PRACTICAL LOGIC AND COLOR THEORIES

BY CHRISTINE LADD-FRANKLIN

Columbia University

The psychologists, when they discuss reasoning at all (and some of them hardly give it passing mention), take the ground that the kind of reasoning that interests them is something very different from the cut and dried formulæ of the logician. The reason for this quarrel between two honorable branches of science is simply, of course, that the psychologist has the inveterate habit of including in his term reasoning the search for what I have called (a technical term) the "adequate" premises—which is half the battle, to be sure, when one is engaged in thinking out a solution to real difficulties. The pure logician, on the other hand, cares nothing for this aspect of the matter—he is concerned only with the validity of structures of premises. I propose to use the term *practical logic*, in a technical sense, for the psychologist's logic, and to call that of the logician theoretical logic, or pure logic. This simple device of giving two names to two different things ought to have the effect of modifying the contemptuous terms in which the psychologists sometimes discuss the logicians.

Why should not the scientist, whose constant occupation is practical reasoning, devote some time, now and then, to polishing up the tools of his trade? Why should he not make a special study, when occasion offers, of the great quagmires of bad reasoning that, in various fields, lie behind him? And it is exactly the psychologist who will have the best material for this study. It is safe to say that there has never been a subject of scientific research that has offered such a good field for studies of this kind as does the subject of color sensation. Both the old theories and the new—both the current theories and the non-current—are rich in not

only common errors of logic but far more, of course, in sins against the fundamental principles (axioms, as G. E. Müller calls them) of the neuro-psychic correlation. And as it is instances of bad grammar that we choose in order to teach the avoidance of errors of speech, so it is the mistaken theories of color which have high pedagogical value in sharpening up the wits of the intending reasoner. As matter of fact, moreover, it is some of the latest of the so-called theories which are quite the best pedagogically; they will in general have been devised with knowledge of the most recent ideas in physics, physical chemistry and physiology—all indispensable to the knowledge of color though not sufficient—and their errors in those plainest of methodological considerations which govern the making of theories in general and of psychological theories in particular will be found to be all the more curious and striking.

As a general principle, whoever notices some one of the innumerable effects of physical light that has not yet been made the basis of a color theory immediately utilizes it as such, quite ignoring the basic anomaly of color sensation—the existence of the "vanishing" color pairs—the fact that of the six possible dual color blends, the blue-greens, etc., there are two, the yellow-blues and the red-greens, which have never been experienced, and that in consequence (so different from what we have in taste) no tertiary blends and no quaternary blend have ever been sensed.

Theories which do not involve some hypothesis for the explanation of this curious circumstance are from the beginning negligible, and of this character are unfortunately most of the color theories of which English physical science has lately been so prolific.[1] Another class of negligible color theories is that of the authors who fail to have noticed that while red and green are (like yellow and blue) a disappearing color pair, they are not white-constitutive but yellow-constitutive; this may be, from the point of view of logico-æsthetics,

[1] In fact a table of the basic facts of color (such as Professor Warren gives in his 'Human Psychology') should always be kept before one when he sits down to devise a theory of color.

a misfortune, but it is nevertheless a fact, and there are many theories that are vitiated by ignoring it. The theory of Helmholtz falls, of course, into the first of these categories, that of Hering into the second. The latest of theories—that of Fröhlich, whose brilliant discovery of the oscillatory character of the nerve impulse in the optic nerve of the Cephalopod led one to expect much from an announced color-sensation theory—suffers, at the beginning, from the defect that it adopts the erroneous view of Hering that red and green are complementary (white-constitutive) colors.

As, in making a study of the proposed new international auxiliary language (Hilfsprache) Ido, you will have a keen intellectual pleasure in seeing how apparently insuperable difficulties in the way of simplification have been overcome by the 'Delegation' of clever language-logicians appointed by the International Congress of Philosophy of 1900, so in studying the wreck of color-theories which have received, and are still receiving, solemn consideration at the hands of the critic, you will have a keen intellectual pleasure in noting what remarkable vagaries the mind of scientific man has now and then been capable of. For the lectures on Logic which I am permitted to give at Columbia University, I find good store of illustrative material from the content of my lectures on theories of color-sensation.

