to have attained the accuracy necessary for our purpose. Until very recently the most trustworthy work using this method was apparently that by DuMond and Bollman (1937). Their result, in terms of the present values of the auxiliary constants, is $h/e' = (1.3765 \pm 0.0003) \times 10^{-17}$, and the divergence between this result and our indirect value of $(1.3793 \pm 0.0002_s) \times 10^{-17}$ constitutes the fundamental discrepancy already mentioned more than once in this paper. The discrepancy is discussed fully by DuMond and Bollman (1937), in terms of the Birge-Bond diagram.

More recently Ohlin (1941) has found * $h/e' = (1.3785 \pm 0.0005) \times 10^{-17}$. By lowering the pressure in his x-ray tube, he eliminates entirely the troublesome "tail" of the excitation curve, found by all previous investigators. On the other hand, he fails to correct his voltage for the work function of tungsten (about 4.5 volts). The discussion of DuMond and Bollman (1937) certainly indicates the necessity for this correction, and, if it is made, Ohlin's new average result

* Ohlin obtains his stated uncertainty of ± 0.0008 by adding his various estimated errors instead of by taking the square root of the sum of the squares. The use of my auxiliary constants changes his published 1.3787 to 1.3785.

The experimental points shown (each with its adopted probable error indicated by a double arrow) are:

- (1) $e' = (4.8025 \pm 0.0010) \times 10^{-19}$ e.s.u. (adopted value).
- (2) von Friesen (1935), $(e/m)(h/e)^2 = (3.3410_s \pm 0.0037) \times 10^{-27}$ in "mixed units".
- (3) Ross and Kirkpatrick (1934), $h/mc = (24.216 \pm 0.057) \times 10^{-11}$ cm.
- (4) Christy and Keller (1940) and Birge (1939 b), $1/\alpha = 136.95 \pm 0.05$.
- (5) $e/m = (1.7592 \pm 0.0005) \times 10^7$ e.m.u. (adopted value).
- (6) Maya and Robinson (1939) and Clews and Robinson (1940), $(e/m)(e'/h) = (1.2741 \pm 0.0015) \times 10^{24}$ in "mixed" units.
- (7) Ohlin (1941), DuMond, Bailey, Panofsky and Green (1941), and DuMond, Panofsky and Green (1941), $h/e' = (1.3800 \pm 0.0005) \times 10^{-17}$ e.s.u.

