official, naval and military life are features which have always constituted a charm and attractiveness to the many American biologists who have already experienced them.

Another consideration very important to my mind is the educational value to young biologists—prospective investigators—to be obtained from such an establishment. The broadened conception of the possibilities of the animal and plant world which even a short experience within the tropics affords is very desirable. To wander amid the beauty and luxuriance of life on a coral reef, or pass amongst the intricacies and remarkable adaptations within a tropical forest, gives an inspiration not to be experienced in temperate regions. For this purpose a station having the greatest variety of both land and marine conditions is obviously most desirable. I conceive that many professors will think it worth while to take or send their most promising students, the idea of a general acquaintance with a tropical fauna and flora predominating over that of discovering material for research. For many years such has been the custom of Professor Brooks with regard to his students, and the conditions found in Jamaica have most nearly approached the ideal.

J. E. DUERDEN.
UNIVERSITY OF NORTH CAROLINA.

SHORTER ARTICLES.

THE PHYSICAL BASIS OF COLOR.

AT the present time no one, I think, questions the validity of the wave-theory of light. We may hold various views as to the nature, or even the existence, of that omnipresent medium, the ether; and the physicist, though unable to get along without it, is continually changing his conceptions of its manner of action; but the broad general principles upon which the theory is built remain unshaken.

The backbone of the theory is *periodicity.* Innumerable measurements of extreme accuracy have been made whilst experimenting in the various domains of optics, all of which agree in the conclusion that light, in its very essential nature, is *periodic;* and the simplest image one can form in his mind of such a phenomenon is a wave-motion, while the simplest method of representing it mathematically is by the circular functions of the sine and cosine.

The three quantities which determine a wave-motion are its amplitude, its wave-length (and, therefore, its frequency or period), and the form or contour of the waves. The mechanical measure of the intensity is proportional to the square of the first of these, while the sensation of color is in some way indissolubly connected with the second—possibly, also, with the third, though I do not know of any direct evidence on the question.

It has been usual to assert that color is purely a function of the wave-length, just as pitch is a function of wave-length in acoustics. Light of one wave-length would excite one color, light of another wave-length, a different color, etc. In an article on 'Color Saturation,' which appeared in the *American Journal of Psychology* (Vol. VII., No. 3, April, 1896), my friend and colleague, Professor A. Kirschmann, expresses dissent from the view generally accepted (by physicists, at any rate), and it is the question raised by him that I wish to briefly consider.

Dr. Kirschmann remarks: 'It is claimed that light of one certain wave-length causes the impression of red, another that of green, etc.; but this is mere hypothesis, for *nobody has ever seen light of one wave-length.*' Perhaps it would be fairer to state the proposition as I have done in the preceding paragraph. The physicist surely does not claim that he has ever worked with light of absolutely a single wave-length, though we shall see how near he has been able to approach to it. If a writer on the wave-theory should indulge in such superficial dogmatic statements, he must not be taken too literally, and the true value of the theory must not be judged therefrom.

Dr. Kirschmann supposes a 'pure' spectrum, a meter in length, to be produced on a screen, and discusses the nature of a narrow band of this image 1/100 of a millimeter in width. Taking the number of vibrations per second, corresponding to extreme red and extreme violet, to be 412 million million and 790 million million, respectively, we see that the total change of vibration-frequency as we pass from one end of the spectrum to the other is 378 million million. Now, the width of the band under consideration is 1/100,000 of this distance, and hence in crossing from one side of it to the other there is a change of frequency of approximately 3,780 millions per second. Ordinarily the light from such a narrow band of a 'pure' spectrum would be considered very homogeneous, *i. e.*, of very approximately a single wave-length; but looked at from the point of view of number of vibrations per second, this variation in frequency of 3,780 millions per second, which is to be found amongst the constituents of the light, would at first sight appear so large that we should not be justified in saying that the light from the band is practically of one wave-length. This view is, indeed, held by Dr. Kirschmann, but I hope to show that it is untenable.