Before taking up a discussion of the logic of color, I must remind my reader of the very essential reforms in color terminology which I have been urgently recommending since 1913, and which the Optical Society of America seems now to be taking up. Half the battle, in taking in the ideas regarding color which are necessitated by modern knowledge of color —neither Helmholtz nor Hering knew the truth regarding, for instance, the development of the color sense—consists in expressing those facts not in the primitive language of an earlier day but in precise, scientific terms

1. Not only do I insist upon regularly using the terms chromatic and achromatic for the specific or toned (as the Germans say) color-sensations, and for the non-specific, tone-

less ones, respectively—most writers, following Wundt, do this already; but I also have boldly proposed the use of the shorter expressions chroma, achroma, in the corresponding senses.[1] It follows that the vision of the totally chroma-blind is not monochromatic but achromatic. The physicist should speak of homogeneous, not monochromatic, light rays, when they are (as they nearly always are) a dual color blend, blue-green, etc. An exact monochromatic yellow may happen on the other hand to come from the mixture of a troop of very non-homogeneous lights.

From chroma and achroma follow at once, of course, chromaticity and achromaticity, which are far better words then saturation and non-saturation. Saturation with what? A blue-green might be saturated with blueness or with greenness; other things can be saturated with sweetness; the word lacks the specific meanings that are conveyed by chromaticity and achromaticity. As, in the case of a series of blue-greens, we speak of the degree of blueness and of the degree of greenness, so here we express the degree of chroma and of achroma by the terms chromaticity and achromaticity. The free use of the term chroma will leave that terribly ambiguous word *color* to be used in its inclusive sense. To the age-old question, then, "Are black and white colors?" the answer is "Yes."

It is hard to fully disabuse the mind of the idea that external objects are colored. Color arises only after specific light-rays have effected their alchemy of the retina (and not always then). Instead of red, yellow, etc., when one means the specific light-ray combinations, one should call them erythrogenic, xanthogenic, chlorogenic, cyanogenic and leucogenic. How could chemistry have ever reached its present stage of development if chemists had balked at hard names?

The terms unitary color and color-blend explain themselves, and they are indispensable. A unitary red is the same thing as a red which is a psychological element, but that a given red is neither bluish nor greenish is the primary,

[1] The plural of *chroma* is of course *chromata*, save for such individuals as are in the habit of saying *scotomas* for *scotomata*.

direct deliverance of consciousness—that it is an *element* contains the addition of theory. Both terms are needed, but unitary comes first. Max Meyer uses the words singular color and dual color—also good and significant terms. But it is only of purely *chromatic* blends that we can say that they are dual only—a gray-green-blue is a quadruple *color*-blend. To the age-old question, then, "How many different chromatic sensations are there in the rainbow (or in the world)?" the answer is, "Four." The fatal confusion involved in the ineradicably wrong use (since Hering) of the terms luminosity and brightness I shall discuss at another time. The lately proposed term "brilliance" merely makes matters worse.

The universe in which we find ourselves consists of three regions—the psychical, the intracorporeal and the extracorporeal. Color phenomena, like other phenomena of the conscious organism, include in the first place the physical, in the last place the psychical, and in the middle place the physiological. For the two 'synapses' (if we may use this word in a metaphorical sense) which constitute the transition points between, respectively, these three regions, we need two names, and we shall also have need occasionally for a term expressing the final, non-direct connection between the extracorporeal-physical and the psychical. I suggest, in the interest of precision, the use of the term psychophysical parallelism for this latter sense (better still would be perhaps physico-psychic parallelism) in which 'physical' is allowed to have the technical sense of extracorporeal physical.[1] For the sense in which psychophysical parallelism is usually used, and for which many writers have already substituted the better term psychophysiological parallelism, I propose to say, since it is in fact only the nerve processes (and chiefly the cortical nerve processes) that are concerned, the neuro-psychic correlation. (This is the seat of Professor Warren's Double Aspect theory.) I shall then have the word physico-physiological for the connection between, *e.g.*,

[1] Wundt explicitly uses the term in this sense.

the physical light-rays and the physiological (in the first place the retinal) processes which they excite. This restricted use of the term physical has perfectly good precedent; we do not think of the 'physicist' as being engaged in the study of physiology, as a whole—in spite of the fact that the dividing line between physics and some parts of physiology has become very narrow.

