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THE PSYCHOLOGY OF VISUAL MOTION.

By HENRY J. WATT.

Two elaborate studies of visual motion have recently been published by A. Wohlgemuth[1] and by Max Wertheimer[2]. Both of these important papers add much to our knowledge of the facts, and excel in clearness and precision of work. They are also alike in rejecting all the psychological theories that have been advanced in their several fields of research and in formulating a physiological theory in explanation of the facts. Neither writer, however, makes any contribution to psychological theory. The possibility of such a thing is hardly even suggested; it is presumably annulled by the mere offer of a physiological theory. But the matter is not debated.

This situation seems to me so anomalous as to be worthy of special notice, the more so as the facts of the case hardly warrant the attitude adopted by these writers. In this paper I propose to deal briefly with the theory and outlook of these two works, which may be considered typical of a certain trend of opinion prevalent at the present time. In view of their general importance, however, and for the sake of brevity, I shall assume for the most part that the reader is already familiar with them and need only be reminded of their contents as each point arises.

[1] "On the After-effect of Seen Movement," this *Journal*, Monograph Supplement, No. 1.

[2] "Experimentelle Studien über das Sehen von Bewegungen" (Habilitationsschrift, Leipzig, 1912), *Ztschr. f. Psychol.* LXI. 161 ff.

I. CRITICISM OF WOHLGEMUTH'S PHYSIOLOGICAL THEORY.

The after-effect of seen movement, which is the object of Wohlgemuth's investigation, is familiar in various natural situations. If fixedly we gaze at a streaming waterfall or look down upon a rushing river for half a minute or so and then turn to look at the ground, the latter will seem to be streaming in a peculiar manner in the direction opposite to that in which the water flowed (relatively to our field of vision). For experimental purposes a simple form of this process is devised. A sheet of paper bearing alternately black and white lines of some little breadth is fixed upon a drum, which is rotated so that the lines move across the field of vision more or less slowly in a direction perpendicular to their length. Under suitable circumstances the lines will appear to move backwards when the motion of the drum is stopped. As the eye has been fixed and steady all the time, this peculiar after-motion cannot be due to any motion of the eye after the stopping of the drum, but must be taken as the after-effect of the preceding motion. Some theorists have therefore supposed it appeared because we were deluded by the previous objective movement into being accustomed to motion and therefore into expecting motion for a longer time than it was really there and thus into seeing what we expected. But this can easily be disproved by the application of incognitive methods, which prevent us from knowing from time to time what really happens. If the same after-effect follows whether the observer knows what is really happening or not, it cannot be the result of an illusion of judgment.

If the mind does not work at all to produce this after-effect, then apparently the only task for theory is to extend the accepted notions regarding the general physiology of neural processes so as to cover the facts; or to imagine a neural mechanism which will shew why motion is sometimes perceived where nothing really moves and why it then runs in a certain direction, opposite to that of the preceding movement. For his theory Wohlgemuth assumes that retinal elements $a1$ and $a2$, $b1$ and $b2$, are each connected with a "subcortical centre of movement," consisting of summation cells $A1$ and $A2$, $B1$ and $B2$, and also in pairs with a *Schaltzelle* $S1$ for the a's and $S2$ for the b's. $A1$, $A2$, and $S1$ are also connected with one another, as are $B1$, $B2$, and $S2$. Impulses are sent by $A1$ and $A2$ to the cortex, but this system of centres of movement is independent of other centres, *e.g.* those for brightness, colour, local sign, etc.

(*a*) Wohlgemuth assumes (pp. 99 ff.) that in the hypothetical centre of movement, owing to the part played by the *Schaltzelle* *S* 1, a state of facilitation lasts in *A* 1, so long as the objective movement stimulates the eye, but that as soon as this movement is stopped, the state of facilitation in *A* 1 is replaced by a state of fatigue in *A* 1. By this means, during the objective movement, *A* 1 is more excited than *A* 2, while during the after-stage *A* 2 is more excited than *A* 1. The psychical counterparts of these relations of intensity are, for the former, movement having the direction *A* 1—*A* 2, for the latter, movement in the opposite direction.

