During the nineteenth century cosmogony has been in the hands of an able group of mathematicians, physicists, geologists and astronomers, among whom should be mentioned Moulton, MacMillan, Chamberlin, Kapteyn, Sears, Russell, Shapley, Jeans, Eddington, deSitter and Einstein. The old ring theory of Laplace has been rigorously attacked and found wanting in many of its details. We now have a new idea under the caption of the planetesimal hypothesis in which the modern concepts of dynamics and the structure of matter play an equally important rôle with mathematics. Geology is no longer ignored. The invention and application of the interferometer has verified and greatly extended the postulated sizes and distances of the stars. The new one hundred inch Hooker telescope is resolving and analyzing the star clusters and nebulae in a manner wholly undreamed of a half century ago.

The two solutions of Einstein's fundamental equations, resulting in a finite, static universe as one extreme and an infinite, expanding universe as the other, give promise of a more general solution approaching objective reality. We must not be misled by the first solutions of so difficult a problem. As often happens in pure mathematics, the special cases are the first to appear, then the more generic gradually evolve.

The size and shape of the universe is probably no more impossible of solution to-day than the size and shape of the solar system was in Ptolemy's day. To our finite minds a universe that requires a beam of light five hundred thousand million light years to circumnavigate it is infinite, but as Sir James Jeans says, "We are not terrified by the sizes of the structures which our own thoughts create, nor by those that others imagine and describe to us. The immensity of the universe becomes a matter of satisfaction rather than awe; we are citizens of no mean city. Again, we need not puzzle over the finiteness of space; we feel no curiosity as to what lies beyond the four walls which bound our vision in a dream."

Schiaparelli once called astronomy the science of infinity and eternity and the description is just. "These words," says MacPherson, "are often used by philosophers and theologians. Astronomy gives some definite sense of what they mean. The concepts of infinity and eternity are soul-staggering, but they are less difficult than those of limitation of space and time. To the higher thought, the chief contribution of modern astronomy is doubtless this sense of the infinity of space and the eternity of time."

THE PHYSIOLOGY OF CONSCIOUSNESS[1]

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MY thesis this evening is that scientific psychology needs more than to become the physiological psychology that Wundt originally called it, and that we are not entirely without the means of proceeding in this direction. Psychology, it seems to me, needs to save for its own uses both consciousness and the nervous system, and it must have both if it is to survive.

Once upon a time psychology had some hope of getting along without a nervous system. There was a time when introspectionists, like Külpe and Titchener, would have hailed with avidity any step that brought psychology nearer to being a descriptive science of the facts of experience, a science that could get along with introspection as its only method and could leave the nervous system and the stimulus ruthlessly in the outer darkness of physiology. There is no need to explain to this audience that the introspective method unsupported failed to yield a psychology, or perhaps even a single factual generalization.[2] The most satisfactory introspective experiments were those that resulted in the correlation of sensory or perceptual data with stimulus. The best theories were formulated in terms of the nervous system or the sense-organs. Unaided introspection proved inadequate in crucial cases, as in the problem of thought where we were left with only a "physiological" determining tendency as a principle of explanation.

The reaction of behaviorism against this state of affairs by the complete rejection of the introspective method was very natural, even though it represented a throwing out of the baby with the bath. Theoretically you can answer for animals, by tests of discrimination or by observation of conditioned reflexes, any of the questions about sensory or perceptual

[1] Address of the retiring vice-president and chairman of Section I—Psychology, American Association for the Advancement of Science, New Orleans, December 29, 1931.

[2] There never were any laws of introspective psychology other than those that state the correlation of conscious processes with the stimulus or with events in the nervous system, with the possible exception only of the law of association. Now-a-days it is superfluous to claim that association is solely a law of conscious events, when we are so constantly being reminded of its physiological counterpart, the conditioned reflex.

capacities that have been answered for human beings by the use of the introspective method. And what can be done with animals in general can be done with human animals. Nevertheless, the behavioristic method is not always advantageous. Sometimes it yields results that are less univocal than those gotten by an introspective method.[3] Sometimes it is terribly laborious and no added precision is gained for the added pains. If you will imagine a behavioristic method which determines, without the use of words, the occurrence of difference tones, or the wave-length of one of the three pure spectral colors, you will see what I mean when I say that these objective methods can be clumsy.[4]

It is worth noting that behaviorism owes its *ism* to consciousness. And what would it be without its *ism*? Well, it would be physiology. Behaviorism has preserved itself as psychology and as something that is not physiology—I speak of the historical fact, whatever may have been the wishes of the behaviorists—by persistently attempting to solve the problems that originated as introspective problems of the psychology of consciousness. The attitude of the behaviorist for introspection has always been ambivalent. He has hated introspection for the love he bore it.

