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ADDRESS

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A PLEA FOR LIGHT WAVES.

It is no doubt universally conceded that no era in the world's history has ever seen such immense and rapid strides in the practical applications of science as that in which it is our good fortune to live. Especially true is this of the wonderful achievements in the employment of electricity for almost every imaginable purpose. Hardly a problem suggests itself to the fertile mind of the inventor or investigator without suggesting or demanding the application of electricity to its solution.

Do events in this fast age follow so rapidly that the delays of even the fastest express trains and steamers are unendurable — the remedy is electricity. Is the labor of animals slow and expensive, or the carriage of the motive power itself, as well as its load, dusty, noisy and troublesome — the remedy is electricity. Are the barbarous tallow candle and the almost semi-barbarous use of gas for illumination totally inadequate to bridge over the hours of night — the remedy is electricity.

It would be wearisome to merely mention a tithe of the problems already solved or those on the eve of successful solution — nor is it at all necessary to insist on the richness of the harvest to be gathered by the successful experimenter in this fertile field. Neither is it surprising that so many world-famous men should have devoted almost their whole lives to the pursuit of this most fascinating study.

In the enthusiasm aroused by so many wonderful, beautiful and bewildering results, such varied and far-reaching discoveries in the vast fields of this subtile and powerful agent, it is not to be wondered at—or indeed entirely to be regretted—that possibly a great deal of attention has been diverted from the sister department of light. Undoubtedly, there have been many important developments and improvements in optical instruments — the microscope, the telescope, the spectroscope and the camera may be said to have reached the point of practical perfection — and it is equally true that the observations and discoveries made with the help of these have more than justified the high expectations which their advent created. Certainly the wonderful impulse the study of biology has received by the revelations of the microscope; the enormous increase in our knowledge of the size, distance and motions of the heavenly bodies, due chiefly to the little less than marvellous power and precision of our telescopes; the knowledge of solar and stellar physics — which a few years ago would have been thought visionary if not impossible — attained by the spectroscope, now so happily supplemented by the camera; the insight gained into the structure of matter by spectroscopic interpretation of the messages which its molecules impart to the luminiferous ether,—these are all even more truly wonderful and important than any of the astonishing marvels of electricity. But their original conception belongs to an era that is past.

If we except the exquisite results obtained in the manufacture and use of diffraction gratings and the very important work accomplished by the bolometer (a purely electrical invention, by the way), it may well be questioned whether, within the last twenty years, there has been a single epoch-making discovery or invention either in theoretical optics or in its applications.

It may, perhaps, be argued that the department of optics has been fairly completed; that its theory (though still imperfect in many important points) has been fairly well developed and the range of its applications fairly well understood. The unexpected wonders it has already accomplished make it somewhat hazardous to reply that these same observations may *now* be applied to electricity and magnetism. Still it is safe to say that at any rate the more important facts and laws as well as the more promising lines of the development of their applications, are now fairly well known, and that inducements to their further study and development are not wanting.

If, therefore, physicists would devote a larger share of their careful study to the completion of optical theories and to the application of light as an instrument of measurement and investigation—it need never be feared that there would be a lack of electricians to carry forward to their completion, upon lines already well developed, the principles and facts already known.

It is mainly with a view of attempting to interest brother physicists and investigators in this to me most beautiful and fascinating of all branches of physical inquiry that I venture to present a limited number of problems and I think promising fields for investigation in light, together with some crude and tentative suggestions as to their solution.

The investigations here proposed all depend upon the phenomenon of interference of light waves. In a certain sense all light problems may be included in this category, but those to which I wish to draw your attention are specially those in which a series of light waves has been divided into two pencils which reunite in such a way as to produce the well-known phenomenon of interference fringes.

The apparatus by which this is effected is known by the inconvenient and somewhat inappropriate name of "interferential refractometer." Among the many forms of the apparatus several are fairly well adapted to the work they have already accomplished, but all are open to serious objections. In all the forms which employ a broad luminous source of light, the plane in which the interference fringes are most distinct, is found to vary rapidly with slight changes in adjustment; in fact, it may happen that different portions of the same fringe appear at enormously different distances, so that it is impossible to fix the true position of a fringe or even to count the number which pass a given fixed point. This very serious objection is avoided by using, as the source of light, a narrow illuminated slit, but of course at a sacrifice of light and of convenience and simplicity. Both classes of instruments are open to the objection that the two pencils are very close together, rarely more than a centimeter apart. For some purposes this may be an advantage, but for many purposes it is a serious defect. Finally, none of the forms in general use are adapted to experiments in which there is a considerable difference in path between the two pencils.