Now *this assumption posits the unfailing occurrence of so special a case that it seems to me to vitiate the whole theory.* We should rather expect many possible relations between facilitation and fatigue: facilitation frequently still increasing with psychical counterpart of similarly directed movement and after-effect, occasional balance with no visible movement or after-effect, frequent fatigue after longer stimulation with a reversal of both seen movement and after-effect, and thereafter periodic return to a state of balance. But, as we read on page 85, "no after-image of the sectors moving in the same direction as the objective movement could at any time be detected."

(*b*) *The theory offered virtually begs the question.* For, in order to suppose that the physiological basis of the experience of pure motion exemplified by the after-effect is a difference of excitation amongst the cells *A* 1, *A* 2, etc., and that the physiological basis of the direction of the felt motion is the spatial distribution of this difference of excitation amongst the cells *A* 1, *A* 2, etc., it must assume that the cells *A* 1, *A* 2, etc., already function as the physiological basis of different localisations, and that real directions within the complex of cells *A* 1, *A* 2, etc. (with or without actual physiological connexions between these cells) form the basis of felt directions; or it must assume that the cells *A* 1, *A* 2, etc., individually and as a complex, are connected and correlated with those other centres that are the physiological basis of localisations and directions. In either case the theory takes the physiological basis of localisation and direction for granted and only offers a theory of motion, treating it as a sort of intensive state, which refers and is attached to these localisations and directions, and endows the "sukzessive Aufspringen eines gleichartigen Eindruckes an verschiedenen Orten"—which we might perceive merely as such, were we beings devoid of the peculiar experience of motion—with this

unitary quality of continuity, namely "ein Hindurchgehen durch die zwischenliegenden Räume[1]."

But can motion really be treated in this way? Is it not rather the case that motion has a direction of its own, which may coincide with, or be opposed to, some other direction of which we are conscious apart from any motion? Is not also the velocity of a motion a characteristic of its own? Are not the motion, the direction, and the velocity, of motion—whether it correspond to a real motion or be pure motion in the sense of the after-effect—the essential aspects of this experience, its vividness being necessary in some degree, but as such relatively unimp rtant? The vividness of the experience may be to some extent interchangeable with its velocity, in so far as an increase in velocity is accompanied by an increase in vividness; but surely it would be contrary to experience to allow this vividness to usurp the place of the velocity of the motion itself, not to speak of its direction. And if motion presents a continuity that is not given in, or derivable from, the data of our space and time *Anschauungen*, should we not expect to find an explanation of this continuity included in the physiological theory of motion? But it is evident that this continuity is taken for granted in the theory as stated.

It seems then that the theory in question offers an explanation really only of the vividness of the experience; and if against this must be written the arbitrary assumption which I have stated under (*a*), the balance leaves nothing to the credit of the theory. We must discover first of all what is the neural basis of pure motion, its direction, and its velocity; it will hardly be very difficult thereafter to find a basis for its vividness.

On page 19 Wohlgemuth says that Borschke and Hescheles admit that, as seen, the movement of two sets of straight rods at right angles to one another "can only be regarded as squares, moving in an oblique direction." This movement can, of course, be described as one pleases; but if it is felt as the movement of squares in an oblique direction, that must be due to psychical, or shall we say, central, reasons; for it is essentially the *Gestalt* of the square which determines the apparent movement. If a point on one of the rods were marked out by colour or shape, we should at once in so far be free from this apparent oblique motion of the squares[2]. In connexion with this the

[1] Cf. Ebbinghaus, as quoted by Wohlgemuth, *op. cit.*, p. 108.

[2] Cf. Pleikart Stumpf, "Ueber die Abhängigkeit der visuellen Bewegungsempfindung und ihres negativen Nachbildes von den Reizvorgängen auf der Netzhaut." *Ztschr. f.*

forced explanation given of the results of experiment 28 on page 107 should be consulted.

(c) *The theory constitutes, as it stands, a lapse from the presumable parallelism of mind and body*; it fails to shew that the relations of mind and body, whatever they may be, follow any general scheme or plan; in fact, it suggests that they vary arbitrarily from one experience to another. For all would agree, I think, that the neural basis of the arrangement of the simplest sensory experiences in respect of their adherent localisations is, proximately or ultimately, the arrangement of neural units of some kind. Of course, we should not expect to be aware of the experiences correlated with these neural units, nor of their localisations, apart from some degree of excitation in these neural units. But neither should we expect to find that the essential aspect of their stimulation, with which alone experience is correlated, is the difference of excitation in them. For even if difference of degree of excitation were a necessary feature of the neural basis of the experience of motion, and of its direction and velocity, these experiences must first and foremost be correlated with the arrangements and interconnexions of the neural units and only secondarily with their difference of excitation. Difference of excitation would, then, be only a means of bringing different localisations with different clearness and insistency to the mind.