On the other hand, if we bring consciousness back into "physiological psychology," without ridding ourselves of the old-fashioned Cartesian dualism, we get no farther along than we have always been. Now there is nothing new in my objecting to dualism. Perhaps the majority of you had thought of dualism as already rooted out of psychology. Nevertheless, you see that the behaviorist, who would have us ignore consciousness in psychology, is thereby a dualist because he has to believe in consciousness as something different from the "objective" world in order to dismiss it as irrelevant. In this way behaviorism has often emphasized the fundamental dichotomy of mind and body by its insistence that the mind is not the body, and that you may take the one and leave the other. *Gestalt* psychology certainly has no use for dualism, and yet it is impossible to read Köhler[5] or Koffka[6] on the correspondence of "direct experience" to "underlying physiological processes" without feeling that the old dichotomy is still fundamental to their thought. The step I am asking you to take, in the interests of getting to a physiology of consciousness, is ever so much more radical than these imperfect attempts to avoid the Cartesian curse: I am asking you utterly to abandon dualism, sincerely, so that if there be a consciousness that could be ignored you will let it into the total system that is your scientific monism.

It is the introspectionists who have been primarily at fault in this matter. Wundt talked about "immediate experience," and Köhler talks about "direct experience." In such phrases there is an implication that there is some way of taking hold of experience, immediately, just as it is *per se*, and of keeping it for scientific purposes, and that in doing so one has introspection. Physical science is supposed to deal with entities that are mediate to experience, to be indirect in the sense that its subject-matter consists of inferential "constructs." The formula that Külpe and Titchener took from Avenarius, that psychology deals with all experience regarded as dependent upon the experiencing individual, is really not so much different, because experience really is dependent upon an experiencing individual for its existence, and when so regarded is thus being taken more immediately, more in its own right rather than as a ground for inference. I am quite serious when I say that this view of introspection seems to me to be nonsensical. The view implies that there is nothing important to introspection, that to have an experience is the same as to be aware of having it, that observation of conscious processes is nothing other than being conscious.[7]

However, the difficulty of regarding "direct experience" as the subject-matter of any science becomes more apparent when we note that we are landed by it in a circle of dependencies. At one time those who hold to this view will tell you that there is experience from which all science is derived, that psychology deals immediately with experience, and that the materials of physical science are mediately derived from experience. The view seems to make psychology prior to all the other sciences. Thus Köhler tries to prove that behaviorism is really introspectional because its data were originally experiential.[8] Nevertheless,

[3] On the behavioristic method as more equivocal than the introspective method in the determination of the two-point limen, see E. G. Boring, "The Stimulus-Error," *Amer. J. Psychol.*, vol. 32, pp. 449–471, 1921.

[4] *Cf.* the awkward (imaginary) experiment that J. B. Watson describes for the determination of difference tones, "Psychology from the Standpoint of a Behaviorist," Philadelphia, 1919, p. 78 (same in 2nd ed.). I have forgotten who it was in the Harvard Psychological Colloquium who suggested that an animal could be tested for *Hauptfarben* in the following manner. If one is after the wave-length for pure yellow, one would train the animal to respond positively to the yellower of two oranges, and would see how far along the spectrum this relational response would be given. Obviously, it should break down at the pure yellow, since a yellow-green is not yellower than a yellow.

[5] *Cf.* W. Köhler, "Gestalt Psychology," N. Y., 1929.

[6] *Cf.* K. Koffka, "Some Problems of Space Perception," in "Psychologies of 1930," pp. 161–187, Worcester, Mass., 1930.

[7] Philosophers have made this same objection, but introspectionists have found little force in it ever since the argument for immediacy was put so trenchantly by E. Mach, "Analyse der Empfindungen," 1886, *et seq.*, and Eng. trans.