I am now able to point out that the psychophysical 'parallelism' in conscious beings is a very different thing in the different sense regions. It holds, for instance, in the sense of taste; for the different taste-qualia we have chemical substances which are capable of stimulating the different sense organs on the tongue. What could be simpler? No 'theory' of the taste-sense is required, or has ever been proposed. For the temperature sense the situation is the same—there are different sense organs for the different sensations, heat and cold. In sound also the physical stimulus—an ordered series of vibrations of different frequencies—runs parallel to an ordered series of tones of different pitch. There are no mixtures of these frequencies which give you a *vanishing* sensation—all possible mixtures are recognized as tonal blends.

But the philosophers quite forget the subject of color when they virtually regard it as coming also under the head of a psychophysical parallelism (in the strict sense of the term). Note what becomes of the parallelism here. A single sensation, say a gray-green-blue, can be excited by a thousand different combinations of electro-magnetic vibratoins—by a million, rather—and certain pairs of vibrations (what ought to give you, for instance, a blue-yellow) vanish, as such, and give you a whiteness instead. It is as if, when you struck a *d* and a *g* on the piano together, you heard neither of these notes, but got a sensation of plain non-specific noise instead. This is what might be called a psychophysical perpendicularity, or topsy-turvyness, rather than a psychophysical parallelism. What to do? It remains for the physiologist (but especially the student of photo-chemistry) to step in and to find out what can be done in the way

of bridging this sad chasm. As matter of fact, our knowledge of this whole situation is such, at the present time, that, with the aid of the one simple hypothesis which forms the basis of my theory—that of a light-sensitive substance which undergoes development from a simpler form to a more complex form—we can make the whole situation fairly comprehensible. And the evolution of a chemical substance is not a far-fetched conception; we have, in the very organs which contain this light-sensitive substance, an instance of structural evolution. That the cones are more highly developed rods was a constituent part of my theory from the beginning:[1] it has since been turned from hypothesis to fact by the work of Ramón y Cajal. It may be mentioned in passing that there is no occasion for speculating about the significance of *rod-shape* and *cone-shape* (as does Mr. Troland, at some length; for it has been made out by Pütter that the one *difference-character* between them that has been preserved throughout the animal kingdom (since these visual elements came in) is simply the character of the connection with the second neuron—the rods have a knob-like ending, the cones have a dendritic ending. I said ('Dictionary of Philosophy and Psychology,' 1907, Art.Vision, p. 767) that these organs ought not to be called rods and cones (the cones are not even cone-shaped in the fovea), but that they should rather be named from their chief characteristics—knob-ended and finger-ended organs. This prevision of mine has now (v. Graefe, 'Handbuch der Augenheilkunde') been confirmed by Professor Pütter. The greater specificity of connection in the cones, of course, suggests a greater specificity in the character of the nerve-impulse, but until the neurologists have finally told us whether the fibrillæ in a single nerve fiber are capable of specific conduction or not, one cannot say whether this has significance. There is another thing that should be mentioned here: the synapses between the bipolar cell and the third neuron occur in five perfectly distinct levels (Luciani); this naturally

[1] Mr. Troland proposes to 'supplement' my theory 'by the assumption of an independent rod response for twilight vision.' 'The Enigma of Color Vision,' p. 13, 1921.

suggests a provision for the specific conduction of the impulses which end in the five specific sense-*qualia*—the four chromata and the whiteness sensation. (Blackness—a non-light sensation—cannot be connected with anything conveyed up from the retina, on account of the fact that it is of one intensity only.)

When the physiologist has finally done his work (which of course he cannot do unless he keeps combined in his one head the wisdom also of the physicist and of the psychologist), there remains the final transformation—that from cortical or possibly subcortical (Herrick) process to conscious experience, the neuro-psychic correlation. Here the correlation must be exact. It will not do to say with v. Kries (the most philosophical of the physiologists) "wir sind einem *gewissen* Parallelismus verpflichtet." It is not necessary (as G. E. Müller has pointed out) that every cortical process that can have a conscious correlate should *always* have its conscious correlate. Consciousness is a lambent flame, and it does not always follow upon the appropriate stimulation. But in the other direction the axiom must always hold. No two final processes of a *different* kind can both give rise to the same gray-green-blue. The distinction cannot be topographical, for, just as in the retina, the *topographical* spread-out-ness has to be utilized for the place-coefficients of vision. But the peripheral rod-whiteness and the foveal cone-whiteness (since they are of the same quality) cannot be due to *different* final processes. v. Kries says that I do not explain why rod-whiteness and cone-whiteness look alike. But in this he overlooks one of the main characteristics of my theory—not only do I explain this, but it is one of the principal things that my theory exists for the sake of explaining, as Hering has explicitly recognized.

