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

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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.

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 ± 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 (c)

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 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 $1·76 \pm 0·02$ 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 λ5700, 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

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* 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 A. and 5000 A. as reasonable values for the effective wave-lengths of the light used, the expected difference in velocity is 1·4 per cent on the basis of the wave index, but 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

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* 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 λ 5500 A., 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) × 299776 = 121·11 km./sec., according to Mittelstaedt, but is correctly (107·5/250000) × 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, sa 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 λ5500 A., or 2·67 km./sec. for λ 5700 A.). 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*.

The first method is the measurement of the velocity of electromagnetic waves, guided by parallel wires. This method assumes merely that visible light waves and electromagnetic waves have the same velocity in vacuum. The only real precision work with this method is that by Mercier (1923 a–24). In the case of parallel wires large corrections are required to reduce the observed velocity to the velocity of free waves in air. An elementary account of the underlying theory is given in a recent text by Suydam (1940). Mercier's final average—and the only final result that he gives—is 299700 km./sec. I quoted this figure in G.C. 1929 as the resulting velocity in vacuum. Actually, however, it is the velocity in *air*, and when multiplied by the square root of the dielectric constant of air ($\epsilon = 1.000545$ at 20° c. and one atmosphere) one gets 299782 km./ sec. as Mercier's true value for vacuum.

Although the calculated corrections just mentioned vary from 130 to 366 km./sec., for Mercier's five listed results, the corrected results themselves show a maximum deviation from the average of only 30 km./sec., thus indicating the essential correctness of the theory. Mercier takes the 30 km./sec. deviation as his " limit of error ", but because he has thus made no allowance for systematic errors of either theory or experiment, I will assume 30 km./sec. as his *probable error*, just as I did in 1934 (Birge, 1934 b).

The second indirect value of *c* is that given by the ratio of an electric charge measured in e.s.u. to the same charge measured in e.m.u. This relation follows directly from the electromagnetic theory of light. By far the finest piece of work on this subject is that in 1906 by Rosa and Dorsey (1907). Their work was discussed in G.C. 1929 (pp. 9–10), and the discussion will not, in general, be repeated. Several small revisions are, however, needed. Their actual average result* is 2.99712×10^{10} cm.$^{1/2}$ sec.$^{-1/2}$ int.-ohm$^{1/2}$, rather than the rounded 2.9971 which they use. Furthermore, the best value of p, as stated in Section E ahead, is now 1.00048 ± 0.00002, in place of the value 1.00051 ± 0.00002 used in 1929. Their final result thus becomes $(1.00048)^{1/2} \times (2.99712 \times 10^{10}) = 2.99784 \times 10^{10}$ cm./sec., or 299784 km./sec., for the value of *c*.† I again take 10 km./sec. as the probable error, and I again wish to emphasize that this work by Rosa and Dorsey is one of the most beautifully executed pieces of precision research in the entire history of science, and constituted certainly the most precise determination of *c* up till the last few years.‡

We have now obtained eight observed values of *c*, and these are the *only* data

* In specifying the unit on page 10, and again on page 60, of G.C. 1929, " int.-ohm$^{-1/2}$ " was inadvertently printed, in place of the correct " int.-ohm$^{1/2}$."

† By starting with their actual data for condensers in air (Rosa and Dorsey, 1907, p. 601), and using a slightly more accurate value of the dielectric constant of air, I get for the final reduced value 299783·2 km./sec., but it has not seemed worth while to make the trifling, but extensive, revisions that would be required by this small change in the adopted value.

‡ Actually all that Rosa and Dorsey could measure, as already stated, was *c* in cm.$^{1/2}$ sec.$^{-1/2}$ int.-ohm$^{1/2}$ units, and until p had been correctly evaluated—which was not done until many years later—no final reliable value of *c* in cm.sec.$^{-1}$ units could be deduced. But this fact does not detract from the *precision* of their work.

·worthy of consideration in obtaining a final most probable value. But in order to consider de Bray's hypothesis of the time variation of *c*, I list in table 1 also the five earlier " final declared values ", as given by de Bray (1927). Merely for the sake of argument I adopt also the stated probable error of the investigator himself, as given by de Bray, although as a result of the evidence uncovered in the recent investigations, it is possible that all of the assumed probable errors of these five earlier investigations are too small. We thus have 13 values of *c*. Table 1 gives also the mean epoch at which the work was performed and the actual published result. Another column gives the method employed, and another the adopted weight, taken, as usual, proportional to the reciprocal of the square of the probable error.

Table 1. Velocity of light

Author	Method	Epoch	Corrected result	Adopted probable error r	Adopted weight $100/r^2$	Original published result
Cornu-Helmert	TW	1874·8	299990	200	0·0025	299990
Michelson	RM	1879·5	299910	50	0·0400	299910
Newcomb	RM	1882·7	299860	30	0·1111	299860
Michelson	RM	1882·8	299853	60	0·0278	299853
Perrotin	TW	1902·4	299901	84	0·0142	299901
Rosa-Dorsey	EU	1906·0	299784	10	1·000	299710
Mercier	WW	1923·0	299782	30	0·111	299700
Michelson	RM	1926·5	299798	15	0·444	299796
Mittelstaedt	KC	1928·0	299786	10	1·000	299778
Michelson, Pease and Pearson	RM	1932·5	299774	4	6·250	299774
Anderson	KC	1936·8	299771	10	1·000	299764
Hüttel	KC	1937·0	299771	10	1·000	299768
Anderson	KC	1940·0	299776	6	2·778	299776

TW = toothed wheel ; RM = rotating mirror ; EU = electric units ;
WW = waves on wires ; KC = Kerr cell.

Using the data of the table, one now obtains the following results. The weighted average of the eight recent observations is $299776\cdot4_{37} \pm 1\cdot4_{76}(\pm 2\cdot7_1)$ km./sec. In this, and in subsequent expressions, the first stated probable error is that obtained on the basis of external consistency (r_e), and the second (in parenthesis) that given by internal consistency (r_i) (Birge, 1932 a). The ratio r_e/r_i is 0·544; in other words, the actual consistency of the results is about twice as great as is to be expected on the basis of the assigned probable errors. One may interpret this as the result of pure chance, or as an indication that the assigned probable errors are too large. I am inclined to believe that it is mostly the latter. On the other hand it seems best always to accept the larger of the two errors, and in order to allow for still unknown systematic errors, I shall adopt 4 km./sec. as the probable error. The final average, which I here *adopt*, is then

$$c = 299776 \pm 4 \text{ km./sec.}$$

This value is identical with that advocated in 1934 (Birge, 1934 b), and because

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 $299873 \cdot 8_8 \pm 8 \cdot 6_8 (\pm 22 \cdot 6_1)$ 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./see. 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, $299777 \cdot 8_2 \pm 2 \cdot 5_{78}$ ($\pm 2 \cdot 6_{94}$) 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

$$c = 299779 \cdot 18 \pm 1 \cdot 97 - (0 \cdot 901 \pm 0 \cdot 192)\ (D - 1930)\ \text{km./sec.}$$

The ratio $r_e/r_i = 0 \cdot 723$, 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

$$c = 299777 \cdot 27 \pm 1 \cdot 37 - (0 \cdot 381 \pm 0 \cdot 158)(D - 1930)\ \text{km./sec.}$$

and $r_e/r_i = 0 \cdot 490$. Hence the probable error of the slope, by internal consistency, is $\pm 0 \cdot 323$ 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 $6·670_3 \pm 0·002_7$, 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 $G . \delta$ (earth) $= 36·797 \times 10^{-8}$ sec.$^{-2}$, where δ (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).

litre/1000, but because of the frequent confusion with cm³, to which it is *not* equal, the " Joint Committee for the Standardization of Scientific Glassware " recommended, in 1924, that the designation *ml.* (millilitre) be used in place of c.c. A very clear and detailed account of the entire situation has been given by Stott (1929). Unfortunately I had not been aware of the 1924 action until I read Stott's paper. Hence in G.C. 1929 I used the designation c.c. in place of ml.

As a result of the discovery of the isotopes of both hydrogen and oxygen, there is now a wide variety of different possible kinds of " water ". This situation, as it affects the litre, has been discussed by Riesenfeld and Chang (1936). Fortunately the density of " ordinary water " appears to be very uniform, although Riesenfeld and Chang say " it is now known that the litre determined from ocean water is 0·0015 cm³ smaller than that from land water ". This difference is due to a change in the abundance ratio of the oxygen isotopes, a subject discussed in Section F.

The relation adopted in G.C. 1929 was, 1 litre $= 1000 \cdot 027 \pm 0 \cdot 001$ cm³. Since then this figure has been corrected by Guillaume (1927) to 1000·028, and because of the variation just mentioned in regard to land water and ocean water, I increase its probable error to 0·002. Hence I *adopt*

$$1 \text{ litre } (= 1000 \text{ ml.}) = 1000 \cdot 028 \pm 0 \cdot 002 \text{ cm}^3$$
$$= 1 \cdot 000028 \pm 0 \cdot 000002 \text{ dm}^3$$

The maximum density of water $\delta_m(H_2O)$ is accordingly

$$1/1 \cdot 000028 = 0 \cdot 999972 \pm 0 \cdot 000002 \text{ g.cm.}^{-3}$$

Since there is still some confusion on the matter, it may be well to emphasize that the litre and millilitre are *not* units of the present c.g.s. system, whose unit of volume is the *cm³.* They are units of volume in a system in which density (of water) replaces length as a fundamental unit.

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SECTION D

THE NORMAL MOLE VOLUME (N.M.V.) OF AN IDEAL GAS

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The normal mole volume of an ideal gas is the volume occupied by one gram mole at 0° c. under one atmosphere pressure. A brief account of the theory was given in G.C. 1929, and a much more detailed account later (Birge and Jenkins, 1934). The theory will not be repeated, but the basic equations will be given in order to make clear a number of changes in symbols.*

For an ideal gas we have

$$pV = mR_0T_0/M, \qquad \ldots\ldots(1)$$

where p = pressure, V = volume, m = mass in grams, M = gram molecular weight, T_0 = absolute temperature of the ice-point (0° c.), and R_0 = gas constant per mole. If $m/M = 1$ and $p = 1$, we get

$$V = V_0 = R_0T_0, \qquad \ldots\ldots(2)$$

* This is a subject in which there is no standard set of symbols. In particular, workers in the field of limiting gas densities often use quite different symbols from those usually employed in the general field of physical chemistry of gases.

where V_0 is the normal mole volume (N.M.V.) in the units chosen. Since the N.M.V. is thus merely the product $R_0 T_0$, its dimensions by equation (1) are those of *pressure* × *volume*, for $m/M = 1$ (one mole of gas).* There are consequently many units for N.M.V., depending on the units of pressure and of volume that are chosen. It is customary to choose one atmosphere as the unit of pressure, but the atmosphere (see Section G) may be in terms of *standard* gravity ($g_0 = 980 \cdot 665$ cm.sec.$^{-2}$), or of $45°$ gravity ($g_{45} = 980 \cdot 616$ cm.sec.$^{-2}$). It is customary for workers in the field of limiting gas densities to express all experimental results in terms of g_{45}, and for convenience in quoting their work I shall do the same. The corresponding value of N.M.V. will be denoted V_{45}.

A real gas satisfies equation (1) only at zero pressure. Hence, from equations (1) and (2),

$$(pV)_0 = mV_0/M, \qquad \qquad \ldots\ldots(3)$$

an equation holding for a real gas, where $(pV)_0$ is the value of pV at zero pressure Let us now *define*

$$(pV)_0/(pV)_1 = 1 - \alpha, \qquad \qquad \ldots\ldots(4)$$

where $(pV)_1$ is the value of pV for one atmosphere pressure.† Let us also *define*

$$L = m/pV, \qquad L_1 = m/(pV)_1, \qquad L_{\text{lim}} = m/(pV)_0. \quad \ldots\ldots(5)$$

Since $m/V = \rho =$ density, we have $L = \rho/p$ and $L = L_1 = \rho$ for $p = 1$. Thus L_1 is the actual density at one atmosphere, and L_{lim} is the density the gas would have, at one atmosphere, if it were an ideal gas. Both L and pV vary with p, for a real gas, and L_{lim} and $(pV)_0$ are the limiting values (obtained by extrapolation) at $p = o$.