[8] Köhler, *op. cit.*, pp. 3–69.

these introspectionists may at another time hold to the opposite point of view that experience is dependent upon the activity of a brain or a nervous system. "No psychosis without neurosis" used to be the phrase. What then have we?

We have first the assertion that the brain, a physical entity, is a "construct," like an atom or an electron, which is not as such given in experience but which may be regarded as real, and as generated from experience in the way that all scientific realities issue out of experience. In this sense the brain is dependent upon experience. However, in another sense experience must be considered as dependent upon the brain. To have each dependent upon the other is not a relationship that is going to help us much. Either dependency alone is valuable, but the two negate each other.[9]

On the other hand, we avoid this circle at once if we admit that psychology is not peculiar among the sciences, that introspection is as much a method as any of the other methods of observation, that it is a method whereby on the basis of experience we establish the existence or occurrence of mental "realities," like sensations or seen movements or any of the other phenomenal objects which introspection yields. The old-fashioned introspectionist will not like my calling a sensation a "mental object," but I mean that it is as much an object as ever a molecule is a physical "object." Certainly the sensation as such is not given in experience itself.

If any of you doubt this statement, you have only to think how science is always proceeding by indirection. It uses experience, yes; but always as symbolic of something else. The behaviorist misses this point when he tries to make behavioral observation more immediate than introspective. Watson said that introspection is verbal behavior, as if there were some virtue in preferring the immediate datum that the experimenter observes, the spoken words, to the conscious processes signified. The behaviorist who uses "objective" methods of recording is just as indirect as the introspectionist: the immediate datum may be a kymograph record, yet it is for him merely a symbol of behavior.

Now, at last, I come to the main issue of this paper, my thesis that *introspection is a method for the observation of certain events in the brain.* Traditional introspectionism would protest such a statement. If I see a red circle, it would say that I am not seeing the brain; no part of the brain is red, presumably no event in it is circular. Nevertheless, I may be observing the brain, just as I can observe animal behavior by looking at records from a kymograph, or as I can observe an electric current by looking at the black and white pattern which is a pointer on the scale of a galvanometer. In scientific observation we always come face to face with symbols, and usually we ignore the symbols and talk about the realities that they signify.

Such a symbolic function of introspection may, of course, be sound epistemologically and yet utterly wrong. A relationship of this kind either does, or does not, grow in the structure of a science. We can not force its acceptance by argument. All we can do is to find it implicit in thought and to bring it out into the open. Even when it is exhibited and accepted by every one, it remains as tentative and temporary as does all scientific truth. It is our task, therefore, to consider the extent to which this view has already found its way into psychological research and whether it has seemed to be thus far successful.

We can best understand what has been going on within psychology if we return to the dualistic tradition, and see that in it there were, roughly speaking, three principal loci for psychological events: (1) the sense-organs, (2) the central nervous system and (3) consciousness. The three are causally related: stimulation of the sense-organ gives rise to a central neural process, which in turn may be said to "cause" a conscious process. In the less exact parlance of the laboratory, we are always thinking about stimulus, brain and consciousness.

The events in the brain, the middle term of this dependent series, have been largely inaccessible to observation. There has been of course some extremely important "direct" experimentation, from Fritsch and Hitzig to Lashley. There has been clinical observation. The rest of what we "know" about the brain is at a higher inferential level. The physiologist holds to the faith that the brain, being made up of neurons, is capable only of that excitation which is the sum of the excitations of many neurons, and that these central neurons obey the same laws and are excited under the same limitations as apply to the peripheral neurons which have been experimentally studied. To this article of faith the psychologist sometimes opposes another belief, that the organization of cerebral excitation corresponds to the organization of phenomenal experience. These two hypotheses are not necessarily consistent, and often we have to choose between them.

The two end-terms of the dependent series, the stimulus and consciousness, have been much more accessible to observation. The result is that the largest body of precise information within experimental psychology consists of correlations between these two terms. These correlations are the facts which make up the

[9] This difficulty is considered at length in Boring, "The Psychologist's Circle," *Psychol. Rev.*, vol. 38, pp. 177–182, 1931.

chapters on sensation and perception in any psychological handbook.