Thus we might revert to the simple theory of common sense and expect motion to be based upon the successive stimulation of neural units correlated with different positions. And it is to be noted that we have as yet no evidence that bears against this view or shews that the effect of motion is producible from simultaneously stimulated neural units, be they stimulated equally or differently. The facts of the after-effect of seen movement do not, of course, afford this evidence. They offer no other evidence than do the ordinary facts of motion. It is only in the eyes of such a theory as Wohlgemuth's that the stimulation of the neural units subserving motion is simultaneous and different. [When the stimulation of the elements of a neural complex in different degrees is said to be simultaneous, that means, of course, for Wohlgemuth as for others, simultaneous and continuous over a short stretch of time.] But he extends this explanation not only to the

Psychol. LIX. 324: "Im Vorbild stimmte die gesehene Bewegung nur nicht dann mit der berechneten Richtung überein, wenn irgendwelche Anhaltspunkte andere Auffassungen begünstigten," etc. Compare the effect of using broken lines and spirals, where the seen movement always corresponds to the objective movement.

after-effect of motion, but to ordinary visual motion. His hypothesis thus stands in sharp contradiction to the facts upon which it ultimately rests, that is, both to the facts of experience and to the facts known regarding the elements of the peripheral stimulation and their relative qualities, intensities, positions, and times. The positions of these elements differ in different times, so that in the several neural units stimulated by them, at least in those proximate to the stimulation, there must necessarily be successive differences, be they differences of intensity or of quality or of both together. Is it not, then, most reasonable to suppose that whenever motion is given, these successive differences occur throughout all the elements of its neural basis, be they proximate or remote?

Therefore it seems that Wohlgemuth's physiological theory of motion fails to shew that the relations of mind and body follow any general scheme or plan; or if it does so implicitly, it places a false emphasis on the part played by the intensive differences of neural processes in the correlation of mind and body.

II. THE INTROSPECTIVE NATURE AND AFFINITIES OF THE AFTER-EFFECT OF SEEN MOVEMENT.

The first task of psychology seems to me to be a thorough study of all distinguishable varieties of experience and their arrangement on the basis of their resemblance to one another, whether the resemblance be that of appearance (*e.g.* of attributes) or of functional properties and variations. We must form a periodic table of experiences, as it were, and we must take that table as the basis and object of explanation of every theory which is to be called psychological.

From Wohlgemuth's valuable historical and experimental researches it appears that the after-effect of seen movement has the following characteristics or properties[1]:—

C 1 The after-effect is an apparent movement, in a direction opposite to that of the previous objective movement.

E 16 Its velocity is comparable with that of an objective movement.

E 15 Its velocity acts as a velocity. It adds itself to an objective movement.

[1] *C* refers to conclusions by agreement between Wohlgemuth and his predecessors, *E* refers to Wohlgemuth's own experiments (the numbers are those of his text), cf. pp. 110 ff.

E 33 Certain observers mistake it for a real objective movement and are unwilling to believe the contrary (p. 87).

E 10—13 It varies in vividness (cf. pp. 46 ff.).

C 3 It is definitely localised.

C 4 It has a definite position in time.

These are its positive features. Negatively it appears that :—

E 32 It is not like "a shadow passing across the stationary surface."

E 33 As compared with an objective movement it has a hollow ghost-like appearance. Or it may have all degrees from reality to evanescence and ghostliness. As Wohlgemuth observes it, it is an experience *sui generis*. For him it never approaches the appearance of real objective movement. It lacks the solidity and reality that is given by change of position in space (cf. pp. 87 f.).

We must, therefore, conclude that, no matter how unusual the isolation of pure motion in the after-effect may be, nor how "unreal" it looks, it does greatly resemble its prototype of objective movement, fusing with the latter both phenomenally and functionally.

III. The Correlation between the Introspective Features of the After-effect and those of the previous Objective Movement.

The next question is whether the introspective kinship thus established is confirmed or contradicted by the evidence regarding the correlation between the introspective features of the after-effect and those of the previous objective movement upon which it is dependent. With which feature or features of simple sensation is the after-effect objectively connected? We may pass in review the chief attributes of sensation: (*a*) quality, (*b*) intensity, (*c*) order (local sign), (*d*) position in time, (*e*) extensity, and (*f*) duration. Of these, however, only the first four really come into question.