From equations (4) and (5)

$$L_1/L_{\text{lim}} = (pV)_0/(pV)_1 = 1 - \alpha. \qquad \qquad \ldots\ldots(6)$$

Then from equations (3), (5) and (6)

$$V_0 \text{ (or } V_{45}) = M/L_{\text{lim}} = M(1 - \alpha)/L_1 \qquad \qquad \ldots\ldots(7)$$

This last equation is used in the actual evaluation of V_0. The values of $(1 - \alpha)$

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* There is a more or less unavoidable ambiguity in the proper treatment of normal mole volume. If V_0 is considered as the actual volume per mole, its dimensions are necessarily those of volume. But the volume of a gas depends on the pressure, and when pressure is introduced, as in equation (1), we get V_0 *defined* by equation (2), from which its dimensions are necessarily those of *pressure* × *volume*, or *energy* (per mole). It merely happens that if energy is expressed in litre-atmos, or cm.^{3}-atmos units, then the *numerical* value of the energy per mole, for one atmosphere pressure, is just the *volume* (in litres or in cm.^{3}) per mole—i.e. the so-called normal mole volume. In G.C. 1929 I followed the example of the *International Critical Tables*, 1, 17 (1926), and specified the unit of V_0 as volume per mole. But if V_0 ($= R_0 T_0$) is to be used to calculate R_0 in various units, as is done in table c, at the end of the paper, then V_0 *must* be given the dimensions of energy. The actual calculations of R_0 in table c indicate the particular symbols to be used in equation (2), for certain specified units. Thus, from table c, $V_0 = R_0''' T_0$ and $V_0' = R_0'' T_0$. Also, from the specific definition of R_0 in table c (in contrast to R_0', R_0'' etc.), we get, in table c, $V_0 A_0 = R_0 T_0$, as a measure of the actual pressure energy in *ergs* per mole, for an ideal gas at $0°$ c., under one atmosphere pressure (see Section G for A_0).

† The symbol $1 + \lambda$ is often used in place of $1 - a$.

and L_1 (hence of L_{lim}) may be obtained in the same investigation. Sometimes $(1-\alpha)$, which by equation (4) requires a measurement only of the pV isothermals and not of the density, is measured in one investigation, and L_1 in another investigation. In order to evaluate V_0, oxygen is always used as the gas, since then $M=32$, without any experimental error. Knowing V_0, one can obtain the molecular weight M of any other gas from its observed L_{lim}.

It has been customary, in limiting gas-density work for the determination of molecular weights, to confine the observations to pressures of one atmosphere and less. For that range of pressure it is known that oxygen and other so-called permanent gases yield an essentially linear relation between L and p (and also between pv and p). But Cragoe (1941), in an exhaustive investigation, has shown that even in the case of a permanent gas, a much more reliable value of $1-\alpha$ can be obtained by the use also of data at higher pressures. For the range 0–100 atmospheres, Cragoe finds as the best least squares solution of the pV isothermals of oxygen

$$\frac{pV}{(pV)_1} = 1-(95\cdot13 \pm 0\cdot94) \times 10^{-5}\left(\frac{V_1}{V}-1\right)+2\cdot246 \times 10^{-6}\left(\frac{V_1}{V}-1\right)^2, \quad \ldots\ldots(8)$$

where V_1 is the volume at one atmosphere pressure. If, now, we put $V=\infty$ (i. e., $p=0$) in equation (8), and use equation (6), we get*

$$(pV)_0/(pV)_1 = 1-\alpha = 1\cdot0009535, \qquad \ldots\ldots(9)$$

The stated uncertainty ($\pm 0\cdot94$) in equation (8) is not the least squares probable error, but a quantity about three times as large, designed to allow for systematic errors. I shall accept this estimate, and accordingly *adopt* $1-\alpha = 1\cdot000953_5 \pm 0\cdot000009_4$. In G.C. 1929 I adopted Baxter and Starkweather's least squares solution (Baxter and Starkweather, 1928) of their own data on oxygen (Baxter and Starkweather, 1926) at one atmosphere and less. This solution gave $1-\alpha = 1\cdot000927 \pm 0\cdot000030$, and $L_1 = 1\cdot428965 \pm 0\cdot000030$ gram.litre^{-1} ($g=980\cdot616$).

Their *observed* value of L_1 is, however, $1\cdot42897 \pm 0\cdot00003$, and if an *independent* value of $1-\alpha$, like Cragoe's, is to be adopted, only directly observed values of L_1 are relevant. I shall here *adopt* the value of L_1 (and its error) just quoted, and with the adopted $1-\alpha$ of Cragoe, we get $L_{lim} = L_1/(1-\alpha) = 1\cdot427609 \pm 0\cdot000037$, as contrasted with $L_{lim} = 1\cdot427641 \pm 0\cdot000055$ given by the G.C. 1929 values.

A proper consideration of all relevant data now available would require a very extended discussion. I shall, however, content myself with the following brief, categorical remarks. Batuecas (1935) has recalculated all of his data by least squares methods, as outlined and advocated by Birge and Jenkins (1934). His final result for oxygen is $L_{lim} = 1\cdot42762 \pm 0\cdot00005$. Moles, Toral and Escribano (1939), in a paper that is more accessible than most of the original references, obtain $L_{lim} = 1\cdot427619$. I believe that these are the best values of L_{lim} that have been obtained since 1929, and they deviate from our present

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* Cragoe incorrectly deduces $1-\alpha = 1\cdot0009513$ from his equation.

adopted L_{lim} only by about one quarter of its probable error. They are also closer to it than to the value adopted in G.C. 1929.

Finally, with the adopted L_{lim} we get, for the units used,

$$V'_{45} = M/L_{\text{lim}} = 32/(1\cdot427609 \pm 0\cdot000037) =$$
$$22\cdot4151_0 \pm 0\cdot0006 \text{ litre.atmos.mole}^{-1} \ (g = 980\cdot616),$$

which I *adopt*. This may be contrasted with $22\cdot4146 \pm 0\cdot0008$ of G.C. 1929, and with the value $22\cdot4149$ given by the L_{lim} of either Batuecas (1935) or Moles *et al.* (1939).

To convert to cm^3 units, we multiply by $1000\cdot028$ (see Section C), and to convert to standard gravity ($980\cdot665$) we multiply by the inverse ratio of the accelerations. Thus we get

$$V_{45} = (22\cdot4157 \pm 0\cdot0006) \times 10^3 \text{ cm.atmos.mole}^{-1} \ (g = 980\cdot616),$$
$$V_0' = 22\cdot4140 \pm 0\cdot0006 \text{ litre.atmos.mole}^{-1} \ (g = 980\cdot665),$$
$$V_0 = (22\cdot4146 \pm 0\cdot0006) \times 10^3 \text{ cm}^3\text{.atmos.mole}^{-1} \ (g = 980\cdot665).$$

It seems preferable to apply the expression " normal mole volume " to V_0 and V_0', rather than V_{45} and V'_{45}.

SECTION E
RATIO OF INTERNATIONAL TO ABSOLUTE ELECTRICAL UNITS

All electrical measurements are still based on the international ampere, defined by means of the silver voltameter, and the international ohm, defined by the resistance of a column of mercury. It had been hoped to adopt, on January 1, 1940, the *absolute* electrical units as the official units. Effectively such action can be taken by merely accepting official values for p and q, where one int. ohm $= p$ abs. ohm, and one int. amp. $= q$ abs. amp.

In preparation for such action, all the national physical laboratories undertook, some years ago, to obtain new and more trustworthy values of p and q. Furthermore, there have now been carried out extensive intercomparisons of the standard resistances and standard cells of the various countries, and from such comparisons a " mean international ampere " and a " mean international ohm " have been defined. Any one laboratory can thus convert its own national standards into these international standards.

Because of the international situation, it has been impossible for all the laboratories to complete the work on p and q, and accordingly no attempt has yet been made to adopt official values. But both the National Bureau of Standards, Washington (N.B.S.), and the National Physical Laboratory (N.P.L.) have completed the necessary investigations, with results that are in beautiful agreement, although the value of q obtained by each laboratory is appreciably different from the value considered most reliable in 1929. The actual results are, very briefly, as follows:—

(1) Curtis, Curtis and Critchfield (1939) obtain $q = 0\cdot99986$ in terms of the N.B.S. int. amp., which becomes $0\cdot999868$ in terms of the mean int. amp.

(2) Vigoureux (1938 a) gets $q = 0.99986$ in terms of the N.P.L. int. amp., which becomes 0.99985 in terms of the mean int. amp.   In both of these investigations. an attempt was made to get a result trustworthy to about 2 parts in 10^5.   From these two results I get as an *adopted* value, $q = 0.99986 \pm 0.00002$, in terms of the mean int. amp.

In the case of p the results are as follows: (1) Curtis, Moon and Sparks (1938) obtain $p = 1.000468$ in terms of the N.B.S. int. ohm, or 1.000472 in terms of the mean int. ohm; this result is the weighted average of the results of two investigations by these authors (1936, 38).   (2) Two investigations at the N.P.L., one by Hartshorn and Astbury (1937), and the other by Vigoureux (1938 b), each give $p = 1.00050$ in terms of the N.P.L. int. ohm, or 1.000508 in terms of the mean int. ohm.   A slightly higher accuracy is claimed for the N.B.S. work, and I accordingly adopt as a final weighted average $p = 1.00048 \pm 0.00002$, in terms of the mean int. ohm.

This adopted value of p agrees fairly well with the value 1.00051 ± 0.00002 adopted in G.C. 1929.   But the new value of $q = 0.99986 \pm 0.00002$ deviates considerably both from the value 0.99995 ± 0.00005 adopted in G.C. 1929, and 0.99993 ± 0.00002 adopted in 1938 (Birge, 1938).   This change in the value of q makes many small changes in other constants, but further appreciable changes. in either p or q are unlikely to occur for some time.

The relations between the international and absolute units of other electrical. and magnetic quantities are listed in table b, at the end of this paper.

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SECTION F

THE ATOMIC WEIGHTS OF CERTAIN ELEMENTS

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The situation in regard to the atomic weights of many elements has been completely changed since G.C. 1929 was written.   In the first place, the discovery of isotopes of oxygen (^{17}O and ^{18}O) has made possible two different scales of atomic weights: (1) the physical scale, based on $^{16}O = 16.0000$, and (2), the chemical scale, based on the ordinary mixture of oxygen isotopes $O = 16.0000$. In the second place, atomic weights of many individual isotopes, on the physical scale, can now be measured by means of the mass spectrograph with an accuracy equalling, or even exceeding, any chemical determination of atomic weight. Hence, in the case of certain elements such as H, He, C and N, the *most accurate* determination of the atomic weight, on the chemical scale, of the ordinary mixture of isotopes (in other words, the ordinary atomic weight of chemistry) can be made by starting with the mass spectrograph values of the individual isotopes.

In order to carry out such a calculation one must know (1) the mass of each isotope, with its probable error, on the physical scale, as determined with the mass spectrograph; (2) the relative abundance, with its probable error, of the various isotopes; (3) the factor r (with its probable error) by which one converts from the physical to the chemical scale of atomic weights.*

* See R. T. Birge (1939 a) for the explicit formula needed to calculate, by propagation of errors,. the final probable error of the specified mixture of isotopes.

The value of r is obtained from the observed masses and relative abundance of the oxygen isotopes, and the value here adopted is $r = 1 \cdot 000272 \pm 0 \cdot 000005$. This value results from the following assumed isotopic abundance,

$$^{16}O : {}^{18}O : {}^{17}O = (506 \pm 10) : 1 : (0 \cdot 204 \pm 0 \cdot 008),$$

together with the masses $^{16}O = 16 \cdot 0000$ (by definition), $^{18}O = 18 \cdot 0049$ and $^{17}O = 17 \cdot 0045$ (Hahn, Flügge and Mattauch, 1940). Whatever small probable errors may exist for the rare isotopes ^{18}O and ^{17}O have a completely negligible effect on the resulting probable error of r.