The instrument which I had the honor of describing to the section at the last meeting is free from all the objections mentioned. It is simple in construction; with a little familiarity it is easily adjusted; it may be used with a broad luminous surface; the pencils may be separated as far as desired; and when properly adjusted *the position of the fringes is perfectly definite.*[1] As an additional advantage it may be stated that this is probably the only form of instrument which permits the use of white light (and therefore of the identification of the fringes) without risk of disturbing the position of either surface by contact or close approximation. It is chiefly this property which renders this instrument peculiarly adapted to the comparison of standards of length.

As this form of refractometer has already proved its value in several experiments already completed and in the preliminary work of others now under way, I may be permitted to recall the chief points of its construction and theory. A beam of light falls on the front surface of a plane parallel piece of optical glass at any angle—usually forty-five degrees—part being reflected and part transmitted. The reflected portion is returned by a plane mirror, normal to its path, back through the inclined plate. The second or transmitted portion is also returned by a plane mirror and is in part reflected by the inclined plate, thus coinciding with the transmitted part of the first pencil; and the two pencils are thus brought to "interfere."[2] A little consideration will show that this arrangement is exactly equivalent to an air-film or plate between two plane surfaces. The interference phenomena are therefore the same as for such an air-plate.

If the virtual distance between the plane surfaces is small, white light may be employed and we have then colored fringes like Newton's rings or the colors of a soap-film. If the distance exceeds a few wave lengths, monochromatic light must be employed.

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[1] It may incidentally be mentioned that extraneous reflections—such as usually accompany most of the phenomena of interference—may be almost entirely avoided by a transparent film of silver on the front surface of the glass plate where the rays separate; and accordingly the fringes in white light present a purity and gorgeousness of coloration that is only rivaled by the colors of the polariscope.

[2] A second plane parallel plate of the same thickness and inclination is placed (for compensation) in the path of the first pencil.

We may confine our attention to the case of two parallel surfaces. Here it can readily be shown that the fringes are concentric circles, the common axis of the rings being the normal passing through the optical centre of the eye or telescope. Further they are most distinct when the eye or the telescope is focussed for parallel rays. In any other case we are troubled with the same perplexing changes of form and position of the fringes as already noted.

If now one of the mirrors have a motion normal to its surface the interference rings expand or contract; and by counting the fringes as they appear or disappear in the centre, we have a means of laying off any given distance in wave lengths.

Should this work of connecting the arbitrary standard of length — the yard or the metre — with the unalterable length of a light wave prove as feasible as it is hoped, a next step would be to furnish a standard of mass based upon the same unit. It may seem a little like exaggeration to say that the solution of this problem may admit of almost as high a degree of accuracy as the preceding.

Suppose a cube, ten centimetres on a side, with surfaces as nearly plane and parallel as possible. Next suppose a testing instrument made of two parallel pieces of glass, whose inner surfaces are slightly farther apart than an edge of the cube.

The parallelism and the distance of these surfaces can be verified to a twentieth of a wave. Now apply this testing instrument to the three pairs of surfaces of the cube and determine their form, parallelism and distance to the same degree of accuracy. We have thus the means of measuring the volume of a cubic decimeter with an error less than one part in a million.

A very convenient and accurate method of making the determination of the weight of this volume of water at its maximum density has been suggested by Professor Morley, which consists in making the cube hollow, so that it will have almost exactly the same density as the water. On weighing the cube in water the excess of weight may be as small as required and may be most accurately measured by a very light and sensitive balance.

It does not seem extravagant to say that by some such plan as this we may obtain a standard kilogram which will be related to the standard of length with a degree of approximation far exceeding that of the present standard.