The theories of Helmholtz and of Hering have been so fully discussed by the psychologists that their several inadequacies are now well known. Mr. Troland says (*Am. Jour. Physiol.*, Am. Optical Company, pp. 8–9, 1913): "The scientific value of the Helmholtz theory is at the present time almost negligible; it explains only the most rudimentary

of the phenomena of visual sensation; and that of Hering, in its pseudochemical, physiological, and psychological aspects is guilty of all manner of offence against fact and reason." Bayliss makes short work of Hering: the disappearance of the two vanishing color pairs cannot be accounted for by processes of assimilation and dissimilation, for there is nothing to prevent those processes from going on in one test-tube (or cone) at the same time. This is an argument that I have myself used against the Hering conception for a long time ('Professor Müller's Theory of the Light-Sense,' PSYCHOL. REV., 6, 1899). Parsons says that I am very severe on the theories of Helmholtz and Hering: I can only say in reply, in the words of a clever medical article that I have just been reading, "If I could be more severe, I would be."

But as I have already said, it is some of the most recent theories of color that present the most instructive instances of sins against fundamental methodological principles. Of particular interest in this way are the color theories of Mr. Troland. These theories are not, indeed, important in themselves, but they are, as I have said, immensely interesting from the pedagogical point of view, as showing what vagaries the mind of a clever scientist is capable of when it goes a-color-theorizing.

It is well known that Mr. Troland has a wonderfully fertile mind in the construction of theories;—there is hardly a subject of scientific investigation in regard to which he has not a theory of his own, and on some subjects he has more than one theory. His original theory of nerve-conduction (that of 1913) will be found reproduced at length in *Physiological Reviews* of last year. But his present theory of nerve conduction is a very different one—in it he agrees with the views of Professor Lillie, as those views were developed up to 1911. This later theory (what Mr. Troland calls 'our theory') is very fully set forth in the PSYCHOLOGICAL REVIEW of last year.

The earlier theory (of 1913) I find of very great interest from the pedagogical (and methodological) point of view.

In this theory of 1913 (what he calls a 'definite' chemical hypothesis) he follows the lines of Hering—there are assumed four distinct chemical processes to account for the four distinct chromatic sensations and a fifth to account for the sensation of white. (Black is, as in my theory—and in all theories except that of Hering—a sensation attached to a non-stimulated condition of the cortex.) But these processes are not antagonistic, nor reversible, as in the theories of Hering and G. E. Müller, respectively. This is, so far, an improvement over those theories; but it still remains to account for the vanishing of the vanishing color-pairs, red-green and yellow-blue. This mysterious phenomenon he explains in the following manner. The specific positive ions for yellow and blue, for instance, proceed safely through two synapses and through one bi-polar cell, but when they reach the large ganglion cell of the inner stratum of the retina [of the third neuron] a sudden exchange of partners takes place—a complicated molecule stands ready to yield up two specific negative ions to these two positive wanderers, and thus to prevent their proceeding farther. But their place is taken, obligingly, by the nucleus of this waiting molecule, which turns out to be a (double) *whiteness*-ion (and in the case of the red-greens a double yellowness-ion); in this way the white-sensation and the yellow-sensation take the place respectively of the missing yellow-blue and red-green.

I have tried to convince Mr. Troland (as he mentions in 'The Enigma of Color Vision,' pp. 28–30) that this conception is too much of the nature of a vicious *ad hoc* hypothesis—and even worse; for when once the four specific chromatic nerve-impulses are safely started on their way to the cortex, why should Nature, for no reason, interpose a process for effecting their mutual extinction? Our blindness to the red-greens and the yellow-blues is a loss, a defect (all blindness is a defect), and it can only have occurred as an unavoidable insufficiency in the method employed to make vision specific (as the heat-sense has not yet been

made specific—we cannot tell one wave-length from another in heat). It is true that Mr. Troland at first proposed the idea that this vanishing of the color-pairs might have sexual significance—that yellow and blue were colors of approach and retreat respectivcly, and that, in order to prevent the embarrassment, on the part of ardent youth and maid, of being tempted to retreat and approach at the same time, they were stricken with blindness (or their low-down ancestors were) to the yellow-blues and to the red-greens. But I understand that its author has at present given up this part of the first theory. My contention that his hypothesis is a purely *ad hoc* one Mr. Troland discusses, but does not assent to it and he still apparently defends this color theory.