The ratio $^{17}O/^{18}O = 0 \cdot 204 \pm 0 \cdot 008$ is due to Murphey (1941). The important ratio $^{16}O/^{18}O$ presents more of a problem. There are three rather recent determinations. Manian, Urey and Bleakney (1934) obtain 514 ± 13 as a final weighted average of observations on oxygen derived from five different sources (meteorites and rocks). The ratios obtained from different sources are all, internally, quite consistent, but vary from 493 to 540 for the different sources. These results, however, are derived by one method. With another method the 493 ratio changes to 526, and the 540 ratio becomes 515, etc. Hence the variations may be only experimental in origin.

The other two investigations are those by Smythe (1934), who obtained a ratio 503 ± 10 for oxygen derived from PbO_2 and by Murphey (1941), who obtained 500 ± 15 from various samples of commercial oxygen and of air. There is probably little difference in the accuracy of these three investigations, and I therefore adopt the unweighted average 506. In view of the variations in the experimental values just quoted, and the additional evidence now to be presented, it would appear that one must take the probable error at least as large as ± 10.

Apart from the question of the *absolute* value of the $^{16}O/^{18}O$ ratio, we have the very interesting question of the *possible real variation of the ratio in the case* of oxygen from various sources. If such a variation exists, there must necessarily be a corresponding variation in the value of r, depending on the source of the oxygen used. Extensive and extremely precise work on this subject has been carried out in the past six years. The method is to use oxygen in the form of water, and to measure the *relative density* of the water prepared with oxygen from various sources. The density of the water is stated in terms of " gamma units ", where one gamma is the *excess* density of the water in question, in parts per million, over ordinary water. It is possible at present to obtain an accuracy of one- or two-tenths of a gamma unit.

Now if the changing density of the water is due solely to a change in the $^{16}O/^{18}O$ ratio of the oxygen, one can easily show that an *increase* of one gamma unit in the density of water equals a *decrease* of 2·04 units in the $^{16}O/^{18}O$ ratio, based on 506 as the standard value. Thus *variations* as small as 0·1 per cent in the $^{16}O/^{18}O$ ratio can be detected in this way. It is not practical to go into details here, but a comprehensive picture of the entire experimental situation can be gained from three recent papers, by Greene and Voskuyl (1939), Swartout

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 $^{1}H/^{2}H = 6900 \pm 100$, given in table a'. The involved experimental procedures designed to isolate a possible changing $^{1}H/^{2}H$ ratio from a possible changing $^{16}O/^{18}O$ ratio are described in detail by Swartout and Dole (1939). Furthermore, if we now assume that the *adopted* $^{16}O/^{18}O$ 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 ($14\cdot0086 \pm 0\cdot0007$) 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 $^{16}O/^{18}O$ ratio.

One of the best determined of all chemical ratios, based on gravimetric work, is $AgNO_3/Ag = 1 \cdot 574790$, which appears to have a probable error of $0 \cdot 000005$ or less.* From $N = 14 \cdot 00740 \pm 0 \cdot 00012$, as determined from mass spectrograph work (see table a'), one gets immediately $Ag = 107 \cdot 8783_5 \pm 0 \cdot 0009_5$, 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 $N = 14 \cdot 0086 \pm 0 \cdot 0007$, and the same $AgNO_3/Ag$ ratio, one gets $Ag = 107 \cdot 880_2 \pm 0 \cdot 001_5$. As already noted, I *adopt* $Ag = 107 \cdot 880 \pm 0 \cdot 002$, pending a more comprehensive examination of this important question.

The value for iodine ($126 \cdot 915 \pm 0 \cdot 004$) results from a study of the latest data, much of which is discussed by Baxter.† The present adopted value deviates considerably from the value ($126 \cdot 932 \pm 0 \cdot 002$) 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

$A_0 = H_0 \cdot \rho_0(Hg) \cdot \delta_m(H_2O) \cdot g_0$, where $H_0 = 76 \cdot 000$ cm. = height of standard barometer;

$\rho_0 =$ standard specific gravity of Hg (at 0° c., A_0), referred to air-free water of maximum density;

$\delta_m(H_2O) =$ maximum density of water $= 0 \cdot 999972 \pm 0 \cdot 000002$ g.cm.$^{-3}$ (Section C);

$g_0 =$ standard gravity $= 980 \cdot 665$ cm.sec.$^{-2}$ (Section B).

The only quantity that remains to be evaluated is thus ρ_0. In G.C. 1929 I adopted $\rho_0 = 13 \cdot 59546$, 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 $13 \cdot 59549$ is open to some question, for the following reason. Their work, carried out by the hydrostatic method, gave two results. One result, $13 \cdot 59540$, 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, $13 \cdot 59558$,

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., $13\cdot59549 \pm 0\cdot00009$.

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 $\rho_0 = 13\cdot59539$.   Their published probable error is only $0\cdot00001$, but a study of their work indicates that $0\cdot00006$ is a more reasonable estimate. With this latter figure, the weighted average of the two investigations is $\rho_0 = 13\cdot59542 \pm 0\cdot00003$, $(\pm 0\cdot00005_0)$.   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 $(=\rho_0 . \delta_m)$, is $D_0 = 13\cdot59504_0 \pm 0\cdot00005_7$ g.cm.$^{-3}$, and finally *

$$A_0 = D_0 \times 76 \times 980\cdot665 = (1\cdot013246 \pm 0\cdot000004) \times 10^6 \text{ dyne.cm.}^{-2}$$

or

$$A_{45} = D_0 \times 76 \times 980\cdot616 = (1\cdot013195 \pm 0\cdot000004) \times 10^6 \text{ dyne.cm.}^{-2}$$

SECTION H
THE ABSOLUTE TEMPERATURE OF THE ICE-POINT (T_0)

In G.C. 1929 the adopted value of the ice-point was $T_0 = 273\cdot18 \pm 0\cdot03°$ K. This result was derived from Roebuck's observed value $273\cdot15°$ K. and Henning and Heuse's value † $273\cdot19°$ 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.   Roebuck and Murrill (1941) give an extensive table of determinations of T_0, with an average value of $273\cdot170°$ K. They advocate $273\cdot17 \pm 0\cdot02°$ K. as the present best value.   Beattie (1941) gives a similar table with an average of $273\cdot165°$ 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 $273\cdot165°$ K. and $273\cdot167°$ K., respectively.

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* Had it not been for my premature rounding of data, the value $A_0 = 1\cdot013249 \times 10^6$ given in G.C. 1929 would have been $1\cdot013250$, 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 273·15° K. as the best value of T_0.* It may be noted, in this connection, that the Leiden value of 273·144° 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 $T_0 = 273·15 \pm 0·02°$ K. I believe that Keesom and Tuyn did not have available Beattie's recent result of 273·165° or 273·167° K.

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

$$T_0 = 273·16 \pm 0·01° \text{ K.,}$$

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 $\pm 0·01°$ 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 $J'_{15} = 4·18420$ 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 $J'_{15} = 4·1832_7$ int. joules, with an estimated probable error of $\pm 0·0008_0$ 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

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* See also the lecture by H. B. G. Casimir (1939), in which $T_0 = 273·15°$ K. is advocated.
† *Nature, Lond.*, **145**, 597 (1940).

writer, find a trend in the J. and S. residuals. This trend they proceed to correct in a quite different way, but their corrected result ($J'_{15} = 4 \cdot 1832$) is almost identical with my own. Their own experimental results, which cover the range $0°$ to $100°$ C., are given in the form of a table, in which J_{15} is listed as $4 \cdot 1850$ int. joules. Their actual result at $15°$ C. is, however, $4 \cdot 18502$.* No explicit estimate of their probable error is given, but Dr. Osborne writes † : " We suspect no sources of systematic errors which, if removed, would change our result by more than one part in 10,000, although there may be an even chance that they were present." This is a field in which systematic errors are important, and I take $\pm 0 \cdot 0004$ (i. e., one part in 10,000) as the probable error of the O.S.G. result.

The direct measurement of the mechanical equivalent is that by Laby and Hercus (1927; referred to in the rest of this section as " L. and H."). This work also was recalculated by the writer, in G.C. 1929, and certain data were discarded. The value, as thus recalculated, is $J_{15} = 4 \cdot 1852_6 \pm 0 \cdot 0008_0$ abs. joules, and this result was later accepted by Laby and Hercus (1935). Since the average residual in the L. and H. work is somewhat smaller than in the J. and S. work, I am now increasing the J. and S. probable error to $0 \cdot 0009$, and decreasing that of L. and H. to $0 \cdot 0007$. In order to use also the value of $J_{15} = 4 \cdot 1852_6 \pm 0 \cdot 0007_0$ abs. joules, it must first be reduced to int. joules by means of the factor † $pq^2 = 1 \cdot 00020 \pm 0 \cdot 00004_5$. The result is $J_{15} = 4 \cdot 1844_2 \pm 0 \cdot 0007_0$ int. joules.

We now have three values of J'_{15}, namely: (J. and S.) $4 \cdot 1832_7 \pm 0 \cdot 0009_0$, (O.S.G.) $4 \cdot 1850_2 \pm 0 \cdot 0004_0$, and (L. and H.) $4 \cdot 1844_2 \pm 0 \cdot 0007_0$. The weighted average is $J_{15} = 4 \cdot 1846_{64} \pm 0 \cdot 0002_{81}(0 \cdot 0003_{24})$. The r_e/r_i ratio is $0 \cdot 867$, an entirely satisfactory value. I therefore *adopt*, for the electrical Joule equivalent, at $15°$ C.,

$$J_{15} = 4 \cdot 1847 \pm 0 \cdot 0003 \text{ int.-joule.cal}_{15}^{-1}$$

Multiplying by pq^2, we get for the *adopted* (mechanical) Joule equivalent, at $15°$ C.,

$$J_{15} = 4 \cdot 1855 \pm 0 \cdot 0004 \text{ abs.-joule.cal}_{15}^{-1}.$$

The adopted value of J'_{15} is $0 \cdot 0012$ larger than that given in G.C.1929, but J_{15} is only $0 \cdot 0003$ larger.

<div align="center">SECTION J</div>

<div align="center">THE FARADAY (*F*)</div>

The Faraday *F* is defined as the quantity of electricity carried in electrolysis by one gram equivalent M of any atom or molecule. Hence, if a charge Q is associated with m grams mass, we have

$$F = Q/(m/M) \qquad \ldots\ldots(1)$$

and $F = Q$ when $m/M = 1$ (one gram equivalent). The number of atoms or molecules in a gram equivalent is N_0/V and each carries a charge Ve, where V

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* See table *b*. In G.C. 1929, $pq^2 = 1 \cdot 00041 \pm 0 \cdot 00010$ was used.
† Private communication from Dr. Osborne.

is the valence. Hence the total charge carried by one gram equivalent is $F = N_0 e$, where $N_0 =$ Avogadro number and $e =$ electronic charge. This last equation is used, in Section K, for the evaluation of e, from the measured values of F and N_0.

Equation (1) may also be written

$$F = M/(m/Q) = M/E, \qquad \ldots\ldots (2)$$

where $E(=m/Q)$ is the electrochemical equivalent, i.e., the mass associated with unit charge. Equation (2) is used for the actual evaluation of F, and if the substance is univalent, the gram equivalent M is merely the atomic or molecular weight in grams. E is measured by an electrolytic cell, and, unfortunately, the mass E^* actually deposited or liberated per unit electric charge in such a cell is not necessarily the true mass E associated with unit charge. Let us denote by F^* the resulting *apparent* value of F, as obtained from $F^* = M/E^*$.

In order to obtain a trustworthy value of F, the first requirement is to select an electrolytic reaction that is likely to yield a value of E^* as close as possible to the true E. Only two such reactions have thus far been seriously proposed. These are the reactions occurring at the cathode of the silver voltameter, and at either the anode or the cathode of the iodine voltameter. Unfortunately the present resulting values of F^* given by these two voltameters differ by an amount greater than had been expected from the obvious experimental errors. Hence there has been, for many years, considerable uncertainty as to the true value of F, and considerable controversy as to the best value to adopt, on the basis of the available evidence.

A brief discussion of the situation, as it existed in 1929, is given on pp. 33–36 of G.C. 1929. At that time I recommended a value of F based wholly on the silver voltameter. At the present time I shall recommend a certain weighted average of the silver and iodine voltameter results. This is a subject that for more than one reason demands a detailed discussion, and I have, in fact, already prepared such a discussion. There is, however, no space for it here, and I accordingly content myself with the following brief statement.