If the scientific mind could be satisfied with correlations, physiological psychology might have ignored the brain as inaccessible to its methods, and have remained content with correlations between stimulus and sensory process in the old days and between stimulus and response later on. The experimental method itself yields, in the first instance, mere correlations; nevertheless, the scientist demands something more. He wants insight into the relationships, a complete and immediate understanding which seems to leave no further questions to be asked. Many persons, in learning to extract square roots by the use of a calculating machine, discover for the first time the rule that the sum of a given number of consecutive odd numbers equals the square of the number of numbers summed; the sum of the first *three* odd numbers, 1, 3, 5, is 9, the square of *3*. Such a relation is a mere correlation and it seems, when first discovered, a great mystery. Even the proving of the general rule by algebra may seem to leave the mystery intact. If, however, one draws big geometrical squares made up of unit squares, one sees at once why to any square one must add a "next odd number" of squares in order to get the next larger square. The mystery has gone and we have what I am calling insight.

It is this need for insight that has forced the brain upon psychologists. The gross psychophysical correlation must be made more intimate. We want, in Fechner's phrase, an "inner psychophysics." Nevertheless, even if we had this knowledge of the brain and the resultant correlations, we should still be wanting insight in both parts of the picture. We should, on the one hand, want more intimate knowledge of the relation of stimulus to brain, and this sort of knowledge we are now actually beginning to get in the all-or-none law of neural excitation, in the frequency theory of intensity that Adrian has so ably promoted, and in the experiment of Wever and Bray on the nature of the impulses in the auditory nerve. Very slowly this physiological continuity is getting worked out. On the other hand, the correlation between consciousness and the events in the brain shows no signs of yielding to insight because there is no conceivable way in which insight can transcend the dualistic gap between mind and body. If there were any ground for dualism, if immediate experience as such seemed capable of scientific study, we might shrug our shoulders and decide to make the best of an unsatisfactory situation. Since, however, dualism seems both to fail to give us a satisfactory scientific dichotomy and also to exclude insight from psychophysiology, we ought, it seems to me, to make all haste to abandon it.

Let us now get down to business and see what there is to be said, for the purposes of psychology, about events in the brain. We had best accept, I think, four of Titchener's dimensions of consciousness[10] as setting the main topics for investigation. Consciousness is organized in respect of four dimensions: quality, intensity, extensity, and the temporal dimension which Titchener called "protensity." We may begin with intensity.

Twenty years ago the physiology of intensity offered little difficulty. A strong stimulus gives rise to an intense sensation; presumably the middle of this causal sequence must consist of strong excitation. However, this simple view became untenable with the acceptance of the all-or-none theory of excitation of the neuron. It seemed at first as if a multiple-fiber theory of intensity were the only remaining possibility, that degrees of sensory intensity must depend upon the number of fibers stimulated. Then came the frequency theory, the generalization, now well established, that a stronger stimulus may excite a greater frequency of impulses in a single fiber.[11] It is not necessarily true that the frequency theory of intensity must displace the multiple-fiber theory. Some of Hecht's conclusions point to the possibility that the stronger stimulus gives rise both to the excitation of a greater number of fibers and to a greater frequency of excitation in the fibers stimulated.[12] The volley theory of Wever and Bray is also such a view.[13]

When such theories are being discussed, it is natural to ask whether there must not be a summation of separate impulses in the brain. I believe that such a question generally indicates the existence of an implicit belief that introspection gives direct information about the brain, that, since sensory intensity is not anything like a frequency or a spatial dispersion, the brain ought somehow or other to collect the separate impulses in order to get some single unitary state that corresponds to what phenomenal intensity itself seems to be. The multiple fiber theory calls for the summation of the impulses in separate fibers; the frequency theory calls for the summation of successive impulses in the same fiber; and any combination of the two theories calls even more for summation, since, when different causes give the same effect, we want some insight into how the different causes are effectively the same.

[10] E. B. Titchener never explicated his doctrine of conscious dimensions beyond his twelve-line note in *Amer. J. Psychol.*, vol. 35, p. 156, 1924.

[11] *Cf.* E. D. Adrian, "The Basis of Sensation," New York, 1928.

[12] See the summary of S. Hecht's views, H. Hoagland, "The Weber-Fechner Law and the All-or-none Theory," *J. General Psychol.*, vol. 3, pp. 354–359, 1930.

[13] E. G. Wever and C. W. Bray, "Present Possibilities for Auditory Theory," *Psychol. Rev.*, vol. 37, pp. 365–380, esp. 376–380, 1930.