(*a*) *Quality.* The relevant facts are these :—

E 17—18 "The after-effect is independent of the quality of the light." The latter may be varied without variation of the former. Cf. *E* 19 below.

E 29 "Fatigue produced by alternating movements of opposite sign is independent of the colour of the light producing it, *i.e.* the fatigue is maintained in light of different colour."

E 1, *C* 11, and *E* 2—4 may also be cited, which shew the manner in which the clearness and vigour of the contents of the visual field reinforce the vividness of the after-effect. The after-effect is also noticeable in the dim field of subjective vision (eyes closed).

The conclusion, then, must be that the after-effect cannot well be produced apart from quality of some kind, but it is independent of the variation of the quality, as such, of the light. It is presumably produced by a factor which accompanies quality and which becomes, to some extent, more insistent as quality becomes more insistent. The explanation which Wohlgemuth offers of *E* 17—18 that "each new colour is a new stimulus" (p. 106) hardly seems consistent with *E* 29. Wohlgemuth's theory may explain the latter, but it can hardly explain the former. Wohlgemuth himself seems to feel this difficulty (cf. pp. 107 and 109).

(*b*) *Intensity.*

E 19 "In the case of different colours difference of brightness is not essential for the production of the after-effect."

E 14 "If a moving series of alternating dark and light stripes excite the retina, a slightly better after-effect seems to be obtained if the stripes be of equal width; but if the alternate dark and light stripes be not of equal width it seems not to matter which stripes are increased and which decreased in width."

E 2—4 "The after-effect is more marked in a brightly illuminated objective field...than in a darker field."

E 5—6 "If during the passage of images over the retina, a stimulus of a given intensity alternates with one of less intensity, the after-effect of movement produced is more vivid than if such stimulus alternates with a (more or less complete) cessation of stimulus."

C 1 "The uniform passage of light stimuli over the retina in any given direction...produces the after-effect."

The decisive case is *E* 19, which shews that a variation of intensity is not an essential condition. *E* 14 is only compatible with Wohlgemuth's theory if the special assumption discussed under I *a* is admitted. The other results, along with those referring to the difference between the light- and the dark-adapted eyes, are concomitant variations, which may depend not only upon the variations of intensity, but upon that of one of the other attributes. The

explanation of *E* 5—6 which Wohlgemuth gives (p. 104) seems strained: "When a black stripe succeeds a white one the synapses, which had been fatigued, immediately regain their former state." But the main theory supposed these synapses to be in a state of facilitation. If they are fatigued, *A* 1 should be more fatigued than *A* 2, having been excited more strongly longer, and the movement should have turned apparently to the direction opposite to that which it shewed at first.

The conclusion then must be that the after-effect cannot well be produced apart from intensity of some kind, but that it is independent of the intensity, as such, of the light. It is presumably produced by a factor which accompanies intensity and becomes to some extent more insistent as intensity becomes greater. If the after-effect is to be got, moreover, either the quality or the intensity must be varied. Both of these may, but need not, be varied at once. The after-effect, therefore, cannot well be dependent upon either of these attributes, but it may be dependent upon a factor which changes with differences in either or both of these[1].

(*c*) *Uniocular order* (local sign).

C 1 Quoted above.

C 2 "This after-effect is more marked if the eyes...remain fixed on a stationary point."

C 8 "The after-effect is producible by any rate of the stimulating movement."

[1] It is necessary to refer at this point to the preliminary notice of experimental results issued by Pleikart Stumpf, in which he says: "Es zeigte sich nämlich zunächst die auffallende Tatsache, dass bei sukzessiver Helligkeitsänderung einer Farbe des einen Farbenpaares sich eine Stelle finden lässt, bei der der Bewegungseindruck in den meisten Fällen vollkommen verschwindet, oder in einigen besonderen Fällen doch ein Minimum an Deutlichkeit erreicht. Zu jeder Farbe lässt sich auf diese Weise ein bestimmtes Grau finden, das mit ihr, so müssen wir wohl annehmen, einen unwirksamen Erregungsübergang bildet, so dass kein Bewegungsempfindungsprozess mehr zustande kommen kann" (*op. cit.*, 328 f.). The grey is that which gives the lowest fusional frequency with the colour concerned. If Stumpf's observations are correct, their inconsistency with those of Wohlgemuth may be the result of the difference of method adopted. Stumpf's method is essentially stroboscopic and his bands of colour are very narrow—two millimetres. No account is given, however, of the means of obtaining the necessary variation of brightness in the grey bands, which to give the result stated must have been most laborious. Until full details are given, Stumpf's result must be held in suspense. In view (1) of the restriction of a "minimum in some cases" which he indicates, (2) of the absence of any reference by Stumpf to differences of velocity, and (3) of the cumulative effect of certain differences in Wohlgemuth's results, Stumpf's case must be supposed to be an exceptional one. At all events, the theoretical procedure upon which alone I wish to insist here, must be applied to all relevant and stable experimental results.