Since both silver and iodine are univalent elements, equation (2) becomes, for these substances,

$$F_{Ag}^* = Ag/E_{Ag}^* \quad \ldots\ldots (3), \qquad\qquad F_I^* = I/E_I^*, \qquad \ldots\ldots (4)$$

where Ag and I represent the atomic weights in grams (given in Section F), and E_{Ag}^* and E_I^* the apparent electrochemical equivalents in grams per coulomb, as measured by the silver and iodine voltameters. Then, from equation (3),

$$F_{Ag}^* = \frac{107 \cdot 880 \pm 0 \cdot 002}{0 \cdot 00111800} = 96493 \cdot 7 \pm 1 \cdot 8 \text{ int. coulombs}, \quad \ldots\ldots (5)$$

since $E_{Ag}^* = 0 \cdot 00111800$ gram (with *no* error) by the *definition* of the international coulomb.

In the case of the iodine voltameter it is necessary to measure not only the mass of iodine released, but also the quantity of electricity Q that has passed

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 $1 \cdot 5$ parts in 10^6.

Thus Q, in int. coulombs, is measured by m_{Ag}/E^*_{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

$$E^*_I = \frac{m_I}{(m_{Ag}/E^*_{Ag})} = \frac{E^*_{Ag}}{(m_{Ag}/m_I)} \qquad \ldots\ldots(6)$$

Vinal and Bates' experimental result is $m_{Ag}/m_I = 0 \cdot 850173_4 \pm 0 \cdot 000016$,† and from equation (6)

$$E^*_I = \frac{0 \cdot 00111800}{0 \cdot 850173_4 \pm 0\ 000016} = (131 \cdot 5026 \pm 0 \cdot 0025) \times 10^{-5} g. \quad \ldots\ldots(7)$$

Finally, from equation (4),

$$F^*_I = \frac{126 \cdot 915 \pm 0\ 004}{(131 \cdot 5026 \pm 0\ 0025) \times 10^{-5}} = 96511 \cdot 4 \pm 3 \cdot 4_6 \text{ int. coulombs.} \quad \ldots\ldots(8)$$

The difference between the results of equations (5) and (8) is $17 \cdot 7 \pm 3 \cdot 9_0$ 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 10^5. Correction for such inclusions raises F^*_{Ag} by $3 \cdot 9$ 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 10^5, or $4 \cdot 8$ 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 10^5, 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

$$F^*_{Ag} = 96497 \cdot 6 \pm 4 \cdot 25 \text{ int. coulombs}, \quad F^*_I = 96506 \cdot 6 \pm 5 \cdot 18 \text{ int. coulombs}.$$

The discrepancy thus becomes $9 \cdot 0 \pm 6 \cdot 7_0$ 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 $F^* = 96501 \cdot 22 \pm 2 \cdot 98$ ($\pm 3 \cdot 29$) 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*,

$$F = 96501 \cdot_2 \pm 10 \text{ int. coulombs}.$$

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

$$F = 96487 \cdot_7 \pm 10 \text{ abs. coulombs}$$

$$= 9648 \cdot 7_7 \pm 1 \cdot 0 \text{ abs. e.m.u.},$$

$$F' = Fc = (2 \cdot 89247 \pm 0 \cdot 00030) \times 10^{14} \text{ abs. e.s.u.}$$

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

$$F = 96514 \cdot_0 \pm 10 \text{ abs. coulombs}$$

$$= 9651 \cdot 4_0 \pm 1 \cdot 0 \text{ abs. e.m.u.},$$

$$F' = Fc = (2 \cdot 89326 \pm 0 \cdot 00030) \times 10^{14} \text{ abs. e.s.u.}$$

The adopted value of $96501 \cdot_2$ int. coulombs, on the chemical scale, is $7 \cdot 2$ coulombs higher than the F^*_{Ag} value 96494, which I have advocated from1 929 up till now. On the other hand, the final reduced value $F = 96514 \cdot_0$ abs. coulombs, on the physical scale, is very close to the value $96513 \cdot_1$ that I used in 1938 (Birge, 1938). The close coincidence is due to two nearly compensating changes: (1) the change of q from $0 \cdot 99993$ to $0 \cdot 99986$, and (2) the change of F, in int. coulombs, from 96494 to $96501 \cdot_2$.

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

$$E_{Ag} = (1 \cdot 11807 \pm 0 \cdot 00012) \times 10^{-3} \text{ g.abs.-coulombs}^{-1} \text{ (chemical scale), and}$$

$$E_I = (1 \cdot 31535 = 0 \cdot 00014) \times 10^{-3} \text{ g.abs.-coulombs}^{-1} \text{ (chemical scale).}$$

SECTION K
THE AVOGADRO NUMBER (N_0) AND THE ELECTRONIC CHARGE (e)

We come now, in conclusion, to the three atomic constants, e, m and h.* One or more of these constants enters into nearly every formula of modern physics and, because of the resulting theoretical relations, it is possible to evaluate many different combinations of the three constants. In fact, in the case of m and h, it is possible to measure them with any real precision *only* in certain combinations, and the three simplest quantities that can thus be determined experimentally are e, e/m and h/e. Hence the three remaining sections of the paper will be devoted to these three quantities, respectively.

As is doubtless well known to the reader, the various measured functions of e, m and h have hitherto exhibited certain serious inconsistencies. This situation has been directly responsible for a flood of experimental investigation, with an accompanying flood of critical discussion. In fact, the major portion of all experimental work on the general physical constants, during the past decade, has been concerned with the atomic constants. Accordingly the major portion of any detailed paper on the general physical constants should be devoted to the atomic constants, and since I have an especial interest in such constants, there is all the more reason for me to consider them at length.

Actually it is necessary to take the opposite course. As already noted, there have been numerous critical papers on this subject, from which the reader can get full details. Among the more recent papers are those by Wensel (1939), Dunnington (1939), DuMond (1939, 40) and Kirchner (1939). Wensel is concerned primarily with the radiation constants σ and c_2, and he treats the atomic constants only in connection with the evaluation of the radiation constants. Much the longest paper, and the only one that gives any experimental details, is that by Kirchner. Dunnington treats the three atomic constants by means of what he and others have termed the Birge-Bond diagram (Bond, 1930, 31; Birge, 1932 b). DuMond treats them in terms of a new type of diagram. As I have pointed out (Birge, 1940 a), there is an infinite number of such possible diagrams, and since each of them attempts to represent in two dimensions the relations between three quantities (e, m and h), each one suffers from the same limitations. Hence the choice of the best form of diagram is more or less a matter of taste.

The history of the values of e, m and h is really a most curious one. In G.C. 1929 I adopted what was known as "Millikan's value of e" ($\sim 4 \cdot 77 \times 10^{-10}$ e.s.u.). Even at that time a quite different value of e ($\sim 4 \cdot 80 \times 10^{-10}$ e.s.u.) had just been obtained by means of the wave-lengths of x rays, as measured with ruled gratings. Three years later (Birge, 1932 b), I still felt that no appreciable modification of the value of e was called for, and this opinion was based not on any special trust in Millikan's work, but rather on the fact that the functions of e, m and h measured with the greatest apparent accuracy were much more consistent with the "oil-drop" (Millikan) value of e than with the "ruled-grating" value.

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* $e=$charge on the electron, $m=$mass of the electron for vanishingly small velocity, and $h=$Planck's constant.

Meanwhile extensive experimental and theoretical work has now made it quite clear that the ruled-grating value of *e* must be essentially correct, and repeated new measurements of the viscosity of air have shown that the value used by Millikan was seriously in error. The present most reliable values of the viscosity raise the oil-drop value of *e* not only up to the grating value, but even above it. But at the moment the grating value appears to have much the greater accuracy, and it alone will be evaluated here.

As a result of the change in the adopted value of *e*, a number of new and apparently serious discrepancies arose. I immediately pointed out (Birge, 1935) the gravity of the new situation, and a few months later I noted (Birge, 1936) that all of the important remaining discrepancies could be reduced to the statement that the directly measured values of *h/e* differed seriously from the value calculated indirectly by means of the observed values of *e* and *e/m* and the Bohr formula for the Rydberg constant. When I started to write this paper, I believed that this final remaining discrepancy had at last been resolved, and hence I felt that it was a propitious time to issue a new consistent set of values of the general constants. During the writing of the paper further experimental progress has been made, and one can now (October 1941) say, with some assurance, that there is a good agreement between the direct and indirect values of *h/e*.

Even two years ago I felt that the preponderance of evidence was in favour of the indirect value, and hence in this paper, as in 1939,* I merely *assume* that the experimental values of *e* and *e/m* are correct, and that the Rydberg constant formula is valid. Then *h/e* (and *h*) becomes a *derived* constant. A brief comparison of this indirectly calculated value of *h/e* with the latest experimental values is given in Section M.

From the wave-lengths of x-ray lines, measured in I.A. units with a ruled grating, and the observed angles θ at which these same x-ray lines are reflected from a crystal, one can calculate by Bragg's equation ($\lambda = 2d \sin \theta$) the value of *d*, the effective grating space of the crystal. The calculation is best made by comparing the observed grating wave-lengths of x-ray lines with their observed wave-lengths on the Siegbahn system. In this system it is *assumed* that the effective grating space of calcite at 18° c., for first-order reflection, is $d''_{18} = 3 \cdot 02904 \times 10^{-8}$ cm. After correction for refraction † one obtains for the true grating space, $d'_{18} = 3 \cdot 029449 \times 10^{-8}$ cm., at 18° c., and $d'_{20} = 3 \cdot 029512 \times 10^{-8}$ cm., at 20° c. The probable errors are negligibly small.

These values are all on the Siegbahn system. To convert them to the ruled grating (or I.A.) system, one multiplies by $1 \cdot 002034 \pm 0 \cdot 000060$. This is the value calculated by Bearden (1935; see also Bearden, 1941) for the ratio λ_g / λ_s of x-ray wave-lengths on the grating and Siegbahn systems. It represents the weighted average of all known determinations. Bearden obtains $\pm 0 \cdot 000016$ as

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* In August 1939 a mimeographed table of constants, with a brief explanation, was circulated privately for suggestions and corrections, preparatory to a detailed published paper.

† See G. C. 1929, p. 40, for details.

the calculated probable error, based on external consistency.* To allow for systematic errors of all kinds, I have assumed a probable error nearly four times as large. We thus obtain $d_{20} = (3 \cdot 03567_4 \pm 0 \cdot 00018) \times 10^{-8}$ cm. for the true grating space, at 20° c., on the ruled grating system—which we identify with the c.g.s. system.†

Knowing the value of d_{20}, we can now calculate the Avogadro number N_0 from the equation

$$N_0 = M/\{2\rho\phi(d_{20})^3\}, \qquad \qquad \ldots \ldots (1)$$

where $M =$ molecular weight of calcite ($CaCO_3$), $\rho =$ density of calcite at 20° c., and $\phi =$ geometrical constant for calcite.‡ To get equation (1) we must make the crucial assumption that the calcite crystal has a geometrically perfect structure. Because of the original apparent discrepancy between the oil-drop and the grating values of e, this assumption regarding the calcite crystal has been subjected to prolonged experimental and theoretical investigation. The beautiful work of DuMond and Bollman (1936, 38) now seems to have decided the assumption in the affirmative, although *very small* deviations from a geometrically perfect structure are still not excluded.§

The numerical values to be used in equation (1) are as follows. From the atomic weights listed in table a' we obtain $M = 100 \cdot 091_4 \pm 0 \cdot 005$ (chemical scale). With essentially no discussion of the evidence, I adopt $\rho = 2 \cdot 71029 \pm 0 \cdot 00003$ g.cm.$^{-3}$ at 20° c., and $\phi = 1 \cdot 09594 \pm 0 \cdot 00001.\|$ With these values we get, as the *adopted* value of the Avogadro number on the chemical scale,

$$N_0 = (6 \cdot 0228_3 \pm 0 \cdot 0011) \times 10^{23} \text{ mole}^{-1}.$$

With our adopted value $F = 9648 \cdot 7_7 \pm 1 \cdot 0$ abs. e.m.u. (Section J) we derive ¶

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* A very recent summary of all data by Bearden (1941) leads to $\lambda_g/\lambda_s = 1 \cdot 00203 \pm 0 \cdot 00001$, which does not differ essentially from his 1935 average of $1 \cdot 002034 \pm 0 \cdot 000016$.