Let us look at it in another way. As we pass from the blue to the red end of our spectrum the wave-length increases by an amount equal to itself—by 100 per cent.—this increase being uniform if the spectrum is *normal*. Hence in passing over a band 1/100 of a millimeter in breadth the wave-length increases by 1/100,000 part of itself. Thus in the light which comes from this narrow space there is, if we consider the case in absolute mathematical strictness, a superposition of waves of different lengths, but yet the waves are extremely uniform, since the longest is only 1/100,000 part greater than the shortest. Stated as an absolute length, this difference between the longest and the shortest waves is but one five-thousand-millionth part of an inch.

The size of the number expressing the measure of any quantity depends only on the unit used, and may be quite meaningless when we come to deal with actual sensations. For example, the length of two rods may differ by but the millionth of an inch, a quantity smaller than the error of experiments made to compare them, so that from our physical measurements we should say that the rods were of the same length; and yet if we chose the diameter of a molecule as our unit of length, the infinitesimal difference between the lengths of the rods would be expressed by millions of millions.

In the light under consideration there is strictly a variation in the wave-lengths, but relatively this is an extremely small quantity—too small to be considered as an essential factor in determining the nature of our sensation. Would it be reasonable to suppose that if there were absolutely no variation at all the effect on the eye would be different? Experiments do not indicate that the eye has such marvelous delicacy that it can detect any such infinitesimal changes.

In a pure spectrum a meter long the widths of the colored bands in the most sensitive portion are given by Rood* as follows:

	Normal Spectrum.	Prismatic Spectrum.
Orange-yellow	26 mm.	20 mm.
Yellow	13	10
Yellow-green and green-yellow	97	104

Of course it must be understood that these quantities are not absolutely definite magnitudes. They are simply the careful estimate of an accomplished scientist and artist.

An attempt was made to test the sensitiveness of the eye in the following way: By means of a narrow slit and a direct-vision train of prisms in front of an electric arc lamp, a spectrum 80 centimeters long was thrown on an opaque screen, in which was an opening about a centimeter wide and 3 centimeters long. By shifting the screen, light from any desired portion of the spectral image was allowed to come through, and it was then received on a finely ground glass plate. By means of black paper strips the portion of the ground glass thus illuminated was further restricted to two narrow bright bands approximately 1.5 millimeters in width. When working in the yellow it was found that these bands could be separated by a dark strip 1.5 millimeters wide, and still be indistinguishable from each other. A second observer, experienced in optical work, agreed in the above result. Thus the eye was unable to distinguish between the colored bands whose centers were 3 millimeters apart. Now, this distance

---

* Rood, 'Modern Chromatics,' p. 24.

is 3/800 or 1/266 of the total length of the spectrum, and hence, in passing over it, the wave-length varies by approximately 1/266 of itself, and yet the eye could detect no difference. This certainly seems to indicate that the minute change of 1/100,000 of the wave-length, above referred to, is not at all able to alter or control the chromatic sensation experienced by the eye. The spectrum used in this experiment was a comparatively pure one, since in the yellow band, one centimeter wide, admitted through the opening in the screen, the bordering colors, orange and green, could clearly be seen at the edges.

But it might be remarked that Dr. Kirschmann's case is a purely hypothetical one, and that my arguments are quite as theoretical, and so it will be interesting to see just how far we have been able to actually go towards obtaining a perfectly monochromatic light.

As is well known, the spectrum given by a grating, with proper adjustments, is normal. The distance between two portions of the spectral image is proportional to the difference of the wave-lengths proper to these portions. Also, the spectrum of a sodium flame (given, for instance, by burning ordinary salt in a Bunsen flame) consists of two bright lines separated by a narrow space. The mean wave-length of the light forming one of these lines is approximately 1/1,000 greater than that forming the other.

When working with a plane reflexion grating, containing 14,400 lines ruled on a space two inches wide, it was observed that the interval between the two sodium lines was about ten times as wide as either line. Now, as we pass across the interval from one line to the other there is a variation of 1/1,000 in the wave-length, and hence in passing from one side to the other of a bright line the change in wave-length is *not greater* than 1/10,000.

But the narrowing of the spectral lines is directly proportional to the total number of rulings on the grating, while the interval between two lines varies directly as the closeness of the rulings; and in some gratings used by Professor Rowland there were as many as 120,000 lines on a space of six inches, *i. e.*, 20,000 to an inch. Here the rulings are eight times as numerous as in the former case, and so the bright lines of the spectrum should be only one eighth as broad; also, the rulings are three times as close, and so the interval between the sodium lines should be increased threefold. Thus a grating like this should give a spectrum in which the interval between the sodium lines is over 200 times the width of either bright line. If such were the case, we could conclude that the wave-length of the light from each sodium line did not vary as much as 1 in 200,000.