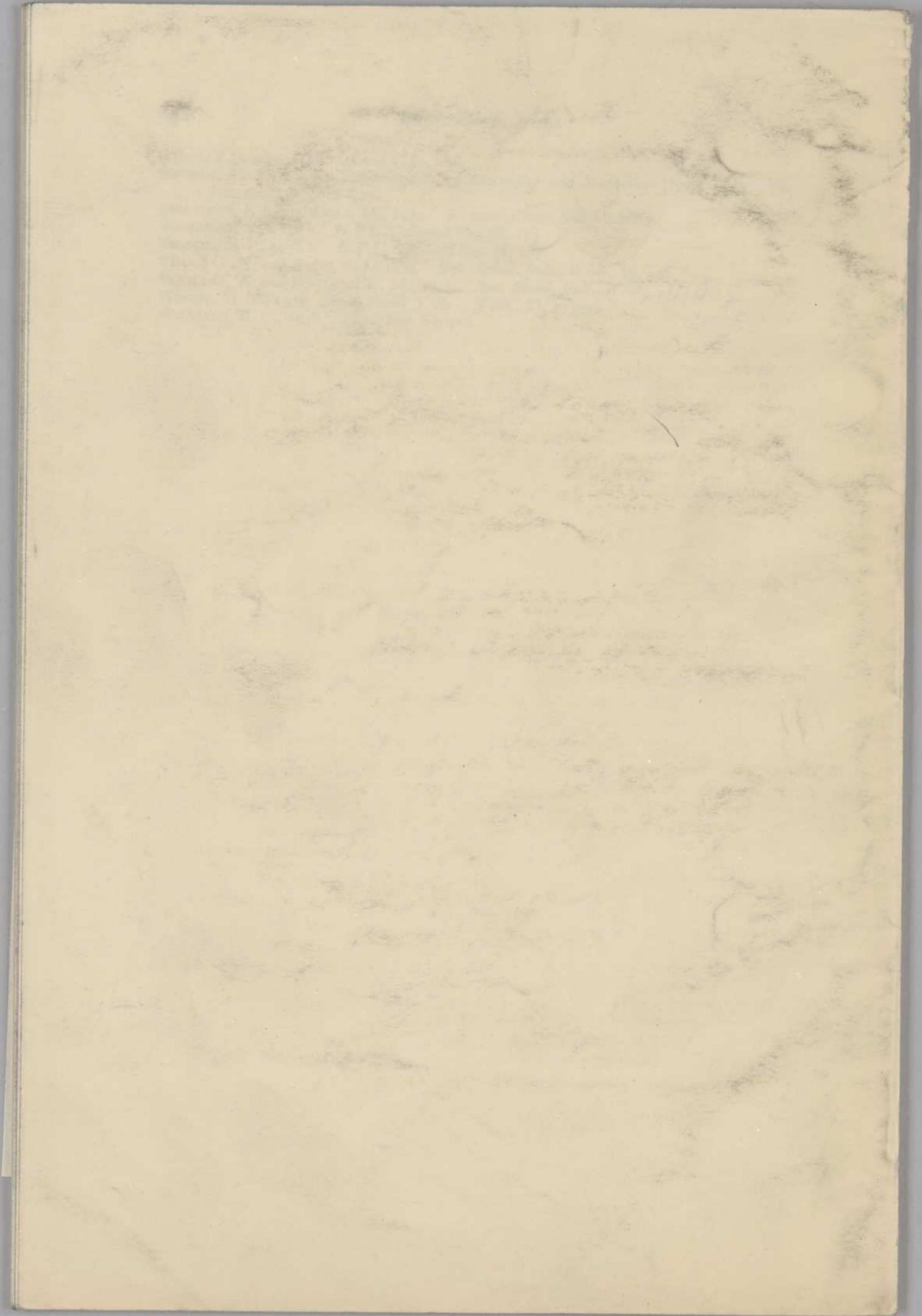
The scales for the radiation constants σ and c_2 are also shown, at $p=\frac{1}{2}$ and 1.0, respectively. The scale for $\mu_1 N_0$ (magnetic moment per mole for one Bohr magneton per molecule—see table c) should be plotted at $p=\frac{1}{2}$, and may be obtained by multiplying the Compton shift (h/mc) scale by $F/4\pi = 2.301754 \times 10^{11}$. The scales for many other $f(e, m, h)$ may similarly be plotted on such a diagram.