† More precisely, the result is 3·03567 I.A. units, but for the present purpose any difference between an I.A. unit and 10^{-8} cm. is completely negligible.

‡ See Bearden (1931) for the equations that connect ϕ with the cleavage angles of calcite.

§ It is quite possible that, in order to allow for such a small mosaic effect, the probable errors adopted here for N_0 and e should have been taken twice as large. See Miller and DuMond (1940)

$\|$ Bearden (1931) adopts 2·71030 as the best average of the latest precision results. But his recalculated value (2·71034) of the De Foe and Compton data should be 2·71030, and this result is then to be averaged with Bearden's own result, 2·71026, and a later result, 2·71029, obtained with the same crystals at the National Bureau of Standards (see Bearden, 1938). Hence our adopted average and its probable error seem justified. The value of ϕ and its probable error, which corresponds to 2″ of arc in the angle β, come directly from Bearden (1931).

¶ A radical, but apparently necessary, revision of symbols is being made here. In the table of derived constants, it is essential that each symbol represent not only a specified quantity (such as the charge on the electron), but also a specified quantity in terms of a *specified unit*. In G.C. 1929 and elsewhere I have followed the objectionable but common practice of using e for the charge in e.s.u. and e/m for the specific charge in e.m.u. Since almost all precision determinations of electric and magnetic quantities are made directly in practical units, which can be converted to e.m.u. by a mere shift of decimal point, I propose now to use *unprimed* symbols such as e and F to represent these quantities in e.m.u., and the *primed* symbols e' and F' to represent them in e.s.u. Then $e' = ec$, $F' = Fc$, $e'/m = ec/m$, $h/e' = h/(ec)$, etc. F and e/m thus have the same meaning as in G.C. 1929, but e' corresponds to the previous e, and h/e' to the previous h/e.

and

$$e = F/N_0 = (1 \cdot 60203_3 \pm 0 \cdot 00034) \times 10^{-20} \text{ abs. e.m.u.}$$

$$e' = ec = (4 \cdot 8025_1 \pm 0 \cdot 0010) \times 10^{-10} \text{ abs. e.s.u.}$$

The value of e, or of e', when derived from oil-drop work, represents a *fundamental* constant, and N_0 is then a *derived* constant, according to the equation $N_0 = F/e$. The two constants N_0 and e are so classified in G.C. 1929 and in practically all more recent discussions. But when e is derived in the way now discussed, it is not e but N_0 that constitutes the fundamental constant, and $e \, (= F/N_0)$ then becomes a *derived* constant. Hence e should logically now appear in table c, along with other derived constants. It seems advisable, however, to retain it in table a.*

The " grating " value of e, as just calculated, is certainly more accurate than any value that can now be obtained from oil-drop work. This statement remains true, even if very much larger probable errors are assumed for the various constants of calcite, as Kirchner (1939) has done. As already stated, no discussion will be included here of recent oil-drop work, or of work on the viscosity of air, but it may be noted that a summarized table of such work has been given by Robinson (1938). Subsequent to his paper, Bearden (1939 b) has obtained $1834 \cdot 16 \times 10^{-7}$ c.g.s. units for the 23° c. viscosity, which with Millikan's oil-drop data yields $e' = 4 \cdot 8129 \times 10^{-10}$ abs. e.s.u., as compared with $4 \cdot 768$ given by the viscosity value used by Millikan. Apart from any error in the viscosity measurement, the probable error in e is at least $\pm 0 \cdot 005 \times 10^{-10}$ e.s.u., due to errors in Millikan's oil-drop work.

Hopper and Laby (1941) have just published the results of a new oil-drop measurement of e. Their published result corresponds to a value of e higher by $0 \cdot 0054 \times 10^{-10}$ e.s.u. than that given by Millikan's data. They conclude that in their work the error in e, exclusive of any error in the viscosity of air, is only $\pm 0 \cdot 0013 \times 10^{-10}$ e.s.u. But they also conclude that the present best value of the viscosity of air is $(1830 \cdot 0 \pm 2 \cdot 5) \times 10^{-7}$ c.g.s. units—and they thus get $e = (4 \cdot 802 \pm 0 \cdot 010) \times 10^{-10}$ abs. e.s.u. for the final result of their work. Although their final value of e thus agrees almost exactly with our adopted x-ray value, the probable error is ten times as large, and, moreover, this large error is due almost entirely, according to Hopper and Laby, to the error in the viscosity of air, rather than to errors in their own oil-drop measurements.

<div align="center">SECTION L</div>

<div align="center">THE SPECIFIC CHARGE OF THE ELECTRON (e/m)</div>

The value of e/m has been measured in numerous investigations, and by the use of many different methods. In fact no other atomic constant has been measured so often and in so many different ways. The entire situation regarding

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* The foregoing considerations do not affect the calculated value of N_0 (or of e), but they do affect the calculated probable error of N_0. Thus if one should calculate the probable error in N_0 from the stated probable errors in F and e, on the assumption that $N_0 = F/e$, the probable error in F would thereby enter twice, whereas by equation (1) it does not enter at all.

e/m has, however, changed since G.C. 1929 was written. At that time it appeared to me necessary to adopt *two* values of *e/m*, a " deflection " value of $(1 \cdot 769 \pm 0 \cdot 002) \times 10^7$ abs. e.m.u. and a " spectroscopic " value of $1 \cdot 761 \pm 0 \cdot 001$. The apparent necessity for this unusual procedure is illustrated by the fact that Gerlach (1926), in a complete and authoritative review, adopted $1 \cdot 766$ as the best average, and listed in his final summary *no* value under $1 \cdot 763$. Possibly as a result of the inconsistency emphasized in G.C. 1929, there have since appeared numerous investigations, both experimental and theoretical, and from time to time I have reported (Birge, 1932 c, 34 a, 36, 38, 40 b) on the resulting situation. At the present time it is quite clear that the " low " spectroscopic value of G.C. 1929 is much more nearly correct than the higher value advocated by Gerlach and others.

The most important spectroscopic methods for evaluating *e/m* involve the Rydberg constant (*R*). A detailed paper on the best values of *R* and of *e/m*, from the spectra of H, D and He$^+$, has now been prepared (Birge, 1941), and the reader is referred to that paper for the relevant material. In connection with its preparation I have recalculated much more thoroughly than at any time in the past all of the precision spectroscopic work bearing on the subject. As a result o fan important investigation by Drinkwater, Richardson and Williams (1940), there has been a significant change in the value of R_∞ from that adopted in G.C. 1929.* The recent change in the value of *q* (Section E) also produces a small change in *every* value of *e/m*.

At the present time there are available twelve precision values of *e/m*, obtained by seven essentially different methods. Table 2 is reproduced from the

Table 2. Values of *e/m*

	e/m	Probable error	Author	Method	$f(F_i pq)$
1	1·75913	$3 \cdot 7 \times 10^{-4}$	Drinkwater, Richardson and W. E. Williams (1940)	H$_\alpha$ and D$_\alpha$	$F_i q$
2	1·75797	$5 \cdot 0 \times 10^{-4}$	R. C. Williams (1938)	H$_\alpha$ and D$_\alpha$	$F_i q$
3	1·75914	$5 \cdot 0 \times 10^{-4}$	C. F. Robinson (1939)	H$_\alpha$ and D$_\alpha$	$F_i q$
4	1·75815	$6 \cdot 0 \times 10^{-4}$	Shane and Spedding (1935)	H$_\alpha$ and D$_\alpha$	$F_i q$
5	1·76048	$5 \cdot 8 \times 10^{-4}$	Houston (1927); Chu (1939)	R^{H} and R_{He}	$F_i q$
6	1·75700	$7 \cdot 0 \times 10^{-4}$	Kinsler and Houston (1934)	Zeeman effect	$1/q$
7	1·76006	$4 \cdot 0 \times 10^{-4}$	Bearden (1938)	X-ray refraction	$1/F_i q$
8	1·76110	$\cdot 10 \cdot 0 \times 10^{-4}$	Perry and Chaffee (1930)	Direct velocity	$1/pq$
9	1·75900	$9 \cdot 0 \times 10^{-4}$	Kirchner (1931, 32)	Direct velocity	$1/pq$
10	1·75982	$4 \cdot 0 \times 10^{-4}$	Dunnington (1933, 37)	Magnetic deflection	$1/q$
11	1·75820	$13 \cdot 0 \times 10^{-4}$	Shaw (1938)	Crossed fields	p/q
12	1·75870	$8 \cdot 0 \times 10^{-4}$	Goedieke (1939)	Busch method	p/q

paper (Birge, 1941) just mentioned. In the case of each result, the published data have been recalculated with the values of the auxiliary constants adopted

* The various values of *R* now adopted (all on the I.A. scale) are, $R_{\mathrm{H}} = 109677 \cdot 581_2 \pm 0 \cdot 007_5$ cm.$^{-1}$, $R_{\mathrm{D}} = 109707 \cdot 419_3 \pm 0 \cdot 007_5$ cm.$^{-1}$, $R_{\mathrm{He}} = 109722 \cdot 263 \pm 0 \cdot 012$ cm.$^{-1}$, and finally, $R_\infty = 109737 \cdot 303 \pm 0 \cdot 017$ cm.$^{-1}$ (or $\pm 0 \cdot 05$ cm.$^{-1}$ in the c.g.s. system).

here. In the case of the spectroscopic results, several modifications have been made in the detailed reduction of the data. The final column of the table lists the dependence of the value of e/m on F_i, the value of the Faraday in international units (hence $F_i q$ gives the Faraday in absolute units), and on p and q (of Section E).

Several significant conclusions may be drawn from the table. In the first place, the weighted average of all twelve values of e/m is $1.7592_{06} \pm 0.0001_{92}$ $(\pm 0.0001_{58})$.* The ratio r_e/r_i is 1.213, which is quite satisfactory. In other words, there is no longer any serious discrepancy between the various results, considered collectively. There is, however, still a very slight discrepancy between the " spectroscopic " and the " deflection " results. Thus the weighted average of the first six † results listed in table 2 (the " spectroscopic " values) is $e/m = 1.75880 \pm 0.00028_2$ $(\pm 0.00020_8)$, and $r_e/r_i = 1.36$. The last five results of the table (the " deflection " values) give an average of $1.75959 \pm 0.00024_1$ $(\pm 0.00030_6)$, and $r_e/r_i = 0.79$. Hence each set of results is reasonably self-consistent. The discrepancy of 0.00079 between the two averages, although only one-tenth of the apparent discrepancy in 1929, is still 2.1 times its own present probable error, whether the error is calculated from internal or from external consistency. Since the odds are about one in six that such a discrepancy is due to pure chance, the situation is not entirely satisfactory. In view, however, of the many serious sources of systematic error that have been revealed, from time to time, in almost every method for measuring e/m, I think that the small remaining discrepancy should not be taken too seriously.

The probable error of the weighted average of all twelve results is only ± 0.0002, as already stated, but just because of the discrepancy of 0.0008 between the two types of measurement, I have increased the adopted probable error to ± 0.0005. Hence the final *adopted* result is

$$e/m = (1.7592 \pm 0.0005) \times 10^7 \text{ abs.-e.m.u. g}^{-1}$$
$$e'/m = ec/m = (5.2736_6 \pm 0.0015) \times 10^{17} \text{ abs.-e.s.u. g}^{-1}$$

The data listed in the table make possible an independent evaluation of the Faraday, since the first five spectroscopic results involve F and the five deflection results do not. The new spectroscopic average is 1.75897, and the result is, therefore,

$$F = (9651.40)(1.75959)/(1.75897) = 9654.80 \text{ abs. e.m.u. (physical scale)},$$

or 96535 int. coulombs (chemical scale).‡ Purely on the basis of the external consistency of the various e/m values involved in the two averages, the resulting

* Even as early as 1932 I suggested (Birge, 1932 b) 1.7592 as the best value of e/m. But in 1934 this " constant " had reached an " all-time low " of 1.757 (Birge, 1934 a). No one, however, has as yet suggested that the value of e/m is actually changing with time, contrary to the situation in the case of the velocity of light (Section A) !