C 10 "Pseudo-movements, *e.g.* stroboscopic movements, produce an after-effect exactly as an actual movement does."

C 7—9 "The after-effect increases in one or several ways, within limits, with the number of stimuli simultaneously affecting a given area of the retina, and or with the frequency with which the stimuli pass given retinal elements."

E 10—13 "The after-effect at first increases very rapidly with the objective velocity, but soon reaches a maximum and then gradually diminishes with further increase of speed."

E 14 Quoted above.

E 21, 1—4 In the periphery of the field of vision the after-effect is at first more vigorous, but diminishes and disappears very rapidly.

E 21, 5 "Any after-effect in a not-stimulated area is of opposite direction to that of the stimulated area." [Not weaker or less rapid.]

E 5—6, 1 Here Wohlgemuth says that "distinctness of contours is not the essential factor in the production of the after-effect." But it is evident from page 37 that "distinctness of contours" is only an alternative reading for "difference of brightness."

E 28 "After fatigue has been produced by a long series of movements alternating in sign (so that the after-effect is greatly reduced), the after-effect of movements at right angles to the direction of the previous ones is only very slightly affected, if at all."

E 26 "When several objective movements of different directions stimulate the same retinal area simultaneously or successively, an after-effect is produced which is the resultant of the after-effects of the various movements."

C 1 obviously admits the influence of order and *C* 2 provides a better basis for its regular introduction. Contour is the chief form of accentuation of visual position, so that the greater the number and frequency of the moving contours the greater the variation of orders (*E* 7—9). The impression of motion comes into full effectiveness more or less suddenly after a certain rate of motion has been obtained, but it becomes less clear with the higher velocities (*E* 8, *E* 10—13). *E* 14 calls for the operation of a factor which is independent of the division of the period between the light and dark portions. It is, on the other

hand, a well-known fact that orders and distances are clearer when they are regular and symmetrical. *E* 5—6, 2, which shews that a grey stripe is more effective in alternation with a white one than a black one is, may be supposed to involve a greater clearness of orders. For when black and white are juxtaposed, they must intensify each other by contrast, and so make irregularities of brightness of their surfaces less noticeable than they would be if the black were replaced by grey. That is to say, grey favours the distinction of positions, or, in other words, it allows of the existence of many orders, besides that given in the contours. With *E* 26 we may compare what was said above about the apparent movement of squares in an oblique direction, when two sets of parallel rods move at right angles to one another. If the after-effect is correlated with the neural basis of orders, directions, and motions, there is no reason why fatigue for one direction should affect the receptivity towards another direction at right angles to the first (*E* 28). Wohlgemuth's explanation of this result, on the contrary, must be said to be highly strained (*vide* p. 107). As regards *E* 21, 1—4, it is a commonly accepted fact that motion is more insistent in the periphery of the field of vision, but that positions there are not so highly differentiated as in the centre of the field. We might, then, expect a more insistent after-effect of briefer duration, rapidly disappearing. It is difficult to see what relation *C* 10, especially as described by Wertheimer, has to the varying intensity of pairs of movement centres. But their relation to differences of order and of time is obvious.

The conclusion must, therefore, be that the after-effect is correlated with, and directly or indirectly dependent upon, the order-differences of sensation given by the objective movement which excites the after-effect. There is no fact which suggests that the after-effect is independent of this attribute of simple visual sensation. *E* 1 and *E* 2—4 only imply that the presence of clear qualities and high intensities involves clearer sensational orders than does a darker or obscurer field.