In his second theory (*J. Opt. Soc. Am.*, 1917) Mr. Troland has ceased to feel the necessity for having yellow (or white) in his list of colors, and he adopts the three resonance curves of Helmholtz, hitherto overlooked by him. He actually says, in this paper, in plain black and white: "To explain the facts of color vision [by vision he means sensation] we have only to assume the existence in the retinal receptors for color (the cones) of three substances of this sort, having the maxima of their resonance curves in three representative regions of the spectrum, and to suppose that the relative intensities[!] of these three excitations, for any given wave-length, can be transmitted along the nerve to the brain." This would seem to be merely a momentary inadvertence on the part of Mr. Troland; he knows as well as do the psychologists that color sensation is tetrachromatic—that yellow (and white as well) needs to be accounted for in whatever purports to be a theory of color sensation. However, he repeats this statement in 1921. He says ('Enigma of Color Vision,' p. 19): It must be that "nerve-conduction must involve at least three independent variables, *and that three are sufficient*"—ignoring again the necessity of providing for yellow and for white. Fundamentally different as these two theories are, Mr. Troland usually refers to them as if he still believed in them both.

The third theory of Mr. Troland is indicated briefly in his paper of 1921 (*Am. J. Physiol. Optics*, p. 18). The habitat of this theory is now that "advanced" region, the cerebral cortex. The vanishing of the color pairs becomes again, for him, antagonistic—namely a process of inhibition and excitation, and the 'excess of nerve energy' that is thus set free produces 'some' effect which is the basis of yellowness and of whiteness. However I fear that even this hypothesis, vague as it is, is not perfectly satisfactory to Mr. Troland, for he says at the end of this same article that the facts of color are still 'in a fluid condition' in his mind.

But the principles of logic can be usefully studied not only in the composition of new theories but also in the nature of the criticisms made upon theories that already exist.

Others have occasionally not fully understood what it is that my theory accomplishes, but no one has so completely misunderstood it as has Mr. Troland. The three important facts in color (I cannot too often insist upon it) are (1) the development of the color sense (with yellow and blue the middle stage, then red and green added), (2) complementation (but with yellow and blue *white*-constitutive, while red and green are *yellow*-constitutive), (3) the fact (Helmholtz, Young) that three initial *receptor-processes* are sufficient to start up the whole color gamut—that yellow and white come in of themselves—that they are secondary products—that no *new* light-stimulus is required to produce them. But what ones of the chromatogenic light rays are these three? Strange to say, *yellow* (which plays such an important rôle in the *development* of the colors) is left out,—all that are needed are red, and green and blue! This is indeed a mysterious situation, and the task of reducing to order and harmony such a contradictory collection of facts would appear to be (I am quoting from Mr. Troland) 'a tremendous one'—'making more rigorous demands upon the intellect than the formulation of far more cosmic theories.' But the problem of reducing these strange facts to harmony

and order I have undertaken to solve,[1] and by the simplest of assumptions. I assume merely that there has been a *development* of a light-sensitive substance, of this nature,—that it has become, by a new segregation of its molecules, by the formation of new constituent radicals, more *specific* in its reactions to light; but at the same time, since the molecules concerned are the same as before, certain of the decomposition products, when present together, unite chemically and form one of the previous nerve-excitant substances. This may of course not be the real state of things; it is offered as hypothesis (not as 'reality'), but it has perfect *vraisemblance*, and it affords a perfect explanation at once of complementation and of development. Until something better has been done, I hold that it invites consideration.

But it is exactly this fact—the explanation of complementation—that Mr. Troland has failed to notice in my theory. He says ('Enigma,' 15.2); "They are supposed to generate an *entirely new type* of nerve excitation which ends in a *yellow* sensation." Not new at all—this is just the point—my theory explains why they produce the *old* nerve excitation—the very one that caused the sensation yellow in the more primitive forms of vision. That is to say, I explain (among other things) why the *primitive* yellow is the *same sensation* as the yellow which (in tetrachromatic times) is made out of red and green, and also (what v. Kries too has overlooked) why the *primitive* white (in the rods) is the *same sensation* as the later white, that of the cones,—in spite of the fact that the cones are such extremely different organs from the primitive rods. (Hering has expressed appreciation of this feature of my theory.) It seems to me that this is accomplishing a good deal in the way of explanation, and by the simplest of means. Unless defects in this hypothesis can be pointed out, I conclude that it ought to be regarded (as it is regarded by many) as an efficient hypothesis. Mr. Troland says that it is ingenious, and that the basic outlines are probably essential (that is,