† Bearden (1939 a) objects to my classification (Birge, 1938) of his x-ray refraction value (Bearden (1938), table 2) with the spectroscopic results. Hence I now place it in a separate category of its own.

‡ The corresponding corrected value of e/m is then necessarily just the deflection average 1.75959, since F is not involved in that average.

value of F has a probable error of ± 20 int. coulombs. In Section J the best average electrochemical value of F has been found to be $96501 \cdot 2 \pm 3 \cdot 0$ int. coulombs, although the adopted probable error has been taken as ± 10 int. coulombs, to allow for possible unforeseen errors. It thus seems fairly certain that the new method for evaluating F is not as accurate as the usual method, and, furthermore, it is far more likely, in my opinion, that the adopted value of 96501 int. coulombs is *too high*, rather than too low.

As noted in Section J, it is now proposed to use the adopted values of e and e/m in the Rydberg constant formula, to *calculate* a value of h. The Birge-Bond diagram immediately shows* that the adopted value of e/m, even with a probable error as large as $\pm 0 \cdot 0005$, is by far the most accurately determined function involving the atomic constants. Hence it is the values of e and h/e that now require critical scrutiny, rather than that of e/m.

<div align="center">

SECTION M

THE PLANCK CONSTANT h

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As already stated in Section K, the Planck constant h, and also h/e, will be treated here as derived constants, and will be calculated from the measured values of e and e/m and the Bohr formula for the Rydberg constant R_∞. This calculation will be followed by a very brief discussion of recent experimental work on h/e.

The Rydberg constant formula involves e, m and h, but e, as calculated in Section K, is itself a derived constant, equal to F/N_0. Hence R_∞ is first to be expressed as a function of h, e/m, F and N_0, and then solved for h, or for h/e. The formula is normally written in the form

$$R_\infty = 2\pi^2 (e')^5 / h^3 c (e'/m) \text{ cm.}^{-1}, \qquad \ldots\ldots(1)$$

where e' indicates the electronic charge in e.s.u. We now change to e.m.u., substitute F/N_0 for e, and in order to simplify the equation introduce the symbol

$$B = 2\pi^2 c^3 / R_\infty. \qquad \ldots\ldots(2)$$

Then equation (1) may be written

$$h = \left\{ \frac{BF^5}{N_0{}^5 (e/m)} \right\}^{1/3} \qquad \ldots\ldots(3)$$

or

$$h/e = \left\{ \frac{BF^2}{N_0{}^2 (e/m)} \right\}^{1/3} \qquad \ldots\ldots(4)$$

Our adopted values of the necessary constants are

$$R_\infty = 109737 \cdot 30_3 \pm 0 \cdot 05 \text{ cm.}^{-1}$$
$$c = (2 \cdot 99776 \pm 0 \cdot 00004) \times 10^{10} \text{ cm. sec.}^{-1}$$
$$F = 9648 \cdot 7_7 \pm 1 \cdot 0 \text{ abs.-e.m u. g.-equiv.}^{-1}$$
$$N_0 = (6 \cdot 0228_3 \pm 0 \cdot 0011) \times 10^{23} \text{ mole.}^{-1}$$
$$e/m = (1 \cdot 7592 \pm 0 \cdot 0005) \times 10^7 \text{ abs.-e.m.u. g.}^{-1}$$

<div align="center">

See, for instance, Dunnington (1939).

</div>

Hence, from equation (2),

$$B = (4 \cdot 84580_{617} \pm 0 \cdot 00019_4) \times 10^{27} \text{ cm}^4 \text{ sec.}^{-3}$$

and from equations (3) and (4),

$$h = (6 \cdot 624_2 \pm 0 \cdot 002_4) \times 10^{-27} \text{ erg. sec.}$$

$$h/e = (4 \cdot 1349_0 \pm 0 \cdot 0007_1) \times 10^{-7} \text{ erg. sec. abs.-e.m.u.}^{-1}$$

$$h/e' = (1 \cdot 3793_3 \pm 0 \cdot 0002_3) \times 10^{-17} \text{ erg. sec. abs.-e.s.u.}^{-1}$$

As mentioned in section K, there are a number of functions involving e, m and h that have been measured with considerable precision. The reader is referred to the papers already quoted (Wensel, 1939; Dunnington, 1939; DuMond, 1939, 40; Kirchner, 1939) for the necessary details. Although I have rather recently carried out extensive and as yet unpublished calculations on the atomic constants, I must here content myself with the following very brief remarks.

The indirect calculation just made corresponds, on the Birge-Bond diagram, to passing a straight line through the two points : (1) the observed value of e, which is plotted at abscissa $p = 0$, and (2) the observed value of e/m, which—after reduction to a $f(e, h)$ only, by means of the Rydberg constant formula—is plotted at $p = 3/5$. Then the corresponding value of h/e lies on the straight line, at $p = 1 \cdot 0$.

Instead of using only these two points, in order to calculate an indirect value of h/e, we may use a group of points and pass, by least squares, the best straight line through them. All such possible points are listed and discussed by Dunnington (1939), but the only ones, in addition to e and e/m, that now seem to be sufficiently accurate to justify real consideration in such a least squares solution are the following * : (1) von Friesen (1935) has obtained a value of $(e/m)(h/e)^2$, from electron diffraction experiments. This point is plotted at $p = 1/3$. (2) Christy and Keller (1940; see also Birge, 1940) have obtained a value of the fine-structure constant α from all available x-ray data, as critically compiled by the writer (Birge, 1939 b). This point is plotted at $p = 1/2$. (3) The value of h/m has been measured by Ross and Kirkpatrick (1934), from the Compton shift. When m is eliminated by means of the Rydberg constant equation, one gets a $f(e, h)$ that is also to be plotted at $p = 1/2$. Hence these two observed points at $p = 1/2$ are first to be replaced by their weighted average, when a least squares solution is made. (4) H. R. Robinson and co-workers have evaluated $(e/m)(e/h)$ by means of the observed $H\rho$ values of electrons ejected by x rays. The two latest investigations, which give almost identical results, are those by Mayo and Robinson (1939) and by Clews and Robinson (1940). This point is plotted at $p = 2/3$.

We thus have points at $p = 0$, $1/3$, $1/2$, $3/5$ and $2/3$. With the values of e and e/m adopted in August 1939, which are almost identical with the present adopted values, and with the use of the additional points just mentioned, I have
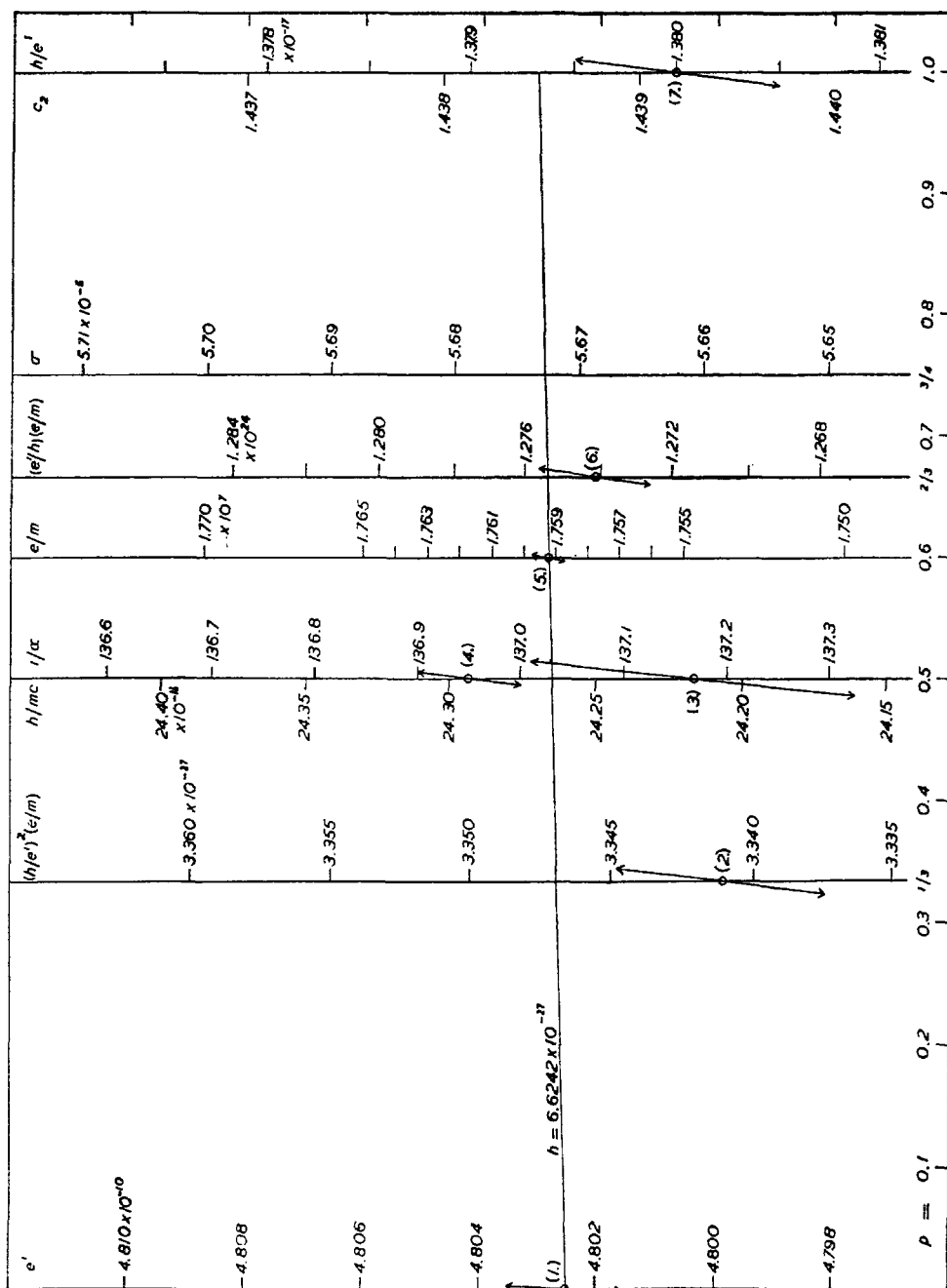
* See figure 1 for these points.

Figure 1. Nomographic chart (Birge-Bond diagram) showing relations between various $f(e, m, h)$. The ordinate scale is so highly magnified (only $\frac{1}{3}$% 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 $h = 6 \cdot 6242 \times 10^{-27}$. The diagram is based on $h = 6 \cdot 625$ 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 $c_2 = (h/e)(Fc/R_0)$, 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 $(1.3793 \pm 0.0002_3) \times 10^{-17}$ 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 * $h/e' \pm (1.3785 \pm 0.0005) \times 10^{-17}$. 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) $e' = (4.8025_1 \pm 0.0010) \times 10^{-10}$ e.s.u. (adopted value).
  (2) von Friesen (1935), $(e/m)(h/e')^2 = (3.3410_5 \pm 0.0037) \times 10^{-27}$ 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) Mayo 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 $p = \frac{3}{4}$ 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 $F'/4\pi = 2.301754 \times 10^{13}$. The scales for many other $f(e, m, h)$ may similarly be plotted on such a diagram.

becomes 1·3800. Both the corrected and the uncorrected results are thus in good agreement with our indirect value.

DuMond and co-workers* are now engaged in remeasuring h/e. By the use of P. A. Ross balanced filters, in conjunction with a two-crystal monochromator, they also have completely eliminated the " tail " on the excitation curve, and they have obtained a remarkably linear plot from which the intercept can be read with very great accuracy. These investigators have now checked their voltage calibration with Bureau of Standards apparatus and their present (October 1941) experimental result † is $(1·3800 \pm 0·0008) \times 10^{-17}$, in exact coincidence with our corrected Ohlin result.

Bearden and Schwarz (1941) have also recently made a preliminary report on a measurement of h/e'. Their results vary from 1·3772 to 1·3793, according to the metal used. They, however, have not eliminated the tail on the excitation curve, and hence their results cannot be considered as trustworthy as those of Ohlin or of DuMond.