Rowland,* however, remarks that there is a limit to the applicability of this line of reasoning, and that the width of a spectral line given by a grating depends not only on the width of the slit and the number of rulings on the grating, but also on the *true physical width* of the line. But it is quite certain that at least one half the high resolving power above referred to was reached by him in his experiments (*i. e.*, that he really obtained a spectrum in which the width of either sodium line was but 1/100 of the interval between them), since he reports having actually photographed some lines in which the variation in wave length was not more than 1/80,000.

But the best test for the homogeneity of any light is to determine what is the greatest difference of path which two portions of it may have and still give interference fringes. With white light this is very small. In the ordinary apparatus for observing Newton's rings only eight or nine rings can be seen with white light, and for the ninth ring the difference of path is about 1/200 of a millimeter, a very minute quantity. If sodium light be used, many more rings can be seen; and indeed interference has been observed† with it with a difference of path of 200,000 waves, or over ten centimeters.

By using the green line ($\lambda = 5461$), obtained on decomposing by a prism the light

---

* *Philosophical Magazine*, 5 ser., Vol. 16, p. 209. 1883.

† By A. A. Michelson and E. W. Morley. See address before the American Association for the Advancement of Science, Cleveland meeting, 1888, by A. A. Michelson.

emitted by mercury vapor in a vacuum tube made incandescent by the passage of an electric discharge through it, Perot and Fabry* were able to secure interferences with a path-difference of 43 centimeters, which is equal to 790,000 wave-lengths; while Professor Michelson, of Chicago, who is preeminent in this work, and whose interferometer is the instrument generally used in such researches, informs me that he has obtained interferences, with the light from this same mercury line, when there was a path-difference of 840,000 wave-lengths. In this case the variation in wave-length could hardly have been greater than one part in 1,000,000—truly an extraordinarily close approach to perfect uniformity.

Now, in all these experiments there was no sign of the color disappearing as the wave-lengths approached more and more nearly to equality. Indeed, Professor Michelson's observation is that as the light approaches perfect homogeneity the intensity of the color sensation is slightly increased!

Light of a single wave-length in optics corresponds to sound of an absolutely pure tone in acoustics. A well-made tuning-fork is by no means a perfect instrument, and yet it emits a note closely approximating a pure tone; but such a fork is just as efficient in producing the sensation of sound as the most complex mixture of wave-lengths given forth by any instrument.

Is it possible, then, that these little variations in the wave-lengths, and not the wave-lengths themselves, are the essential physical cause of the sensation of color? Surely it would be just as reasonable to believe that, by removing all the impurities from water, or nitric acid, or any other definite chemical compound, these substances would lose the *taste* characteristic of each.

I think it unreasonable, therefore, to contend that for the production of the sensation of color it is necessary to have a superposition of waves of different lengths. It is quite true that two color sensations which are indistinguishable from each other may be produced in different ways—either by light of approximately uniform wave-length or by a combination of quite different wave-lengths. For instance, a mixture of red and green will give an orange which, as a sensation, is indistinguishable from spectral orange. In Maxwell's phrase, they are *chromatically* the same, but *optically* they are different. In such matters the eye is much inferior to the ear, which can, in many instances, resolve a compound sound into its constituents; and it would be hard, indeed, to produce a combination of sounds which would so perfectly simulate a simple tone that one could not distinguish between them when they were heard together.

But I can not see why color-quality should not be considered simply as a function of the wave-length. Dr. Kirschmann says: "It may just as well be—and the probability for this supposition is even greater—that the color-quality is a function of the superposition of wave-lengths, so that to every qualitative difference in spectral colors corresponds a difference in the mode of superposition." I think the facts I have given show conclusively that a spectral color is not at all dependent on any 'mode of superposition.' We need no such idea to define spectral colors, and the introduction of it seems a needless complication.

Let us now briefly consider Dr. Kirschmann's 'inverted spectrum,' and his application of the principle of superposition to explain the true position of purple.