[1] The other facts of vision, after these fundamental ones are disposed of, are of course more easily handled.

indispensable) to any elucidation of the visual phenomena. (He means the light-sensation phenomena.) But this is exactly what I have myself said regarding my theory from the first—that it is, at least, a 'scaffolding' within which any final theory, when more detailed knowledge regarding the effects of light on the retina has been acquired, must be contained. Prof. Burton Sanderson (presidential address, British Association, 1893) after discussing the then recent facts regarding color said that they could be best understood in terms of my theory, and added, quite correctly, that my schema is, of course, diagrammatic. At the same time I hold, with Mr. Troland, that it is indispensable.

Mr. Troland's attitude towards my theory[1] is, on the whole, rather complicated. These are the features of it which he appreciates:

1. He accepts my theory of the black sensation (but he has not noticed my theory of why we have a sensation of blackness nor my proof that the blackness sensation is of one intensity only—that the series of grays are correlated with variations simply in the intensity of the white-constituent).

2. He yields to my insistence upon the fact that red and green lights, when mixed, are productive of yellow, not of white. (This is of course in complete contradiction to his Theories I. and II.—those of 1913 and 1917.) This view of mine has also been accepted by Westphal, by v. Kries and others, and it has virtually been accepted by Titchener, when he, with great honesty, plainly says that the Hering green and red are not the exact green and red, but verdigris and crimson. There are two horns, you will note, to this dilemma. The real red and green lights, when mixed, do not make white but yellow. On the other hand, if you take a sufficiently bluish red and a sufficiently bluish green ('crimson' and 'verdigris') you will have complementary (or white-constitutive) colors, it is true, but you will be dealing

[1] Good diagrams of my schemata—better in fact than I have been using myself—will be found in Woodworth's 'Psychology.' See my article in a forthcoming issue of *Science*.

with three (not two) unitary sensations, red, green and blue (we have here the great Helmholtz fact), and you will need to assume for them, on the most elementary principle of methodology, not two but three photochemical processes. My theory, in fact, is the only one that both takes account of this fact and offers an explanation of it. The Helmholtz theory so called accepts indeed the fact, but (being, as Professor Cattell has pointed out, both pre-psychological and pre-evolutionary) it has not felt under compulsion to offer any explanation of it. This, as I have said, is an element of my theory which Mr. Troland explicitly appreciates; it is in contradiction to his Theory I. and it is unexplained in his Theory II., but he takes it over into his Theory III.

3. He recognizes—the central feature of my theory—that it takes in both the sets of facts upon which the theories of Helmholtz and of Hering are founded (facts which are, respectively, subversive each of the other theory). He says, after 'supplementing' my theory by the assumption of a primitive white process in the rods (which was of course a fundamental and essential part of it from the beginning), "I think it is clear that the Ladd-Franklin theory is distinctly superior to the other two. This theory establishes contact on the one hand with the [four] psychologically primary color qualities and on the other hand it takes cognizance of the three-color-mixture laws upon which the Young-Helmholtz theory is based." ('Enigma of Color Vision,' Am. Optical Company, pp. 13–14.)

Mr. Troland's explicit objections to my color theory are two in number:

(1) He thinks that any theory of color should deal exclusively with cortical processes—he says that retinal processes are probably 'negligible' as throwing light upon these phenomena, and that the more 'advanced' view is that the basis for the peculiarities of color is to be found at some higher level. (It sounds a little like Christian Science, and New Thought, to say that some views on colors are more 'advanced' than others. Topographically it is true that the cortex is more 'advanced' than the retina.)

(2) His other objection is that in order for the partial dissociations to take place that my theory requires, there must have existed in the original whiteness molecule such aggregates of atoms as are those now to be broken off.