- BEATTIE, J. A., 1941. *Temperature (Its Measurement and Control in Science and Industry)* (Reinhold Pub. Co.), New York, pp. 74-89.
- BIRGE, R. T., 1929. *Rev. Mod. Phys.* 1, 1.
- BIRGE, R. T., 1932 a. *Phys. Rev.* 40, 207.
- BIRGE, R. T., 1932 b. *Phys. Rev.* 40, 228.
- BIRGE, R. T., 1932 c. *Phys. Rev.* 42, 736.
- BIRGE, R. T., 1934 a. *Nature, Lond.*, 133, 648.
- BIRGE, R. T., 1934 b. *Nature, Lond.*, 134, 771.
- BIRGE, R. T., 1935. *Phys. Rev.* 48, 918.
- BIRGE, R. T., 1936. *Nature, Lond.*, 137, 187.
- BIRGE, R. T., 1938. *Phys. Rev.* 54, 972.
- BIRGE, R. T., 1939 a. *Amer. Phys. Teacher*, 7, 351.
- BIRGE, R. T., 1939 b. *Phys. Rev.* 55, 1119.
- BIRGE, R. T., 1940 a. *Phys. Rev.* 57, 250.
- BIRGE, R. T., 1940 b. *Phys. Rev.* 58, 658.
- BIRGE, R. T., 1941. *Phys. Rev.* 60, 766.
- BIRGE, R. T. and JENKINS, F. A., 1934. *J. Chem. Phys.* 2, 167.
- BOLLMANN, V. L. and DUMOND, J. W. M., 1938. *Phys. Rev.* 54, 1005.
- BOND, W. N., 1930. *Phil. Mag.* 10, 994.
- BOND, W. N., 1931. *Phil. Mag.* 12, 632.
- DE BRAY, M. E. J. GHEURY, 1927 a. *Astr. Nachr.* 230, 449.
- DE BRAY, M. E. J. GHEURY, 1927 b. *Nature, Lond.*, 120, 404 and 602.
- DE BRAY, M. E. J. GHEURY, 1931. *Nature, Lond.*, 127, 522.
- DE BRAY, M. E. J. GHEURY, 1934. *Nature, Lond.*, 133, 464 and 948.
- CASIMIR, H. B. G., 1939. *Nature, Lond.*, 143, 465.
- CHRISTY, R. F. and KELLER, J. M., 1940. *Phys. Rev.* 58, 658.
- CHU, D. Y., 1939. *Phys. Rev.* 55, 175.
- CLEWS, C. J. B. and ROBINSON, H. R., 1940. *Proc. Roy. Soc. A*, 176, 28.
- CRAGOE, C. S., 1941. *Bur. Stand. J. Res., Wash.*, 26, 495.
- CURTIS, H. L., CURTIS, R. W. and CRITCHFIELD, C. L., 1939. *Bur. Stand. J. Res., Wash.*, 22, 485.
- CURTIS, H. L., MOON, C. and SPARKS, C. M., 1936. *Bur. Stand. J. Res., Wash.*, 16, 1.
- CURTIS, H. L., MOON, C. and SPARKS, C. M., 1938. *Bur. Stand. J. Res., Wash.*, 21, 375.
- DOLE, M. and SLOBOD, R. L., 1940. *J. Amer. Chem. Soc.* 62, 471.
- DRINKWATER, J. W., RICHARDSON, Sir OWEN and WILLIAMS, W. E., 1940. *Proc. Roy. Soc. A*, 174, 164.
- DUMOND, J. W. M., 1939. *Phys. Rev.* 56, 153-64.
- DUMOND, J. W. M., 1940. *Phys. Rev.* 58, 457-66.
- DUMOND, J. W. M. and BOLLMAN, V. L., 1936. *Phys. Rev.* 50, 524.
- DUMOND, J. W. M. and BOLLMAN, V. L., 1937. *Phys. Rev.* 51, 400.
- DUMOND, J. W. M. and BOLLMAN, V. L., 1938. See BOLLMAN and DUMOND, 1938.
- DUMOND, J. W. M., BAILEY, H. H., PANOFSKY, W. K. H. and GREEN, A. E. S., 1941. *Phys. Rev.* 59, 219.
- DUMOND, J. W. M., PANOFSKY, W. K. H. and GREEN, A. E. S., 1941. *Phys. Rev.* 60, 163.
- DUNNINGTON, F. G., 1933. *Phys. Rev.* 43, 404.
- DUNNINGTON, F. G., 1937. *Phys. Rev.* 52, 475.
- DUNNINGTON, F. G., 1939. *Rev. Mod. Phys.* 11, 65-83.
- EDMONDSON, F. K., 1934. *Nature, Lond.*, 133, 759.
- VON FRIESEN, S., 1935. *Dissertation (Uppsala)*.
- GERLACH, W., 1926. *Geiger-Scheel Handbuch der Physik*, 22, 48-82.
- GOEDICKE, E., 1939. *Ann. Phys., Lpz.*, 36, 47.
- GREENE, C. H. and VOSKUYL, R. J., 1939. *J. Amer. Chem. Soc.* 61, 1342.
- GUILLAUME, CH.-ED., 1927. *La Crédation du Bureau international des Poids et Mesures et son Œuvre* (Paris). See p. 258.