Of *C* 9 Wohlgemuth says: "This result is probably merely a question of fusion of two retinal fields like results Nos. [*C*] 6 and 7" (p. 103). These binocular cases do surely belong to quite a different class of integrative processes to be studied separately from uniocular cases.

(*d*) The only other attribute which could come into question at all is that of *position in time* which represents rate of succession of stimuli.

It is undoubtedly involved in the production of motion and its after-effect, both in Wohlgemuth's theory and in any other. It is definitely involved in *C* 1, *C* 8, *C* 10, *E* 7—9, *E* 10—13, and *E* 14. Neither motion nor its after-effect is to be correlated with simultaneous sensations.

The following results do not apparently favour or disfavour any particular theory of the psychological or neural basis of motion and its after-effect: *C* 5, *E* 22, *E* 25, *E* 27, *E* 30.

The preceding investigation thus bears out the suggestions given by the psychical affinities of the after-effect of motion. This not only resembles motion, but it is related by direct psychical correlation with the experience of motion evoked by preceding objective movement, and with the conditions which favour or indicate a greater clearness of the orders of the sensations aroused by the objective stimulus. I offer no physiological theory alternative to that of Wohlgemuth. Nor do I mean to suggest that the after-effect of seen motion is linked to the preceding objective movement by any bonds of psychical causation. But I would maintain that the introspective nature of the after-effect is such that it resembles motion and order, while the correlations which experiment has established between the objective motion and the after-effect are such as to lead one to believe that the physiological basis of the after-effect is identical with that of motion and that both are connected with, and dependent upon, the physiological basis, not of intensity, but of order. A purely psychological statement of the resemblances and correlations between experiences must precede, not only every psychological theory regarding their connexion, but also, and *a fortiori* every physiological theory of their basis. It cannot be a safe proceeding to construct physiological theory by inference from psychological facts while the task of systematization of the psychological facts is neglected, whether a psychological theory of these facts is given or not.

IV. WERTHEIMER'S CRITICISMS OF CERTAIN PSYCHOLOGICAL THEORIES.

In Wertheimer's experiments, as in Wohlgemuth's, motion is seen when there is no real motion at all, but only the successive appearance, at times separated by varying intervals, of (usually) two brief stationary visual stimuli, *a* and *b*, separated by a short space, or at right angles to one another, like the two parts of the letter *L*. The

motion seen may be indistinguishable from the seen motion of a single real object, *e.g.* a short line turning through a right angle, or it may be double, as if first one small line made a movement through say 30° downwards from the vertical and then another small line through say 30° into the horizontal position; or it may even sometimes be so evanescent as to appear, apart from the motion of either small line, as a sort of pure, abstracted motion in a definite part of the field between the two lines, a mere 'going over' or torsion. All this, moreover, withstands the test of incognitive methods, just as does the examination of pictures shewn by the cinematograph. One may know the theory of the cinematograph or not, it makes no difference. So here again, the mind does not play a part in the production of the motion, not even by associating the parts omitted by the cinematograph with the parts shewn by it. For the motion will be seen even for objects that have never been actually seen by the spectator, *e.g.* an aeroplane, just as well as for the most familiar objects. Besides how could reproduction of the lost stages make the pictures move when they are not in motion at all, so long as they can be apprehended by vision as pictures? Thus we seem again to be driven to the physiology of the central nervous system for an explanation.

No objection need be raised against Wertheimer's physiological theory from the psychological side. Evidently it is only a theory of this kind which, as Wertheimer shews, can explain the facts relating to the production of motion by the stroboscope, the cinematograph, or other similar devices. I wish only to call attention to his criticism of those theories which attempt to regard motion as a form-quality (*Gestaltqualität*) or as a complex quality (*Complexqualität*) or the like, and which attempt to construct a psychological theory of motion from this leading idea. In Wertheimer's view these theories are put out of court by the fact that they demand that the motion which arises when the stimulations *a* and *b* are given in the manner described, shall apply to, and embrace, phenomenally both *a* and *b*. But, as Wertheimer has shewn experimentally, there are such things as "partial movements,"—*a* moving over one space and *b* moving over another space, the two movements being separated from one another by a small space; there is also such a thing as singular movement, when only *a* or *b* moves; and, best of all, the seen motion may not apply to, or embrace, *a* or *b* at all: these may be completely at rest and there may be in the space between them the phenomenon of pure motion or torsion, an experience much like Wohlgemuth's after-effect. Thus a theory which

suggests that motion is founded upon at least two contents, in this case *a* and *b*, may be dismissed without further comment. Besides such a theory would have to explain all the other facts gathered by Wertheimer, which, needless to say, it could never do[1]. Wertheimer, finally, offers a physiological theory of the facts, his theory of "physiological short-circuit."