Now I think we can indicate positively the sense in which summation must occur on any theory of intensity, but before I go into that matter I wish to point out how already prevalent is this view of the direct correspondence between consciousness and the brain.

Most psychologists have accepted this conception as a matter of course. It is the common assumption in different physiological theories of Weber's law, the theories of Wundt and G. E. Müller. Köhler is the most courageous modern to state the general view. He thinks of intensity as the electrical charge of a concentration of ions in the nervous tissues. He dislikes a constancy hypothesis between stimulation and central excitation, and so he suggests how the logarithmic relation of Weber's law might occur on the purely physiological level. However, Köhler likes a "constancy hypothesis" of the relation of consciousness to the brain (of course, he does not call his hypothesis by that term), so that there he supposes a simple relation between gradients of intensity and of electrical potential.[14] This notion of Köhler's, that there is a direct and simple correspondence between consciousness and events in the brain, only just misses what I take to be the necessary denial of dualism.

The physiologists are apt to avoid the problems of consciousness, but when they consider them they tend to make the same assumptions as the psychologists. Let me illustrate by reference to a recent article of Hoagland's on the Weber-Fechner law. This law requires that the plot of the measure of sensation against the logarithm of the stimulus should be linear. Hoagland, citing Hecht, points out that the Weber-Fechner law is known not to hold at the extremes, and that this semi-log plot may be, not linear, but sigmoid in shape, thus corresponding to other functions familiar to physiologists.[15] He cites certain cases where what we may call "excitation" does show this kind of dependence upon the logarithm of the stimulus. He cites Hecht's analysis of König's data for the visual discrimination of brightnesses as proving the same point. What can one conclude? That Hecht and Hoagland, at least, have rejected dualism and are ready to accept introspection as a measure of physiological excitation, for König's data were introspective, and they are ready to bring them under a physiological generalization.

Certainly then it is good form to assume that sensory intensity is a symbol of neural excitatory intensity. Can we justify the view further?

Here it is, I think, that we need to appeal to the physiology of introspection. Let us suppose that the occurrence of a given sensory intensity leads to a judgment of its degree. It does not matter whether that judgment is a word, spoken aloud or written on paper, in English or German, or whether it is the pressing of a key connected with an apparatus, or whether it is some imaginal form of note-taking which only later leads to expression. The judgment differentiates *the* intensity from other intensities, and as such it is a response to *the* intensity. In the absence of such response there can be no knowledge that the intensity occurred and hence no introspection.

However, the discriminative response is a response *to* the intensity, which one must now think of in physiological terms. Do the excitations of many fibers have to be summated in order that a response may occur to them? Do the successive excitations of a single fiber have to be summated to lead to a response that depends upon the frequency? Not necessarily in any objective sense, if summation implies that all the impulses are collected together into one place at one instant. On the other hand, there is summation in the sense that the response is to the totality of the impulses, that all the impulses are collectively effective in producing a single response which is characteristic of them all as a totality. For instance, if the neural impulse is electrical in nature, there need never be a summation of ion charges, but there must be a functional summation into a single physiological effect.

The experiment of Wever and Bray illustrates this point beautifully.[16] They hooked an electrode on the central end of the cat's auditory nerve where it enters the medulla; they talked to the cat and greatly amplified the currents of action in the nerve, leading the amplified currents to a loud speaker. Human speech was heard over the cat's nerve with little distortion and tonal frequencies up to 4000 cycles were accurately transmitted. It is still extremely doubtful whether a single auditory fiber can transmit a frequency greater than 1000 cycles. However, Wever and Bray transcend this difficulty in their "volley theory" which combines the multiple-fiber and the frequency theories of intensity. I can not expound this theory here, but the point of it is that, if a tonal frequency and amplitude, corresponding respectively to a given pitch and intensity, are put into the ear, the resultant excitation in the auditory nerve is utterly different, consisting of a number of frequencies in a number of fibers, and yet a simple electrode is able, out of the total effect of the excitations within the nerve, to pick up the original frequency and amplitude. The electrical circuit "responds" to the

[14] *Cf.* Köhler, "Die physischen Gestalten in Ruhe und im stationären Zustand," Braunschweig, Germany, pp. 211–227, 1920.

[15] Hoagland, *op. cit.*, 351–370.