In the manufacture of plane surfaces, the only practicable method of testing their accuracy is to place the surface close to a standard plane and examine the appearance of the Newton's rings formed in the air film between them. This process when executed with proper care is undoubtedly the most accurate, and, indeed, is the only one possible for producing a standard surface; but it is attended with a number of inconveniences, among which may be mentioned the use of sodium light, the troublesome reflection from the first surface, the faintness of the fringes when the surface to be tested is metallic and the difficulty of getting rid of dust between the surfaces. All of these inconveniences are avoided by the use of the refractometer. For this purpose the apparatus is placed in a vertical plane, the lower mirror, which would then be horizontal, is replaced, first, by the test plane and then by the surface to be worked. The interference fringes in white light can then be conveniently studied while the surface is being corrected.

Another application of this apparatus, suggested by Professor Morley, is the measurement of coëfficients of expansion. For this purpose a bar is provided with silvered glass mirrors at each end (both facing the same way) and a second bar of the substance to be examined and of the same length is furnished in the same way. These are placed in the refractometer, so that the front mirrors, as well as the rear ones, give interference fringes in white light. The auxiliary bar is kept at zero. The bar to be examined is heated, and the fringes which pass at the front surface are counted as the bar expands, the fixity of the rear mirror being controlled by the colored fringes at its surface. In this method the bars may be a meter in length, and, therefore, the accuracy of the determination would be proportionally greater than in the celebrated experiments of Fizeau, in which the length was limited to a few millimetres.

Evidently the same disposition will also serve for measurements of coëfficients of elasticity, with the evident advantage of studying the elastic properties of the substance in thick rods or bars instead of small wires. This method of investigation is not limited to the determination of changes in length, but is quite as applicable to changes in density and optical properties; particularly to the effect on the velocity of light in solids, liquids and gases due to alterations in temperature, pressure, or magnetic or electrical conditions.

It may be mentioned that a great deal of valuable work has already been accomplished in this direction. I need only cite the very interesting and important experiments of Quincke on the compressibility of liquids; of Jamin on the variation of index of refraction of water and of Ketteler and of Mascart on the index of refraction of gases.

It seems somewhat curious that, while the immense advantage of the refractometer as an accurate means of measuring indices of refraction has been so fully appreciated, its use should be limited to differential measurements. Thus, while it is easy to measure indices of gases, since the difference in optical path for gas and vacuum is so small, the indices of solids and liquids can only be determined in thin plates, and the accuracy of such measurements must be limited to that of the estimation of the thickness. Such experiments may furnish the data for very interesting and important conclusions concerning the index of refraction and specially the anomalous dispersion of intensely opaque substances, such as metals and quasi metallic bodies. In such work the advantage of the interference method over the prismatic must be quite apparent; but I hope to show that for all measurements of refraction and dispersion—for solids and liquids as well as for gases—this method promises results which may far surpass those given by the prism.

Suppose a piece of the substance cut in the form of a plane parallel plate. The accuracy, parallelism and distance of the surfaces in wave lengths may be determined exactly as in the case of the proposed standard cubic decimeter. Next the nearest whole number of waves in the solid can be determined either by actual count or perhaps more conveniently by a method described in a previous paper. The residual fractional parts of a wave may also be found as there described, or by direct observation of the interference rings between the two surfaces.

The measurement of the index of refraction of a liquid is even more simple. A vessel is made with plane parallel sides, and the number of waves between the inner surfaces determined, first, when empty and then when filled with liquid.

The ratio of these two numbers will be the index of refraction. It will be noted that the only observations required in this process are the counting of two numbers; and as Professor Mendenhall has aptly remarked, a mistake in counting of a whole number is not an error but a blunder.

A blunder very easy to make, be it noted, in dealing with such large numbers as two or three hundred thousand, but whose chance of occurring may be indefinitely diminished by making several independent observations with different kinds of light.

Perhaps one of the most important applications of the method is the determination of the wave length of standard lines, both relative and absolute. In the paper above referred to, it was stated that the maximum difference of path at which interference fringes are visible, had been increased to over two hundred thousand waves (Fizeau's number is 50,000) by using light from highly rarefied sodium vapor in an exhausted tube. Since then I have observed interference under similar conditions with thallium with a difference of over three hundred and seventy thousand waves, and with mercury, with a difference of more than five hundred and forty thousand waves.