It may very well be that this or that theory of the type criticized has, in its ignorance of the facts, attempted to explain what was known of the facts of stroboscopic movement by using *a* and *b* as "founding contents" (*fundierende Inhalte*). But a critic may be expected to see the virtue, as well as the vice, of a theory. Like the eastern monarch who was invited to witness a horse race, and replied: "I already know that one horse can run faster than another," may we not also say: "we know already that there can be two disconnected movements, or that one thing may move and another be at rest, or that a motion may take place in the space between two things without affecting either?" Surely if Wertheimer offers a physiological theory of his facts, he thereby discredits his criticism of the form-quality type of theory! A felt motion may have any manner of cause you please, so long as the felt motion is supposed to correspond to its subservient, central neural basis. In Wertheimer's experiments *a* and *b* are mere stimuli, not founding contents.

V. THE PRESENT THEORETICAL OUTLOOK.

I indicated in the opening lines of this paper that neither Wohlgemuth nor Wertheimer explicitly discusses the general attitude he adopts towards psychological and physiological explanations. They do not say why a psychological theory need not be offered for certain facts, nor why a physiological theory of these facts is admissible. Probably the reader is supposed to be sufficiently disciplined in these matters already. But if the relations between the component parts of a complex attitude remain obscure, there is grave danger that one of these parts may be over-emphasized and overworked, so that confusion results. It will therefore be well to discuss this attitude, to clarify the relations of its parts, and to find which should dominate the others if the best and most harmonious results are to be obtained. *The whole situation may be seen analytically by means of a survey from two opposite points of inquiry.*

[1] Cf. Wertheimer, pp. 242 f.

(*a*) *What reasons can be given for the absence of psychological theory?* There are three which may be imputed to these authors. Either (1) they feel convinced that there is nothing for such a theory to explain; or (2) they see in experience no basis upon which pure psychological theory might be built up; or (3) they are convinced that the facts of experience are mere discrete differences which can be explained only by physiological theory, based upon the special relations between experiences and the various features of the stimulative processes which evoke them. Acceptance of the third situation obviously excludes occupation of the first two. For if there is nothing to explain, there is no need for a theory of any kind; and if experiences are not connected in some way, but are mere discrete differences or qualities, the physiological entities (mechanisms, etc.) deduced therefrom will also be discrete and unconnected and therefore useless. And that is what we find; for just in so far as Wohlgemuth and Wertheimer identify pure motion or the after-effect with ordinary motion, they construct their physiological theories to accommodate both; and in so far as Wohlgemuth distinguishes motion from successive and continuous change of position[1], he must be held to give a purely illusory theory of motion or he assumes the existence of what he calls a "subcortical centre of movement[2]," and that, after all, is nothing but a ready made, specially created machine, which cannot have evolved out of the fundamental neural processes. But surely both the body and the mind must evolve; and if so each must evolve out of its own fundamental processes by the inner necessity and illumination that is given by progressively increasing effectiveness. To treat experience as a heterogeneous collection of elementary varieties, more or less similar, but essentially independent, therefore renders every scientific endeavour based upon the study of experience nugatory. Experience, like the starry sky which guides the sailor, is not merely one of the happy accidents of creation, merely "just so," and no more. It was a world of life before the sceptics tried to take it as an occasional, natural chart to the dark oceans of neural physiology. And it will be all the better a chart when it has again taken its place in knowledge as an ordered, inwardly coherent world.

If then we neglect the systematization and theoretical study of experience, we upset the natural hierarchy of the component parts in the complex task of the psychologist or psycophysiologist and so achieve confusion. Neglect to systematize experience leads to neglect to systematize the physiological mechanisms we imagine by inference.

[1] Wohlgemuth, p. 88. [2] *op. cit.* p. 99.

And without systematization there can be no theory of the evolution either of the brain or of experience.