I take up the first objection first. In this tangled course of events from physical light to psychical light-sensation—from a 'rectilinear' series (to use G. E. Müller's good term) of electro-magnetic vibrations to a poverty-stricken four chromatic sensations with their four (not six even, as there would naturally be) series of color-blends, it is absurd to say that any *one* event is of 'prime importance.' It is a total misconception of fundamental principles of methodology not to recognize that if we have a causal chain of events such that *A* conditions *B*, *B* conditions *C*, *C* conditions *D*, etc., each one of the intermediate events is, not only of prime, but of absolute importance. Who does not know that a chain is no stronger than its weakest link?—though there has not often been occasion, perhaps, to set it down as an axiom of scientific logic.

What Mr. Troland really means to say is that in his opinion the specificity of the visual chain of processes is more likely to reside in the cortical processes than in the retinal ones. But this is merely an opinion, and the fact that we know more about synapses than about photochemistry (if it is a fact) does nothing (though Mr. Troland seems to think it does) to support it. I agree with the opinion of Dr. Howell (just expressed to me in a personal communication) that the specific features of the visual chain of events are more likely to reside in the receptors, and in the receptor processes, than in any of the nerve-structures. When once the nerve impulse has started on its course it would seem to be practically the same thing in every sense region (save for the fact that the psychologist may demand of the physiologist provision for at least five different mechanisms of conduction in a single nerve fiber). In the debouching of nerve impulse upon that cortical structure, whatever it may be, which is correlated with the psychic experience of redness or greenness, there must indeed be some-

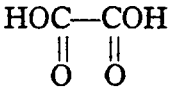
thing specific. There is no reason to think however that a 'red' excitation and a 'green' excitation can proceed uninjured to the cortex only there to be degraded into the basis for a more primitive yellow sensation.[1] It is far simpler to suppose that the 'transformer mechanism' (by which for innumerable wave-length combinations are substituted *five* colors only) has its site in the retina. But Mr. Troland says (p. 15) that my theory would be more satisfactory if it were transferred to the cortex. I have no objection to carrying on, at need, a second theory in the cortex, but I confess that I still prefer the retina as my theory's principal habitat.

When it comes to the final link in the chain—what I have called the neuro-psychic correlation—we have indeed something which, as Mr. Troland says, is the most 'interesting' stage of all. But it is a wilful ignoring of fundamental principles to suppose that light-sensation (or anything else) will ever do anything towards 'explaining' it. What is an explanation? It is a subsuming of facts, or of partial laws, under some more general law of which they constitute particular instances. But this connection of final cortical process with conscious experience is unique—*sui generis*—there is no companion-piece to it in the universe elsewhere, and there is no more general situation under which it can be subsumed; hence the effort to explain it will be futile. Its attendant conditions we may know more about, but the real *why* of the situation is plainly undiscoverable.

Mr. Troland's second objection is not only trivial, it is also purely factitious. It is that if there are three groups of atoms in my final molecule which are separately cleavable by light, they must have existed in it from the beginning. This is not only a complete misunderstanding of my theory—it has also no foundation in fact. It is difficult to know where Mr. Troland got this idea, that if certain substances are separated out from a given mother substance they *must* have existed already preformed in it. This fictitious rule

[1] In the Theory I of Mr. Troland they only reach a large ganglion cell of the retina, there to be captured and to set free a waiting whiteness excitation. It may seem to some that this idea lacks 'reality,' and *raison d'être*, though it is true that it has 'definiteness.'

does not hold even in the building of the complex nuclei of atoms out of the positive and negative electron groups of which they are composed. I have just come upon this statement by Professor Harkins who is doing such important work in the evolution of the atom (*Phil. Mag.*, *42*, 333, 1921): "That groups which are not contained as such in more complex groups may nevertheless be separated out from them is one of the common experiences of workers in chemistry; for example, anhydrous oxalic acid, with a formula



which does not contain the group HOH, nevertheless gives water when it is heated."

In the case required in my theory it is not a matter of a simple chemical reaction, but of the development of a chemical substance in the interest of greater specificity to light radiations; it is an idea, therefore, which is still further removed from being questionable. In fact the belief that substances must be preformed, if they are to undergo cleavage, is, my chemical advisors tell me, a conception that has not been current in chemistry since 1845.