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- GUTTON, C., 1911. *C.R. Acad. Sci., Paris*, 152, 1089.
- HAHN, O., FLÜGGE, S. and MATTIAUCH, J., 1940. *Phys. Z.* 41, 1.
- HARTSHORN, L. and ASTBURY, N. F., 1937. *Philos. Trans. A*, 236, 423.
- HENNING, F. and JAEGER, W., 1926 a. *Geiger-Scheel Handbuch der Physik*, 2, 494.
- HENNING, F. and JAEGER, W., 1926 b. *Geiger-Scheel Handbuch der Physik*, 2, 491.
- HEYL, P. R., 1927. *Proc. Nat. Acad. Sci., Wash.*, 13, 601.
- HEYL, P. R., 1930. *Bur. Stand. J. Res., Wash.*, 5, 1243.
- HOPPER, V. D. and LABY, T. H., 1941. *Proc. Roy. Soc. A*, 178, 243.
- HOUSTON, W. V., 1927. *Phys. Rev.* 30, 608.
- HÜTTEL, A., 1940. *Ann. Phys., Lpz.*, 37, 365.
- JAEGER, W. and STEINWEHR, H. V., 1921. *Ann. Phys., Lpz.*, 64, 305.
- JAEGER, W. and STEINWEHR, H. V., 1926. *Z. InstrumKde.* 46, 105.
- KAROLUS, A. and MITTELSTAEDT, O., 1928. *Phys. Z.* 29, 698.
- KINSLER, L. E. and HOUSTON, W. V., 1934. *Phys. Rev.* 46, 533.
- KIRCHNER, F., 1931. *Ann. Phys., Lpz.*, 8, 975.
- KIRCHNER, F., 1932. *Ann. Phys., Lpz.*, 12, 503.
- KIRCHNER, F., 1939. *Ergebn. exakt. Naturw.* 18, 26-77.
- LABY, T. H. and HERCUS, E. O., 1927. *Philos. Trans. A*, 227, 63.
- LABY, T. H. and HERCUS, E. O., 1935. *Proc. Phys. Soc.* 47, 1003.
- MANIAN, S. H., UREY, H. C. and BLEAKNEY, W., 1935. *J. Amer. Chem. Soc.* 56, 2601.
- MAYO, R. L. and ROBINSON, H. R., 1939. *Proc. Roy. Soc. A*, 173, 192.
- MERCIER, J., 1923 a. *Ann. Phys., Paris*, 19, 248.
- MERCIER, J., 1923 b. *Ann. Phys., Paris*, 20, 5.
- MERCIER, J., 1924. *J. Phys. Radium*, 5, 168.
- MICHELSON, A. A., 1927. *Astrophys. J.* 65, 1.
- MICHELSON, A. A., PEASE, F. G. and PEARSON, F., 1935. *Astrophys. J.* 82, 26. Professor Michelson died in 1931, shortly after the measurements were started.
- MILLER, P. H., Jr. and DUMOND, J. W. M., 1940. *Phys. Rev.* 57, 198.
- MITTELSTAEDT, O., 1929 a. *Ann. Phys., Lpz.*, 2, 285.
- MITTELSTAEDT, O., 1929 b. *Phys. Z.* 30, 165.
- MOLES, E., TORAL, T. and ESCRIBANO, A., 1939. *Trans. Faraday Soc.* 35, 1439.
- MURPHY, B. F., 1941. *Phys. Rev.* 59, 320.
- OHLIN, P., 1941. *Dissertation (Uppsala)*.
- OSBORNE, N. S., STIMSON, H. F. and GINNINGS, D. C., 1939. *Bur. Stand. J. Res., Wash.*, 23, 197.
- PERLMAN, M. L. and ROLLEFSON, G. K., 1941. *J. Chem. Phys.* 9, 362.
- PERRY, C. T. and CHAFFEE, E. L., 1930. *Phys. Rev.* 36, 904.
- POINCARÉ, H., 1913. *The Foundations of Science* (New York : Science Press).
- RAMSEY, A. S., 1940. *Nature, Lond.*, 146, 180.
- RAYLEIGH, Lord, 1881 a. *Nature, Lond.*, 24, 382.
- RAYLEIGH, Lord, 1881 b. *Nature, Lond.*, 25, 52.
- RIESENFIELD, E. H. and CHANG, T. L., 1936. *Phys. Z.* 37, 690.
- ROBINSON, C. F., 1939. *Phys. Rev.* 55, 423.
- ROBINSON, H. R., 1938. *Nature, Lond.*, 142, 159.
- ROEBUCK, J. R. and MURRILL, T. A., 1941. *Temperature (Its Measurement and Control in Science and Industry)* (New York, Reinhold Pub. Co.), pp. 60-74.
- ROSA, E. B. and DORSEY, N. E., 1907. *Bur. Stand. Bull., Wash.*, 3, 433-604. See also pp. 605-22.
- ROSS, P. A. and KIRKPATRICK, P., 1934. *Phys. Rev.* 46, 668.
- SCHEEL, K. and BLANKENSTEIN, F., 1925. *Z. Phys.* 31, 202.
- SHANE, C. D. and SPEDDING, F. H., 1935. *Phys. Rev.* 47, 33.
- SHAW, A. E., 1938. *Phys. Rev.* 54, 193.
- SMYTHE, W. R., 1934. *Phys. Rev.* 45, 299.