When we view through a prism a dark line of the proper width, on a white background, we see a kind of 'inverted spectrum,' with purple in the middle. With Dr. Kirschmann, 'we must agree that the existence of this color does not prove anything more than that the mixture of the ends of the spectrum gives purple.' That is all the experiment appears to me to prove.

If a narrow bright band be viewed through a prism we get a 'pure' spectrum. As we all know, the term 'pure' here has not a very definite meaning. It signifies that certain optical requirements have been complied with; and if we are using sunlight the familiar test for purity is the presence of the Fraunhofer dark lines. If now the bright slit be gradu-

---

* *Comptes Rendus*, Vol. 128, p. 1223, 1899.

ally widened the colors rapidly lose their pure hues, the change at the central portion being most marked at first. Finally, if the slit be made wide enough, the color entirely disappears from the middle part, while at one end (accepting Dr. Kirschmann's description) there is a band of red, orange and yellow; at the other, one of blue and blue-green. The explanation of this is well known. A wide slit may be considered as the sum of a great number of narrow ones, each of which gives rise to a pure spectrum, but these spectra are superposed, producing perfect white in the middle and the colors mentioned at the ends.

Dr. Kirschmann explains the absence of green proper by saying that its two neighbors, blue and yellow, are here separated, and so are deprived of the power of cooperation, just as in the ordinary spectrum red and blue are separated, and thus can not produce purple. But the nature of the mixture is very different in the two cases, and even though we should grant that the sensation of green is due to the superposition of blue and yellow, we should hardly be justified in concluding that purple should be considered as simple a color as green, since blue and yellow have wave-lengths nearly equal, while the wave-length of red is approximately twice that of blue. We *know,* from physical considerations, that purple is not simple like spectral green.

From the explanation given above it would appear that the 'inverted spectrum' is far from being a pure one, though Dr. Kirschmann thinks that this statement can be proved unfounded. When we examine with a spectroscope the light reflected from a very thin sheet of mica, we see the spectrum crossed transversely by a number of dark bands. This phenomenon is one of the large interference-family of 'colors of thin plates,' and is ordinarily known as the 'channeled spectrum.' Many investigations have been made on it. Dr. Kirschmann states that he was able to obtain these 'channels' in the 'inverted spectrum.' For the production of such, however, the spectrum need not by any means be pure. In a single experiment with a direct-vision spectroscope and a sheet of mica about 1/100 of a millimeter thick, I was able to see the 'channels' when the slit was 0.6 millimeter wide, while to show the *most prominent* Fraunhofer dark lines, and thus have a slight approach to purity, the slit had to be less than 0.25 millimeter wide.

To use the channeled spectrum for the purpose of measuring wave-lengths, as suggested, is not very convenient, since the thickness of the thin plate, its index of refraction, the angle of incidence (or of refraction), as well as the 'order' of the interference, would all have to be determined. But if *some* wave-lengths are known, *others* may be conveniently located by this means. A very elegant application of this method was made by Maxwell* in his classical experiments on the mixing of spectral colors. His thin plate was a layer of air between two plane plates of glass, and by the channels in the spectral image shown at the end of his color-box he was able to calibrate in wave-lengths an arbitrary scale put across it.

C. A. CHANT.

DEPARTMENT OF PHYSICS,
UNIVERSITY OF TORONTO.

SURFACE TENSION; MOLECULAR FORCES.

IN deducing the surface tension equations by the method of Laplace we start with the assumption that the force with which one element, dv, of the liquid attracts another δ element, v, is

$$F=\frac{1}{k}\rho^2\cdot dv\cdot \delta v\cdot f(r)$$

(usually the k is wrongly omitted), where ρ is the effective density of the liquid, and $f(r)$ is the law of the variation of the force with the distance. Finally we find that the surface tension is

$$T=\frac{1}{k}\rho^2 I,$$

where I is a definite integral (and hence a constant) derived from $f(r)$. In measuring the surface tension of liquids we are usually content to stop when we have found T, or we endeavor to find relations between the values of T for different liquids. We can do much

---

* *Phil. Trans. of R. S.*, 1860. 'Scientific Papers,' Vol. I., p. 410, § 6.