(*b*) Adopting an opposite point of inquiry, we may now ask: *What insight justifies the confidence with which a physiological theory of certain facts is offered and admitted?* When double contacts give single touch, anyone apparently may understand that that is explicable only by a theory which assumes the existence of a single point of maximum central excitation and explains the way in which that arises out of the given double peripheral excitations. When an after-effect of negative sign arises from preceding objective movement, or when a movement arises from one or more resting stimulations, anyone may likewise understand that no laws of mind lie hidden here. The assumption of an indubitable parallelism of mind and body seems to be the only justification of these views. But this assumption, as we have seen, is abandoned by Wohlgemuth in his special physiological theory of motion and its after-effect. Why should we, then, retain it at all? Why not maintain that, when single touch results from double contacts, both the central and the peripheral excitations are double, and that single touch is due to the fact that for the two excitations the soul has rendered only one experience? Alternatively we might assume that the two excitations really did arouse two sensations, but that these two fused for some reason into one. Such assumptions have indeed been made, not perhaps for double contacts, but for those binocular stimulations which result in single fused vision. And no charge of absurdity or of obvious error could be brought against them. But these two cases of single touch and single vision from double stimulations are essentially parallel in nature. For the former only physiological explanations are generally admitted; for the latter physiological reasons have also been given, but they have been held to be utterly inadequate and psychological interpretations have been favoured instead[1]. If the physiological or the psychological line of explanation is preferable in special cases, there must surely be clear ground for the preference.

This ground seems to me to be a tacit recognition of the possibility and validity of pure psychological theory. In dealing with single touch or any other similar sensations, we recognise that there is nothing psychologically simpler and more primitive than elementary sensation itself to which we might appeal for an explanation of its characteristics. Consequently, if one class of sensations shews features which another does not possess, we feel justified in assuming that the anomaly must

[1] Cf. W. McDougall, *Body and Mind*, London, 1911, chap. xxi.

be due to the peculiar nature of the stimulus or of the receptor of that sense, *i.e.* it must be due to physical or physiological causes. All sensations, then, must be of one psychological class and of one psychical type and must behave, apart from extraneous causes, in the same way. This assumption is quite admissible as a working hypothesis, since no positive arguments can be brought against it, no matter how difficult it may be to establish it. On the other hand, the admission that, apart from discrete differences in quality and in the extent of range of variation of any attribute, sensations may be of different types, is scientifically self-destructive. For the departure from type means the failure of generalisation and therefore the absence of explanation. There can be no true science of psychology at all, unless the simplest sensations conform essentially to one type. Hence the common appeal from the psychology of the sensations to physiological theory implies both the admission of the assumption of types and the recognition of a fragment of pure psychology.

Similarly it is justifiable to offer a physiological theory of the after-effect of seen movement and of stroboscopic movement; for there seems to be no obvious psychical reason why the after-effect should be of a direction opposite to that of the preceding objective movement. If previous writers have offered psychological theories, a closer examination of the facts shews that the processes they appealed to are not involved in these experiences[1]. Nor is there any apparent psychical reason why the presentation of a successive series of stimuli differing in position should arouse the experience of a continuous movement over a distance or of many small neighbouring movements, etc. Besides, these peculiar effects are so like the experiences evoked by objective movements that we may at once assume that the physiological basis of the latter is identical with that of the former. Psychological theory has, then, only to classify and systematize the varieties of movement experience and to set them into relations of resemblance to the already classified simple sensations. The result of this task defines the problem for the physiological theory of motion, which has not only to imagine a neural basis of motion, but has also to shew how it is connected with the neural basis of the simple sensations, besides indicating, by reference to the incidental features of the physical processes taking place in these neural structures, how the anomalies of the correlation of external or preceding stimuli and consequent experiences (reversed after-effect, movement from stationary stimuli) are to be accounted for.

[1] Cf. Wohlgemuth, pp. 90 f. ; Wertheimer, pp. 240 f.

This attitude towards the problems of simple sensations and the simplest other sensory experiences is confirmed by a consideration of those cases in which two systems of sense-organs, eyes or ears, work together to make certain experiences possible. A careful survey of the problems is here made inevitable, because the facts suggest the view that the unity of binocular vision has no unitary neural counterpart[1]. We seem compelled to allow that we get unitary vision not only from double peripheral, but also from double central excitations. That the method of approaching these cases must also give *first place to positive psychological classification and theory* I have attempted to shew elsewhere[2].

[1] Cf. McDougall, *loc. cit.*

[2] "The Relation of Mind and Body," this *Journal*, 1912, v. 299 ff.

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