- STOTT, V., 1929. *Nature, Lond.*, **124**, 622.
SUYDAM, V. A., 1940. *Fundamentals of Electricity and Magnetism* (New York: Van Nostrand), pp. 616-627.
SWARTOUT, J. A. and DOLE, M., 1939. *J. Amer. Chem. Soc.* **61**, 2025.
VIGOREUX, P., 1938 a. *N.P.L. Collected Res.* **24**, 173.
VIGOREUX, P., 1938 b. *N.P.L. Collected Res.* **24**, 277.
VINAL, G. W. and BATES, S. J., 1914. *Bur. Stand. Bull., Wash.*, **10**, 425.
VINAL, G. W. and BOUARD, W. M., 1916. *Bur. Stand. Bull., Wash.*, **13**, 147.
WENSEL, H. T., 1939. *Bur. Stand. J. Res., Wash.*, **22**, 375-96.
WILLIAMS, R. C., 1938. *Phys. Rev.* **54**, 568.



$$c = 2.9978 \times 10^{10} \frac{\text{cm}}{\text{sec}} = 3.0000 - 0.0022 = 3.0000(1 - 0.0007) = 3.0000(1 - 7 \times 10^{-4}) \frac{\text{cm}}{\text{sec}}$$

$$T_0 = 273.16^\circ K$$

$$\theta_{16} = 16.0000, \theta_{16}: \theta_{15}: \theta_{14} = 506:1:0.204, \theta = 16.00436, r = \frac{\theta}{16} = 1.00027 = 1 + 2.7 \times 10^{-4}$$

$$1 \text{ atm} = 1.0132 \times 10^6 \frac{\text{dyn}}{\text{cm}^2} \quad V_0 = 22.415 \text{ liter}$$

$$F = 96500 \text{ coul. (chem scale 96488 phys. scale 96314 diff. 26)} = 2.7 \times 10^{-4}$$

$$N_0 = 6.0236 \times 10^{23} \quad (6.0228 \times 10^{23} \text{ chem sc. } 6.0244 \times 10^{23} \text{ phys sc. diff. 1.62} = 2.7 \times 10^{-4})$$

$$e = 1.60205 \times 10^{-19} \text{ e.m.u.} = 4.8025 \times 10^{-10} \text{ e.s.u.}$$

$$\frac{e}{m} = 1.7592 \times 10^7 \quad " \quad - 3.2736 \times 10^17 \quad "$$

$$h = 6.624 \times 10^{-27}$$

$$k = 1.3805 \times 10^{-16} \quad R = 8.314 \times 10^7$$

$$\mu N_0 = 5585 \quad \mu = 0.927 \times 10^{-20} \quad \frac{M}{m} = 1837$$

$$c = 3 \times 10^{10} (1 - 0.7 \times 10^{-3}) \frac{\text{cm}}{\text{sec}}, T = 273.16^\circ K, F = 96.500 \text{ coul} \quad R = 8.314 \times 10^7$$

$$N_0 = 6.024 \times 10^{23} \quad e = 4.8025 \times 10^{-10} \quad h = 6.624 \times 10^{-27} \quad k = 1.3805 \times 10^{-16} \quad \frac{M}{m} = 1837$$