[16] Wever and Bray, "The Nature of Acoustic Response: the Relation between Sound Frequency and Frequency of Impulses in the Auditory Nerve," *J. Exper. Psychol.*, vol. 13, 373–387, 1930.

totality of events in the nerve, without the original frequency and amplitude ever being reinstated in the nervous tissues. In the same way the physiology of sensory intensity must be at least a simple totality of degree of excitation, even though it may not be summated at any point at any one instant of time.

If we turn from the problem of intensity to the problem of extensity we find that ever so much more has been written about this problem and that the solution is less certain.

The oldest theory of extensity is the projection theory. It is a theory that was helped a hundred years ago by the theory of the specific energy of nerves, for, if the mind perceives, not objects, but "the states of the nerves" (as Johannes Müller said), it can "perceive" spatial pattern only if it be projected upon the brain. Now-a-days, Köhler, while denying projection, nevertheless keeps our thought in the same channels by his principle of correspondence between the spatial order of phenomenal experience and the underlying physiological processes.[17] It is certainly good form to suppose that the perception of shape and size is dependent upon spatial differentiation in the fields of central excitation.

There is not the same amount of supporting evidence for this view as there was in the parallel case of intensity. Nevertheless, I think that the conclusion for intensity helps us to a belief in a broad conception of correspondence for all the dimensions of consciousness. It looks as if some sort of physical intensity (like electrical potential) were the physiological fact of sensory intensity. It looks as if spatial differences of stimulation were the occasion of spatial differences of central excitation, and temporal differences of stimulation, of temporal differences in central excitation. We are not in such a statement making an appeal to analogy; we are saying that the physical dimensions of peripheral stimulation are likely to be the dimensions of the organization of central events, that there are not enough possible dimensions for us to expect a change of kind, and that the adequacy of perception to certain dimensions of the external world is thus readily explained.

The projection theory of extensity would thus be a very acceptable theory were it not inadequate to the facts. Since we can not undertake to review the entire field of space perception, let us select for consideration two special problems: the problem of the third dimension in vision and the problem of visual size.

Koffka has recently suggested that the existence of the third dimension in visual perception implies a tridimensional neural pattern in the brain.[18] Such a view is consistent with Köhler's theory of correspondence, but I think that a stronger case can be made out for it than a mere appeal to an uncertain generalization. So much has been said about the dependence of the perception of depth and distance on convergence, accommodation and retinal disparity, that Koffka is at pains to take the cases of depth that occur in simple drawings, where neither accommodation nor any binocular *differentia* is possible. He shows that you may see the Necker cube in either perspective, but that you practically can not see it as a flat geometrical design in the plane of the paper. What is the difference then between the plane design and the two perspectives? As projections they would be identical. If the two-dimensional pattern means a two-dimensional field of excitation, it is almost inevitable to look for a tridimensional field when the third dimension comes in immediately to the perception.

The case becomes stronger when we consider how retinal disparity works in stereoscopic vision. Let us think of the case of the truncated cone, which stretches out convexly toward the observer in stereoscopic vision. Each eye sees only a small circle within a large circle, but the relation of the small circle to the large circle is disparate for the two eyes. When the eyes first view the two drawings, there may appear two completely separate images in perception. Then the eyes move in respect of each other until there is seen, let us say, but a single large circle. At this stage the small circles may remain double within the single large circle. Thus far the experience fits the projection theory. The two large circles appear as two circles until the eyes move so that they lie upon corresponding points; then there is but one circle, because the two circles are projected upon the same locus in the brain. Moreover, we see that the eyes tend to move so as to bring similar images upon corresponding points. It is as if the mechanism of vision operated in the interests of simplification.

However, the eyes can never move so as to make both the large and the small circles coincide at the same time. If the small circles coincide, the large circles must be double, and *vice versa,* if we keep to projective geometry. However, projective geometry is just what perception does not preserve. Presently both small and large circles coincide and we see the truncated cone as a solid. Is not the conclusion almost inescapable that the tendencies for the large circles to combine and the small circles to combine are realized by the establishment of the circles in different fields, which for bidimensional figures would have to be separated in a third dimension?[19]

[17] Köhler, "Gestalt Psychology," *op. cit.*, p. 64.