Although no discussion is given here of the values of h/e obtained by the remaining four, and less accurate, methods already listed, it is only fair to say that *all* such investigations, with the exception of that by Perlman and Rollefson (1941), have thus far yielded values of h/e' that are considerably *below* the indirect value of 1·3793, and in some cases considerably below even the older DuMond and Bollman (1937) value of 1·3765.‡ On the other hand, the very recent work on h/e just discussed has brought to light important sources of possible systematic error in the earlier work, and furthermore, the new work, as we have seen, is giving values of h/e as high as, or even higher than, our " high " indirect value. Hence it is possible to assume that no real conflict exists between the direct and the indirect values of h/e, and if this assumption is finally confirmed, the values of the general physical constants will be at last in a fairly satisfactory condition.

With these all too brief remarks, I close the present discussion of the general constants. Tables a, a' and b, which follow, are reasonably complete, but table c represents a selection of a few only of the derived constants in most common use. A far more extensive list will be published in connection with the detailed article that I hope to complete in the not too distant future, provided the international situation allows any pure scientific work to be continued.

Table a. Principal constants and ratios §

Section

A	Velocity of light	$c = (2·99776 \pm 0·00004) \times 10^{10}$ cm.sec.$^{-1}$
B	Gravitation constant	$G = (6·670 \pm 0·005) \times 10^{-8}$ dyne.cm. g.$^{-2}$
C	Litre (= 1000 ml.)	$l = 1000·028 \pm 0·002$ cm.^{3}

* DuMond, Bailey, Panofsky and Green, 1941 ; DuMond, Panofsky and Green, 1941.

† I am greatly indebted to Dr. DuMond and Mr. Panofsky for prompt and detailed reports on the progress of this work.

‡ See DuMond (1940, p. 463) for an extended table of such results.

§ Unless otherwise specified, all quantities in these tables that involve the mole or the gram equivalent are on the chemical scale of atomic weights.

Section

D   Volume of ideal gas (0° c., A_0) $V_0 = (22\cdot4146 \pm 0\cdot0006) \times 10^3$ cm.³atmos. mole⁻¹

         $V_0' = 22\cdot4140 \pm 0\cdot0006$ litre.atmos.mole⁻¹

D   Volume of ideal gas (0° c., A_{45}) $V_{45} = (22\cdot4157 \pm 0\cdot0006) \times 10^3$ cm.³ atmos. mole⁻¹

         $V_{45}' = 22\cdot4151 \pm 0\cdot0006$ litre.atmos.mole⁻¹

E   International ohm ($=p$ abs. ohm) $p = 1\cdot00048 \pm 0\cdot00002$

E   International ampere ($=q$ abs. amp.) $q = 0\cdot99986 \pm 0\cdot00002$

F   Atomic weights (see table a')

G   Standard atmosphere $A_0 = (1\cdot013246 \pm 0\cdot000004) \times 10^6$ dyne. cm.⁻²atmos.⁻¹

G   45° atmosphere $A_{45} = (1\cdot013195 \pm 0\cdot000004) \times 10^6$ dyne. cm.⁻²atmos.⁻¹

H   Ice-point (absolute scale) $T_0 = 273\cdot16 \pm 0\cdot01°$ к.

I   Joule equivalent $J_{15} = 4\cdot1855 \pm 0\cdot0004$ abs.-joule.cal.₁₅⁻¹

I   Joule equivalent (electrical) $J_{15}' = 4\cdot1847 \pm 0\cdot0003$ int.-joule.cal.₁₅⁻¹

J   Faraday constant

    (1) Chemical scale :

$$F = 96501\cdot_2 \pm 10 \text{ int.-coul. g.-equiv.}^{-1}$$
$$= 96487\cdot_7 \pm 10 \text{ abs.-coul. g.-equiv.}^{-1}$$
$$= 9648\cdot7_7 \pm 1\cdot0 \text{ abs.-e.m.u. g.-equiv.}^{-1}$$
$$F' = Fc = (2\cdot89247 \pm 0\cdot00030) \times 10^{14} \text{ abs.-e.s.u.}$$
$$\text{g.-equiv.}^{-1}$$

    (2) Physical scale :

$$F = 96514\cdot_0 \pm 10 \text{ abs.-coul. g.-equiv.}^{-1}$$
$$= 9651\cdot4_0 \pm 1\cdot0 \text{ abs.-e.m.u. g.-equiv.}^{-1}$$
$$F' = Fc = (2\cdot89326 \pm 0\cdot00030) = 10^{14} \text{ abs.-e.s.u.}$$
$$\text{g.-equiv.}^{-1}$$

K   Avogadro number (chemical scale) $N_0 = (6\cdot0228_3 \pm 0\cdot0011) \times 10^{23}$ mole⁻¹

K   Electronic charge $e = F/N_0 = (1\cdot60203_3 \pm 0\cdot00034) \times 10^{-20}$ abs. e.m.u.

     $e' = ec = (4\cdot8025_1 \pm 0\cdot0010) \times 10^{-10}$ abs. e.s.u.

L   Specific electronic charge $e/m = (1\cdot7592 \pm 0\cdot0005) \times 10^7$ abs.-e.m.u. g.⁻¹

     $e'/m = ec/m = (5\cdot2736_6 \pm 0\cdot0015) \times 10^{17}$ abs.-e.s.u. g.⁻¹

M   Planck constant h (see table c).

Table a'. Atomic weights

(1) Physical scale (^{16}O $= 16\cdot0000$)

     ^{1}H $= 1\cdot00813 \pm 0\cdot00001_7$ ^{2}H $= 2\cdot01473 \pm 0\cdot00001_9$

     H $= 1\cdot00827_6 \pm 0\cdot00001_7$ (from ^{1}H/^{2}H abundance $= 6900 + 100$)

     ^{4}He $= 4\cdot00389 \pm 0\cdot00007$

     ^{12}C $= 12\cdot00386 \pm 0\cdot00004$ ^{13}C $= 13\cdot00761 \pm 0\cdot00015$

     C $= 12\cdot01465 \pm 0\cdot00023$ (from ^{12}C/^{13}C abundance $= 92 \pm 2$)

     ^{14}N $= 14\cdot00753 \pm 0\cdot00005$ ^{15}N $= 15\cdot0049 \pm 0\cdot0002$

     N $= 14\cdot01121 \pm 0\cdot00009_5$ (from ^{14}N/^{15}N abundance $= 270 \pm 6$)

     ^{16}O $= 16\cdot0000$ ^{17}O $= 17\cdot0045$ ^{18}O $= 18\cdot0049$

     O $= 16\cdot00435_7 \pm 0\cdot00008_6$

         (from abundance ^{16}O : ^{18}O : ^{17}O $= (506 \pm 10) : 1 : (0\cdot204 \pm 0\cdot008)$

(2) Chemical scale (O=16·0000)

Ratio physical to chemical scale

$$r=(16\cdot004357\pm0\cdot000086)/16=1\cdot000272\pm0\cdot000005$$

${}^{1}H=1\cdot00785_{6}\pm0\cdot00001_{8}$ (from physical scale)

${}^{2}H=2\cdot01418_{2}\pm0\cdot00002_{1}$ (from physical scale)

$H=1\cdot00800_{2}\pm0\cdot00001_{8}$ (from physical scale)

${}^{4}He=4\cdot00280\pm0\cdot00007$ (from physical scale)

$C=12\cdot01139\pm0\cdot00024$ (from physical scale)

$N=14\cdot00740\pm0\cdot00012$ (from physical scale)

$N=14\cdot0086\pm0\cdot0007$ (direct observation)

$Na=22\cdot994\pm0\cdot003$

$Cl=35\cdot457\pm0\cdot001$

$Ca=40\cdot080\pm0\cdot005$

$Ag=107\cdot880\pm0\cdot002$

$I=126\cdot915\pm0\cdot004$

Table *b*. Additional quantities evaluated or used in connection with table *a*

Section		
A	Ratio of e.s.u. to e.m.u. (direct)	
	$c'=(2\cdot9971_{2}\pm0\cdot0001)\times10^{10}$ cm.${}^{1/2}$sec.${}^{-1/2}$ int.-ohm${}^{1/2}$	
	$=(2\cdot9978_{4}\pm0\cdot0001_{0})\times10^{10}$ cm.sec.${}^{-1}$	
A	Ratio of e.s.u. to e.m.u. (indirect)	
	$c'=c=(2\cdot99776\pm0\cdot00004)\times10^{10}$ cm.sec.${}^{-1}$	
B	Average density of earth	$\delta=5\cdot517\pm0\cdot004$ g.cm.${}^{-3}$
C	Maximum density of water	$\delta_{m}(H_{2}O)=0\cdot999972\pm0\cdot000002$ g.cm.${}^{-3}$
D	Acceleration of gravity (standard)	$g_{0}=980\cdot665$ cm.sec.${}^{-2}$
D	Acceleration of gravity (45°)	$g_{45}=980\cdot616$ cm.sec.${}^{-2}$
D	Density of oxygen gas (0° c., A_{45})	$L_{1}=1\cdot42897\pm0\cdot00003$ g. litre^{-1}
D	Limiting density of oxygen gas (0° c., A_{45})	$L_{\lim}=1\cdot427609\pm0\cdot000037$ g. litre^{-1}
D	Factor converting oxygen (0° c., A_{45}) to ideal gas	$1-\alpha=1\cdot000953_{5}\pm0\cdot000009_{4}$
E	International coulomb ($=q$ abs. coul.)	$q=0\cdot99986\pm0\cdot00002$
E	International gauss ($=q$ abs. gauss)	
E	International henry ($=p$ abs. henry)	$p=1\cdot00048\pm0\cdot00002$
E	International volt ($=pq$ abs. volt)	$pq=1\cdot00034\pm0\cdot00003$
E	International joule ($=pq^{2}$ abs. joule)	$pq^{2}=1\cdot00020\pm0\cdot00004_{5}$
G	Specific gravity of Hg (0° c., A_{0}) referred to air-free water at maximum density	$\rho_{0}=13\cdot59542\pm0\cdot00005$
G	Density of Hg (0° c., A_{0})	$D_{0}=13\cdot59504_{0}\pm0\cdot00005_{7}$ g.cm.${}^{-3}$
J	Electrochemical equivalents (chemical scale):	
	Silver (apparent)	$E^{*}_{Ag}=1\cdot11800\times10^{-3}$ g. int.-coul.${}^{-1}$
	(corrected)	$E_{Ag}=(1\cdot11807\pm0\cdot00012)\times10^{-3}$ g.abs.-coul.${}^{-1}$
	Iodine (apparent)	$E^{*}_{I}=(1\cdot315026\pm0\cdot000025)\times10^{-3}$ g.int.-coul.${}^{-1}$
	(corrected)	$E_{I}=(1\cdot31535\pm0\cdot00014)\times10^{-3}$ g.abs.-coul.${}^{-1}$

Section

K   Effective calcite grating space (18° c.), $d''_{18} = 3 \cdot 02904 \times 10^{-8}$ cm.
     Siegbahn system

K   True calcite grating space (20° c.), $d'_{20} = 3 \cdot 02951_2 \times 10^{-8}$ cm.
     Siegbahn system

K   True calcite grating space (20° c.), $d_{20} = (3 \cdot 03567_4 \pm 0 \cdot 00018) \times 10^{-8}$ cm.
     c.g.s. system

K   Ratio of grating and Siegbahn scales $\lambda_g / \lambda_s = 1 \cdot 002034 \pm 0 \cdot 000060$
     of wave-lengths

K   Density of calcite (20° c.) $\rho = 2 \cdot 71029 \pm 0 \cdot 00003$ g.cm.$^{-3}$

K   Structural constant of calcite (20° c.) $\phi = 1 \cdot 09594 \pm 0 \cdot 00001$

K   Molecular weight of calcite (chemical $M = 100 \cdot 091_4 \pm 0 \cdot 005$
     scale)

L   Rydberg constant for hydrogen (^{1}H) $R_{\mathrm{H}} = 109677 \cdot 581_2 \pm 0 \cdot 007_5$ cm.$^{-1}$
     (I.A. scale)