By repeated measurements of the diameters of the interference rings, the fractions of a wave can be found to within a fiftieth—which means that the number of waves in this fixed distance can be found to within less than one part in twenty-five million. Any two kinds of light of this degree of purity can be compared with this same precision. The accuracy of the measurement of absolute wave-lengths will of course depend on the accuracy with which the fixed distance can be compared with the standard meter; and this may be estimated as one part in two million.

The results of the remarkable work of Rowland do not claim a much greater degree of accuracy than one part in half a million for relative determinations; while the elaborate research of Bell on absolute wave-lengths claims but one in two hundred thousand.

We have thus at any rate a very promising method of excelling by far the best results that can possibly be obtained by the most perfect gratings.

It may possibly help to realize the very considerable superiority of this instrument over the grating—at any rate for the class of work in question—if I recall to your attention the fact that by its means it has been possible to show that the red line of hydrogen is a very close double. A short time ago the same was found true of the green thallium line. Both these lines are something like a fiftieth of the distance of the sodium lines, and like these are of unequal intensity. It is even possible to measure this very small interval easily to within a fourth of one per cent. Following are the numbers obtained for the distance from one maximum or minimum of distinctness to the next:—

	Maxima	Minima
	1.025	1.012
	1.050	1.012
	1.025	1.050
	1.017	1.033
	1.000	1.025
	1.038	1.000
	1.021	1.017
Mean	1.025	1.021

One unit means a distance of 24.6 mm. which gives for the average distance 25.2 mm. and for the ratio of the wave-lengths of the two lines 1.0000212.

Closely connected with the preceding investigations is the study of the effect of the temperature, thickness, and density of the source on the composition of the radiations, as shown by the symmetrical or unsymmetrical broadening of the spectral lines and the consequent shifting of their mean position. This question has quite recently been taken up by H. Ebert and the results he has already obtained are very promising. The principal effects noted are: first, the shortening of the difference of path at which interference can be observed; secondly, the shifting of the fringes as the mean wave length changes. Ebert has shown that the interference method is far more delicate than the spectroscopic; and by its means he has established two conclusions which, if verified, are of the greatest importance—namely; first, that the chief factor in the broadening of the spectral lines is the increase in density of the radiating body; secondly, that the broadening, in all the cases examined is unsymmetrical—causing a displacement of the line toward the red end of the spectrum. The importance of these conclusions, in their relation to the proper motions of the heavenly bodies and their physical condition, can hardly be overestimated. The value of results of this kind would, however, be much enhanced if it were possible to find a quantitative relation between the density of the radiating substance and the nature of its radiations. In the case of hydrogen enclosed in a vacuum tube this could readily be accomplished. It may, however, be objected that it would be difficult in this case to separate the effects of increased density from those due to the consequent increase in the temperature of the spark. The problem of the temperature of the electric discharge in rarefied gases is one which has not yet been solved. In fact it may seriously be questioned whether in this case temperature has anything to do with the accompanying phenomena of light; and it appears to me much more reasonable to suppose that the vibratory motion of the molecules is not produced by collisions at all but rather by the sudden release of tension in the surrounding ether.

Whether true or not, the results obtained and interpreted by this hypothesis would be of great interest. The method could be applied directly to any substance, mercury for instance, for which the relation between the temperature and the pressure is known. For substances for which this relation has not been established, as sodium, thallium, etc., the density may be found by heating the substance in a tube closed with plane parallel glass ends and measuring its index of refraction. The density will be very approximately proportional to the excess of this index over unity.

Aside from its application to this problem, it seems highly probable that this method of measuring the density and pressure of vapors may be made to yield excellent results in cases where these are far too small to be measured directly.

It may not be entirely out of place in this connection to present a few observations concerning the causes of the broadening of the spectral lines. It seems to me that by a thorough and systematic study and discussion of this phenomenon we have a possible means of materially increasing our knowledge of a subject, of which we are at present in almost total ignorance: namely, the real action of the forces and motions of vibrating atoms and of the ether which transmits these vibrations in the form of light.

The possible causes of the broadening of spectral lines are as follows:—

First, the addition of vibrations of periods differing from the normal period, due to the influence of neighboring molecules; secondly, the effect on the wave length due to the velocity of the molecules.