But is my idea of the evolution of a color molecule so out of the usual? I admit that when I first proposed my theory this plan of paralleling (1) the known psychical development of sensation (non-specific whiteness into tetrachromatism) and (2) the known structural development of the visual elements (rods into cones) by an assumed development in the photochemical substance of the retina was somewhat venturesome, though even at that time it can hardly have seemed far-fetched. But the situation is completely changed now. Mr. Troland, who takes this view, but who at the same time explicitly accepts my idea that the added red and green sensations of normal foveal vision are not of the nature of a simple addition (as he took them to be in his Theory I.) but that they constitute a real evolution out

of a more primitive yellow sense, has probably not noticed that the *evolution of chemical substances* is at present very much to the fore. Aside from the wonderful new theory of Perrin—the radiation theory of chemical activity—I take it that there is nothing so exciting at present going on in the scientific world as the making out of the evolution of the atom by Soddy, Rutherford, Bohr and Harkins. I find myself, in fact, in the best of company in my conception of a chemical evolution, and I consider that Mr. Troland is a little old-fashioned in opposing it.

But I have, as it happens, a very beautiful companion-piece to my own conception—of such fundamental importance, in fact, as to give me all the analogy that I can ask for. Chlorophyll and hæmoglobin (perhaps the two most important substances in nature, Bayliss says), are both constructed out of practically the same constituents—both are compounds (with other substances present also, namely iron in chlorophyll and magnesium in hæmoglobin) of three dimethyl ethyl pyrrols. Willstätter (in a personal communication to Bayliss) uses these very striking words,—he says that he does not regard the similarity in constitution between chlorophyll and hæmoglobin as being of any great significance. (By this he means that it is nothing extraordinarily unusual.) I quote from Bayliss: "The mother substances were probably at hand, and compounds with the properties of the two pigments, respectively, being required (if one may use the expression) these pyrrol derivatives were made use of."

With this analogy, so very much to the point, I confidently rest my case, as far as a chemical evolution is concerned.

I cannot then too strongly urge upon the scientist the study of practical logic as it is exhibited in the good, but more especially in the bad, theories of color. The subject of light-sensation is a difficult one in the sense that there is no parallel in any other domain of the senses to throw light upon it, and also that the facts, which are numerous,

are, indeed, when not woven together into some consistent structure, complicated and confusing in the extreme. These complicated facts have given rise to many attempted solutions, most of them however of a negligible degree of *vraisemblance* and wholly lacking in conformity to the simplest methodological principles. It is probable, in fact, that no subject of human endeavor has ever been the source of more vagaries and misconceptions—not even the squaring of the circle or the trisection of the angle—than will be found in the existing theories of color sensation, sixty or seventy in number according to Professor Frank Allen.

I do not however agree with Mr. Troland when he says that the task of comprehending color is a 'tremendous' one, and that it makes, perhaps, 'more rigorous demands upon the intellect than does the formulation of far more cosmic theories.' If, indeed, one envisages the phenomena of color from the point of view of the way in which the color sense has actually been developed in the animal kingdom, and if one also bears in mind that instead of a spacious cochlea Nature had only a very minute space—an infinitesimally small cone, in the fovea—in which to set up a whole new mechanism for specificity, it will be found, I believe, that the phenomena of color are by no means so much more mysterious than other events which psycho-physiology has to deal with. Certainly when it comes to the nature of nerve conduction, and of the final neuro-psychic correlations, there is no reason to think that the domain of color differs fundamentally from any other of the sense domains. And in the light of the known course of development of the color sense, the facts of peripheral vision and of the illuminating progression of stages of color-blindness cease to offer difficulties.

A more explicit discussion of the logical principles that should govern the making of theories of color will be found in my paper 'The Theory of Color Theories.' [1] The science of methodology has of course a totally new problem pre-

[1] Comptes rendus du VI[e] Congrès international de Psychologie, Genève, 1909, 698–705.

sented to it when it reaches this subject. In other sense-regions variations in stimulation produce variations in consciousness, but that is far from being the case in color. In general, a point on the color area can be got by countless different combinations of wave-lengths. Such a sensational point is however uniquely expressed in terms of the three variable quantities which have been substituted, in the retina, for the octave of frequencies of the visible energy-radiations. This constitutes what I have called a "transformer mechanism"; if one fastens his mind upon the color-triangle (the map of possible color-sensations), and forgets the visible spectrum, comprehension of the phenomena of color will be greatly facilitated.[1] There is, it is true, an immense amount of work to be done (and being done) in making out, quantitatively, the details of each stage of the color-sensation processes, but nothing is gained by over-emphasis upon the fundamental difficulties of the subject.

[1] PSYCHOL. REV., 23, 242–245, 1916.