L   Rydberg constant for deuterium (^{2}H) $R_{\mathrm{D}} = 109707 \cdot 419_3 \pm 0 \cdot 007_5$ cm.$^{-1}$
     (I.A. scale)

L   Rydberg constant for helium $R_{\mathrm{He}} = 109722 \cdot 263 \pm 0 \cdot 012$ cm.$^{-1}$ (I.A. scale)

L   Rydberg constant for infinite mass $R_{\infty} = 109737 \cdot 303 \pm 0 \cdot 017$ cm.$^{-1}$ (I.A. scale)
     or $\pm 0 \cdot 05$ cm.$^{-1}$ (c.g.s. system)

Table *c*.   Partial list of derived quantities *

Planck constant :

$$h = \left\{ \frac{2\pi^2 c^3 F^5}{R_\infty N_0^5 (e/m)} \right\}^{1/3} = (6 \cdot 624_2 \pm 0 \cdot 002_4) \times 10^{-27} \text{ erg. sec.}$$

$$h/e = \left\{ \frac{2\pi^3 c^3 F^2}{R_\infty N_0^2 (e/m)} \right\}^{1/3} = (4 \cdot 1349_0 \pm 0 \cdot 0007_1) \times 10^{-7} \text{ erg. sec. abs.-e.m.u.}^{-1}$$

$$h/e' = h/ec = \left\{ \frac{2\pi^2 F^2}{R_\infty N_0^2 (e/m)} \right\}^{1/3} = (1 \cdot 3793_3 \pm 0 \cdot 0002_3) \times 10^{-17} \text{ erg. sec. abs.-e.s.u.}^{-1}$$

Atomic weight of electron : $E = F/(e/m)$

     (physical scale) $= (5 \cdot 4862_4 \pm 0 \cdot 0017) \times 10^{-4}$

     (chemical scale) $= (5 \cdot 4847_5 \pm 0 \cdot 0017) \times 10^{-4}$

Band spectra constant connecting wave number and moment of inertia :

$$h/8\pi^2 c = \left\{ \frac{F^5}{256\pi^4 R_\infty N_0^5 (e/m)} \right\}^{1/3} = (27 \cdot 98_{65} \pm 0 \cdot 01_0) \times 10^{-40} \text{g.cm.}$$

Boltzmann constant :

$$k = R_0 / N_0 = V_0 A_0 / T_0 N_0 = (1 \cdot 38047_4 \pm 0 \cdot 00026) \times 10^{-16} \text{erg. deg.}^{-1}$$

Charge in electrolysis of one gram of H :

$$F/H = 9572 \cdot 1_{73} \pm 1 \cdot 0 \text{ abs.-e.m.u. g.}^{-1}$$

Charge in electrolysis of one gram of ^{1}H :

$$e/M_{1\mathrm{H}} = F/^1 H = 9573 \cdot 5_{60} \pm 1 \cdot 0 \text{ abs.-e.m.u. g.}^{-1}$$

* In order to be able to calculate, by propagation of errors, the probable error in a derived quantity, it is necessary to express the quantity explicitly in terms of the various fundamental quantities of table *a* or of table *b*, and that has been done in each case. Since in this paper *e* and *h* are treated as derived quantities, they do not therefore appear in such explicit expressions. But in calculating the *numerical* value of a derived quantity, the work can often be greatly simplified by using the values of other previously calculated derived quantities—in particular *e* and *h*. In order to show how certain derived quantities depend on quantities like *e* and *h*, such alternative expressions are given in many cases.

Compton shift at 90° :

$$h/mc = \left\{\frac{2\pi^2 F^2(e/m)^2}{R_\infty N_0{}^2}\right\}^{1/3} = (0.024265_{14} \pm 0.000005_7) \times 10^{-8} \text{ cm.}$$

Energy in ergs of one abs.-volt-electron :

$$E_0 = 10^8 e = 10^8 F/N_0 = (1.60203_3 \pm 0.00034) \times 10^{-12} \text{ erg}$$

Energy in calories per mole for one abs.-volt-electron per molecule :

$$\frac{F(\text{abs. coul. per gram-equiv.})}{J_{15}(\text{abs. joules per cal.})} = 23052 \cdot_{85} \pm 3 \cdot_2 \text{ cal.}_{15} . \text{ mole}^{-1}$$

Fine structure constant :

$$a = 2\pi(e')^2/hc = \left\{\frac{4\pi R_\infty F(e/m)}{N_0}\right\}^{1/3} = (7.2976_4 \pm 0.0008_6) \times 10^{-3}$$

$$1/a = 137.030_2 \pm 0.016$$
$$a^2 = (5.3256 \pm 0.0013) \times 10^{-5}$$

Gas constant per mole :

$$R_0 = V_0 A_0/T_0 = (8.31436 \pm 0.00038) \times 10^7 \text{ erg. deg.}^{-1} \text{ mole}^{-1}$$
$$R_0' = R_0 . 10^{-7}/J_{15} = 1.98646_7 \pm 0.00021 \text{ cal.}_{15} \text{ deg.}^{-1} \text{ mole}^{-1}$$
$$R_0'' = V_0'/T_0 = (8.20544_7 \pm 0.00037) \times 10^{-2} \text{ litre. atmos.}$$
$$\text{deg.}^{-1} \text{ mole}^{-1}$$
$$R_0''' = R_0/A_0 = V_0/T_0 = 82.0566_7 \pm 0.0037 \text{ cm.}^3\text{atmos. deg.}^{-1}\text{mole}^{-1};$$

also *

$$R_0 T_0 = V_0 A_0 = (2.27115_0 \pm 0.00006) \times 10^{10} \text{ erg. mole}^{-1}$$

Loschmidt number (0° c., A_0) :

$$n_0 = N_0/V_0 = (2.6870_{12} \pm 0.0005_0) \times 10^{19} \text{ atmos.}^{-1} \text{ cm.}^{-3}$$

Magnetic moment of one Bohr magneton :

$$\mu_1 = (h/4\pi)(e/m) = \frac{1}{4\pi}\left\{\frac{2\pi^2 c^3 F^5(e/m)^2}{R_\infty N_0{}^5}\right\}^{1/3}$$
$$= (0.9273_{45} \pm 0.0003_7) \times 10^{-20} \text{ erg. gauss}^{-1}$$

Magnetic moment per mole for one Bohr magneton per molecule :

$$\mu_1 N_0 = \frac{1}{4\pi}\left\{\frac{2\pi^2 c^3 F^5(e/m)^2}{R_\infty N_0{}^2}\right\}^{1/3} = 5585 \cdot 2_4 \pm 1 \cdot 6 \text{ erg. gauss.}^{-1} \text{mole}^{-1}$$

Mass of α-particle :

$$M_\alpha = (\text{He} - 2\text{E})/N_0 = (6.6442_2 \pm 0.0012) \times 10^{-24} \text{g.}$$

Mass of atom of unit atomic weight :

$$M_0 = 1/N_0 = (1.66035 \pm 0.00021) \times 10^{-24} \text{g.}$$

Mass of electron :

$$m = e/(e/m) = (F/N_0)/(e/m) = (9.1066_0 \pm 0.0032) \times 10^{-28} \text{g.}$$

Mass of ¹H atom :

$$M_{^1\text{H}} = {}^1\text{H}/N_0 = (1.67339_3 \pm 0.00031) \times 10^{-24} \text{g.}$$

Mass of proton :

$$M_\text{P} = ({}^1\text{H} - \text{E})/N_0 = (1.67248_2 \pm 0.00031) \times 10^{-24} \text{g.}$$

Radiation density constant :

$$a = 8\pi^5 k^4/15 c^3 h^3 = \left(\frac{V_0 A_0}{T_0}\right)^4 \frac{4\pi N_0 R_\infty (e/m)}{15 c^5 F^5}$$
$$= (7.569_{42} \pm 0.004_9) \times 10^{-15} \text{ erg. cm.}^{-3} \text{ deg.}^{-4}$$

Ratio mass ¹H atom to mass electron :

$$M_{^1\text{H}}/m = (e/m)({}^1\text{H}/F) = 1837.5_{61} \pm 0.5_6$$

* See long footnote in Section D. In my Aug. 1939 mimeographed list, the probable error of $R_0 T_0$ should have been 0.00008, in place of the stated 0.0008.

Ratio mass proton to mass electron

$$M_P/m = (e/m)\left(\frac{^1H-E}{F}\right) = 1836 \cdot 5_{61} \pm 0 \cdot 5_6$$

Second radiation constant:

$$c_2 = hc/k = \frac{T_0 c^2}{V_0 A_0}\left\{\frac{2\pi^2 F^5}{R_\infty N_0^2 (e/m)}\right\}^{1/3} = 1 \cdot 4384_8 \pm 0 \cdot 0003_4 \text{ cm. deg.}$$

Specific charge of α-particle:

$$2e/M_\alpha = \frac{2F}{\text{He}-2E} = 4822 \cdot 3_3 \pm 0 \cdot 5_1 \text{ abs.-e.m.u. g.}^{-1}$$

Specific charge of proton:

$$e/M_P = \frac{F}{^1H-E} = 9578 \cdot 7_7 \pm 1 \cdot 0 \text{ abs.-e.m.u. g.}^{-1}$$

Stefan-Boltzmann constant:

$$\sigma = a\dot{c}/4 = 2\pi^5 k^4/15c^2 h^3 = \left(\frac{V_0 A_0}{T_0}\right)^4 \frac{\pi^3 N_0 R_\infty (e/m)}{15(Fc)^5}$$

$$= (5 \cdot 672_{83} \pm 0 \cdot 003_7) \times 10^{-5} \text{ erg. cm.}^{-2} \text{ deg.}^{-4} \text{ sec.}^{-1}$$

Wave-length associated with one abs. volt * :

$$\lambda_0 = 10^{-8} c^2 (h/e') = \frac{c^2}{10^8}\left\{\frac{2\pi^2 F^2}{R_\infty N_0^2 (e/m)}\right\}^{1/3} = (12395 \cdot_4 \pm 2 \cdot_1) \times 10^{-8} \text{ cm. abs.-volt}$$

Wave number associated with one abs. volt:

$$s_0 = 1/\lambda_0 = \frac{10^8}{c^2}\left\{\frac{R_\infty N_0^2 (e/m)}{2\pi^2 F^2}\right\}^{1/3} = 8067 \cdot 4_9 \pm 1 \cdot 4 \text{ cm.}^{-1} \text{ abs.-volt}^{-1}$$

Wien's displacement-law constant† :

$$A = c_2/4 \cdot 965114 = 0 \cdot 28971_8 \pm 0 \cdot 00007 \text{ cm.deg.}$$

Zeeman displacement per gauss:

$$(e/m)/4\pi c = (4 \cdot 6699_1 \pm 0 \cdot 0013) \times 10^{-5} \text{ cm.}^{-1} \text{ gauss.}^{-1}$$

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* The factor 10^{-8} was accidentally omitted, in G.C. 1929, in the equation for λ_0.

† The factor 4·965114 is the root of $e^{-\beta} + (\beta/5) - 1 = 0$.

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THE GENERAL PHYSICAL CONSTANTS

By RAYMOND T. BIRGE

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CORRIGENDA

Page 90, last line. Insert " most " before " dependent ".

,, 103, line following equation (3). Insert " limiting " before " value ".

,, 107, line 10 from bottom. The sentence is clarified by the substitution of " the gamma value " for " one gamma ".

,, 112, line 4. J_{15} should be J_{15}.

,, 112, line 5. * should be †.

,, 112, line 20. † should be *.

,, 120, last line of table 2. " Goedieke " should be " Goedicke ".

,, 121, first line of first footnote. " 1932 b " should be " 1932 c ".

,, 122, footnote. Insert the symbol *.

,, 123, line 15 from bottom. " Birge, 1940 " should be " Birge, 1940 b ".

,, 125, first line of last paragraph. " $h/e' \pm$ . . . . . " should be " $h/e' =$ . . . . . ".

,, 126, Table *a*. In the unit of *G*, " cm. " should be " cm^2 ".

,, 130, Radiation density constant. The numerator " $4\pi N_0$ . . . . . " should be " $4\pi N^3_0$ . . . . . ".

,, 133, Manian, Urey and Bleakney. " 1934 " should be " 1935 ".