$$1 \text{ atm} = 1.0132 \times 10^6 \frac{\text{dyn}}{\text{cm}^2} \quad V_0 = 22.415 \text{ l.} \quad \frac{e}{m} = 5.274 \times 10^{17} \quad \mu = 0.927 \times 10^{-20} \cdot N_0 = 5585$$

$$2\pi Cr, r = R = 200 \frac{\text{m}}{\text{sec}} = 2 \times 10^4 \frac{\text{cm}}{\text{sec}}$$

$$\frac{k}{2} m c^2 = kT, \alpha = \sqrt{\frac{2kT}{m}} = 1.29 \times 10^4 \sqrt{\frac{T}{m}} \quad T=450, M=133 \quad \sqrt{\frac{T}{m}} = \sqrt{\frac{9.3}{2.4}} = 1.5 \sqrt{1.5} = \frac{1.29474}{1.83711} = 1.837$$

$$\alpha = 1.29 \times 1.837 \times 10^4 = 2.37 \times 10^4 \frac{\text{cm}}{\text{sec}} \quad 3.3_m; C = \frac{\alpha}{\sqrt{3}} = \frac{2.37 \times 10^4}{\sqrt{3}} = 1.367 \times 10^4 \frac{\text{cm}}{\text{sec}}, 3.3_x = 3 \times 0.174 = 0.522, 1.4 = 0.1308$$

$$T=64, M=4, \sqrt{\frac{T}{m}}=4, \alpha = 5.16 \times 10^4 \frac{\text{cm}}{\text{sec}}, T=100, \alpha = 6.45 \times 10^4 \frac{\text{cm}}{\text{sec}}$$

$$r = 2 \text{ cm}, 2\pi r t = 4\pi = 10.5664 \quad r \cdot \frac{2 \times 10^4}{12.57} = 1.59 \times 10^3 \frac{\text{cm}}{\text{sec}} = 9.55 \times 10^4 = 95,500 \frac{\text{cm}}{\text{min}}$$

$$t = \frac{l}{c} \quad s \cdot v t = l \quad c = v \frac{l}{s}, \frac{dc}{ds} = -v \frac{l}{s^2} = -\frac{c}{s}, \left| \frac{dc}{c} \right| = \left| \frac{ds}{s} \right| = \frac{l}{s}$$

$$\frac{l}{s} = 10^2 \quad l = 10^5 \text{ cm}, s = 10 \text{ cm} \quad l = 10 \text{ cm} \quad v = c \quad l = 100 \text{ cm} \quad v = \frac{l}{10} c$$

$$dn = q e^{-\frac{E}{kT}} \left[\frac{g}{\alpha} \right]^3 d\frac{c}{\alpha}, \Delta n = q e^{-\frac{E}{kT}} C^3 dC, \frac{d}{dc} = \frac{\Delta n}{AC} = q e^{-\frac{E}{kT}} C^3, \frac{d^2}{dc^2} = q \left(e^{-\frac{E}{kT}} 3C^2 - \frac{3}{\alpha^2} e^{-\frac{E}{kT}} C^3 \right)$$

$$\frac{d^3}{dc^3} = q' e^{-\frac{E}{kT}} C^3 \left(\frac{3}{C} - \frac{2C}{\alpha^2} \right) = \frac{q'}{C} \left(3 - \frac{2C^2}{\alpha^2} \right), \frac{d^4}{dc^4} = \frac{dc}{C} \left(3 - \frac{2C^2}{\alpha^2} \right)$$

$$mv^2r = \frac{h}{2\pi c}, \frac{mv^2}{r} = \frac{e^2}{r^2}, mv^2r = e^2, v = \frac{e^2 2\pi c}{h}, r = \frac{h}{2\pi m v} = \frac{h \cdot h}{2\pi m 2\pi c^2 (\frac{h}{2\pi c})^2 \frac{1}{m}}$$