[18] Koffka, *loc. cit.*

[19] There is a very simple system of geometrical projection in which a disparity, which is like retinal disparity, actually gives the projection in the third dimension, but I forego its discussion since I can not make it seem like acceptable physiology.

Now let us turn to the problem of visual size. It seems probable that the perceived size of stimuli at the same distance from the observer is proportional to the size of the corresponding retinal images. As usual we can begin with a projection theory.

However, a projection theory breaks down when we consider size in relation to distance. As a stimulus-object is moved away from the observer its perceptual size decreases, but it decreases not nearly so fast as does the size of the retinal image. The alley experiments have worked out the law of the dependence of phenomenal size upon distance. The facts are as if perception compromised between the projection theory and some other theory, under which a given object would maintain its size, irrespective of distance.

There is another way in which size varies. The moon in the zenith is perceptually smaller than the moon on the horizon. For all the controversy that has gone on about this illusion, it seems fairly accurate to say that the size of the moon or of any other stimulus-object is diminished when the head and the eyes assume the strained position required for looking at the zenith and *when distance is indeterminate*. It is this second condition that has fooled the experimenters. The illusion fails, or is reduced to a few per cent., for an artificial moon a few feet from the observer. Schur showed that the illusion may be as much as 50 per cent. for cardboard moons 33 meters from the observer. Beyond 33 meters in the vertical it is not possible to carry most experiments, but the implication of Schur's results is that position of the head and eyes makes a difference to size when distance is indeterminate, as it becomes when it is great. With shorter and determinate distances, the laws of the alley experiments hold. With the moon, distance is completely indeterminate and the illusion is maximal.

It thus appears that perceptual size is a complex function of retinal size, of distance, and at times even of the position of the head and eyes. Moreover, there is some ground for belief that this variation in size applies to three-dimensional fields. It is very hard to adjust this sort of physiology to the conventional notions of neuron reflex arcs. These phenomena accord much better with Lashley's principles of equipotentiality and mass action in the cerebral cortex.[20] Hunter has criticized Lashley's views on the ground that it is possible to explain the animal behavior in question in terms of a more conventional physiology.[21] The difficulty with Hunter's position, so it seems to me, is that conventional physiology, even if it can explain Lashley's data, can do very little else for the theory of perception. What, for instance, can it do for these problems of the visual perception of solidity and of size?

I am not proposing that we disregard facts in favor of theories. I am proposing merely that we accept, tentatively, the most productive hypotheses, those with the greatest resolving power. We may need some day to abandon the hypothesis that consciousness always involves some kind of brain action. However, I think that this view should be kept just now, and with it the more explicit view that spatial and intensive phenomena, given in introspection and representing respectively spatial and intensive aspects of the stimulation, are symbols of spatially and intensively differentiated events within the brain. The evidence for such an hypothesis is scanty enough, goodness knows, but I think it is greater than the grounds for faith in the simple reflex-arc theory of the brain. If we must choose an hypothesis, let us choose one that gives us some insight into the extensive knowledge of perception which experimental psychology includes to-day.

There is no time for me to discuss the other two dimensions of consciousness. Of protensity, the temporal dimension, we know but little. We should be looking, with Köhler, for durations in the brain when introspection shows duration to be a characteristic of the perception.

Quality has had no acceptable physiological hypothesis for itself since the theory of the specific energy of nerves and the related theory of sensory centers broke down. All we can be sure of is this: whatever quality is within the brain, it must be differentially dependent upon whatever quality is within the stimulus. In tonal hearing this view means a frequency theory of quality, and Wever and Bray have shown how such a view is not necessarily incompatible with the theory of peripheral frequency for intensity. In the other senses we are dependent upon more knowledge of the receptor processes.[22]

Let me see if I can now, in closing, repeat all that I have said in the compass of a few words.

Dualism is dead. It ought to be buried. It can not work for us any more and we do not need it.

The great delusion of psychology has been the be-

[20] The standard reference is K. Lashley, "Brain Mechanisms and Intelligence," Chicago, 1929; but perhaps the clearest exposition of this point is his "Mass Action in Cerebral Function," SCIENCE, vol. 73, pp. 245–254, 1931.

[21] *Cf.* W. S. Hunter, "A Consideration of Lashley's Theory of Equipotentiality of Cerebral Action," *J. General Psychol.*, vol. 3, pp. 455–467, 1930.