It is evident on considering the second cause, that it could not possibly account for more than a small fraction of the effects observed. For example, to effect a change in wave-length corresponding to the difference between the two sodium lines, would require velocities of the order of three hundred thousand meters per second, over a hundred times as great as the velocities given by the kinetic theory. But it is also clear that when a gas is so rarefied that the first cause cannot possibly produce any perceptible effect, the second cause would be quite sufficient to limit the fineness of the lines, and to impose a definite limit to the difference of path at which interference is visible; and it is worthy of note that the actual limits observed are of the same order of magnitude as those given by the kinetic theory.

There is still a third cause which might limit this distance, but which would not have any effect in broadening the lines; namely, the diminution in the amplitude of the vibrations after collision. There must be such a diminution and it would evidently be the more marked the more rapidly the energy was transferred to the ether, that is, the brighter the light. If the effects due to this cause alone could be separated from the others it would be possible to measure the diminution in amplitude and therefore the rate of transference of the energy. Thus it may be shown that a vibrating sodium atom gives up to the surrounding ether less than six millionths of its energy at every oscillation.

Returning to the first and chief cause of broadening, it may be remarked that the universal opinion of scientific men seems to be that during collisions between the molecules the vibrations are entirely "irregular;" and the longer the collisions last in proportion to the time between collisions, the more intense will be the light due to these "irregular" vibrations, and consequently the broader the lines and the more impure the light.

The following consideration would seem to show that this explanation will not hold.

If, in the refractometer, so frequently referred to, white light be used, all phenomena of interference are lost to sight when the difference of path exceeds a few wave lengths, for the well-known reason that the fringes due to the infinite number of different kinds of light are superposed, thus producing a uniform illumination. If now this light be analyzed by a spectroscope, the spectrum will be traversed by well-marked interference fringes which are the finer and closer, the greater the difference of path of the interfering pencils. Now, I have observed such interference fringes in the white light from the incandescent carbons of an arc light when this difference amounted to thirty thousand waves. And it may be added that this limit was reached by the closeness of the lines rather than by their indistinctness.

It seems to me that the only conclusion which can be drawn from this experiment is that in the light from an incandescent solid the vibrations must be *isochronous* for at least thirty thousand waves. The same observation applies also to the so-called "irregular" vibrations of the broadened sodium lines, for the same limit (about thirty thousand waves was also observed in this case). The inference seems irresistible that the broadening is not caused by "irregular" vibrations, but by the addition of vibrations whose intensity is greater the nearer their period is to that of the normal vibrations and which may be almost if not quite as regular as the normal vibrations themselves.

If these conclusions be granted we must profoundly modify our conception of radiation in solids and liquids, or at least that part of it which supposes that such radiation produces a continuous spectrum because the molecules have no "free path," and, therefore, no proper periodic vibrations.

What, then, is the nature of the effect produced by the collision of molecules? If it be to produce or reinforce vibrations differing from the normal type, it must be granted that these new vibrations are *isochronous*. If so, they must be due either to a change in the form or in the mass of the molecule itself produced by collision, such changes tending to revert back to the type when the frequency of the collisions is not too great. The only alternative is to suppose that the molecules differ among themselves, either in form or weight. In this case, the molecules agreeing most nearly with the type and hence having a proper period differing but little from the normal would be more easily set in vibration than the others, or their vibrations once started would outlast the others. Accordingly, when a gas is very much rarefied, the collisions are few, hence only the typical vibrations persist; but when the collisions are frequent the other vibrations are also sustained.

I fear I have wandered so far from the subject of this address, if such a name be at all appropriate, ever to return; and, though many other interesting and important applications of light-waves clamor for recognition, I fear they would be wearisome even to enumerate.

I fear also that it may with some justice be said that I have made a plea for my own instruments and theories, rather than "a plea for light waves;" and still more that I have presented many crude and half digested ideas, when it would have been more to the purpose to present facts and results of diligent study and careful experiment.

In extenuation and in conclusion I can only hope that if I have created the slightest interest in the matters here presented for your consideration, if there be any chance that even a few of the seeds may germinate, grow, blossom and bring forth fruit worthy of acceptance, my purpose will be accomplished.