$$r = \left(\frac{h}{2\pi c}\right)^2 \frac{N_0}{M} = \left(\frac{6.6 \times 10^{-27}}{6.3 \times 4.8 \times 10^{-10}}\right)^2 \frac{6 \times 10^{23}}{1837} = (2.18 \times 10^{-16})^2 \frac{6 \times 10^{23}}{4.837} = 4.76 \times 10^{-36} \times 1.1 \times 10^{26} = 0.523 \times 10^{-8} \text{ cm}$$

$$\alpha = \frac{2\pi e^2}{hc} = \frac{v}{c} \quad v = \frac{2\pi c e^2}{h} = \frac{6.2832 \times 4.8025 \times 10^{-20}}{6.6242 \times 10^{-34}} = 2.183 \times 10^8 \frac{\text{cm}}{\text{sec}}$$

$$(4.8 + 0.0025)^2 = 4.8^2 + 0.005 \times 4.8 = \frac{23.0400}{23.064} \quad \frac{v}{c} = \frac{2.183 \times 10^8}{2.998 \times 10^10} = 7.30 \times 10^{-3} \frac{1}{\alpha} = 134.0$$

$$r = \left(\frac{h}{2\pi c}\right)^2 \frac{1}{m} = \left(\frac{6.6242 \times 10^{-34}}{6.2832 \times 4.8025 \times 10^{-10}}\right)^2 \frac{1}{9.1066 \times 10^{-28}} = \frac{2.1953^2 \times 10^{-36}}{9.1066 \times 10^{-28}} = \frac{4.8194 \times 10^{-8}}{9.1066} = 0.5292 \times 10^9 \text{ cm}$$

$$\begin{array}{rcl} 6.6242 : 6.2832 = 1.05428 & \frac{h}{2\pi} = 1.0543 \times 10^{-24} & 4.8194 \cdot 9.1066 = 0.5292 \\ 6.2832 & & 4.5533 \\ 0.34100 & & \\ 0.31416 & \frac{1.0543}{4.8025} = \frac{1}{4.8025} (1 + 0.0543) = \frac{0.20822}{0.80045} & 0.26610 \\ 0.02689 & & 0.18213 \\ & & 0.08394 \\ & & 0.08196 \\ & & 201 \end{array}$$

in Mc O Stern

$$eV = \times 1.601 \times 10^{-12} \text{ erg} \quad h = 1.3805 \times 10^{-16} \frac{\text{erg}}{T}, k = 0.8625 \times 10^{-4} \frac{\text{eV}}{T}$$

$$1.3805 : 1.601 \times 10^{-12} = 0.8625$$

$$\begin{array}{r} 1.3805 \\ - 1.601 \times 10^{-12} \\ \hline 0.71900 \\ 0.0960 \\ \hline 0.0040 \\ 32 \end{array}$$

$$\begin{array}{r} 1.4250 \\ - 1.4825 \\ \hline 25875 \\ 2524 \\ \hline \end{array}$$

$$T = 293 \quad kT = 0.253 \times 10^{-2} \text{ eV}$$

$$T = 290 \quad kT = 0.250 \times 10^{-2} = 0.0250 = \frac{1}{40} \text{ eV}$$

$$\begin{array}{r} 580 \\ - 273 \\ \hline 307 \end{array}$$

$$C = 3 \times 10^{10} (1 - 0.4 \times 10^{-3}) \quad F = 1 \times 10^5 (1 - 36.3 \times 10^{-3}) \quad R = 8.3 \times 10^7 (1 + 1.7 \times 10^{-3})$$

$$N_0 = 6 \times 10^{23} (1 + 4.0 \times 10^{-3}) \quad h = 6.6 \times 10^{-24} (1 + 3.6 \times 10^{-3}) \quad e = 4.8 \times 10^{-10} (1 + 0.5 \times 10^{-3}) \quad \text{eV} = 1.6 \times 10^{-12} (1 + 0.5 \times 10^{-3})$$

$$k = 1.38 \times 10^{-16} (1 + 0.4 \times 10^{-3}) = 1.4 \times 10^{-16} (1 - 14.1 \times 10^{-3})$$

13 15 east 634 golden triangle 63

bed + g M61 overl. Km 8' 15" m on the 15