[22] Perhaps this view accords with J. P. Nafe's objections to the theory of the specific energy of nerves and thus with his quantity theory of feeling; *cf.* his discussion in "The Foundations of Experimental Psychology," pp. 395–399, Worcester, Mass., 1929. I have never understood Nafe's theory; but if he means that quality must ultimately be understood as a function of the quantifiable aspects of nerve-conduction and the relationships that are quantitatively statable between excitations, then I suppose that all scientifically minded psychologists would immediately agree with him.

lief that we can have a science of direct experience. Scientific facts come out of experience, but they are then no longer in it. Science does not attempt to reconstitute experience; it builds up inferentially a world of constructs which are its realities.

A careful examination of the introspective process shows that introspection, like any other observation, is the taking note of symbols that mean occurrences in this constructural or real world.

We are, therefore, free to examine these symbols, the phenomenal data of introspection, to see what they can symbolize with the greatest profit for scientific psychology; and we conclude that neural events are the sort of mental constructs that introspective data most effectively "intend."

We can then set out to test this view, to see what it will yield us in the way of a physiological psychology. Such a view is necessarily subject to test and to correction, in the same way that a galvanometer is subject to test and correction as to how it means or "intends" the strength of an electric current.

When we go to the physiological theories of psychologists (and of some physiologists, too) we find many views consonant with the thought of the present paper, as indeed we could have known from the start, since the paper has been written to explicate and evaluate these views.

In general, the most plausible theory of the brain seems to be that the four conscious dimensions find reality there in four physical dimensions of intensity, extensity, duration, and an uncertain fourth which must have an immediate dependence upon the physical variable for quality in the stimulus.

Such a general view is most definitely explicable for intensity. Sensed intensity must represent degree of excitation in the brain. Such excitation does not, however, have to be localized at a single place at a single time, except that it must all be effective in producing a simple subsequent neural event, which is the first physiological term of the introspective process.

In respect of extensity, the notion that introspection tends approximately to mirror the brain is, at the present day, a plausible view and a useful one. A more conservative physiology not only leaves one without an hypothesis for most of the facts of space perception, but implies certain limitations which are contradicted by the facts.

Finally, in urging this view upon you for serious consideration, I would make bold to remind you that scientific hypotheses and scientific truth are temporary and provisional, and that hypotheses that are false to-day have been largely instrumental in leading us to what is true to-day. However, I doubt if a false hypothesis ever led far toward the truth unless it was at the time believed to be true. You have in that statement both my admonition and my apology.

SCIENTIFIC EVENTS

THE MASTER'S DEGREE FOR POSTGRADUATE STUDENTS IN THE MEDICAL SCHOOL OF COLUMBIA UNIVERSITY

ACTION to establish higher standards in the practice of surgery and other specialties of medicine has been taken by the Columbia University Council. Dean Willard C. Rappleye, of the Medical School, characterized the step as having "important significance of a public character." He said:

> The university has adopted a standard of training in each of the clinical specialties, successful completion of which will carry the degree of master of science. The university, however, will not grant recognition for postgraduate training for less than that which will qualify the man as competent in the specialty concerned.
>
> The time will come in this country, as it has in others, when the public and the profession will demand that only those who are properly trained to do major surgery, for example, will be permitted to do it. At the present time large numbers of doctors are doing surgery who are quite incompetent and untrained.

The new regulations for the degree of master of science in postgraduate medical education follow:

> The university grants recognition for acceptable postgraduate work in the clinical specialties by means of the degree of master of science.
>
> This degree is non-specific, that is, it does not carry a designation of the special field of study to which the student has devoted himself. Only a broad definition of requirements is stated in order to permit flexibility in the training for the various clinical specialties and adaptation of that training to the needs and preparation of each student. The specific requirements for each of the specialties are formulated by the departments concerned.
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> A student who wishes to secure the degree of master of science in postgraduate medical work must present evidence of graduation from a medical school approved by Columbia University, and completion of an internship of not less than one year after graduation in a hospital approved by Columbia University.
>
> Students who offer work pursued in other universities, laboratories or hospitals for part fulfillment of the requirements for the degree of master of science as hereafter set forth, should file a certified statement of such training with the director of university admissions for evaluation.
>
> A student admitted to the university for postgraduate medical studies who wishes to become a candidate for the