

THE SEMIAUTONOMOUS PROCESS: ITS NATURE AND NURTURE¹

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THIS report has to do with the present status of a class of theory, dealing with thought and perception. However, not theory alone—here and there an experimental fact is bound to show up in the fog, a navigational marker to show that solid ground is not far away (though it may be straight down). The theories in question are ones that try to analyze the complexity of the “central process” of Hilgard and Marquis (1940, p. 275). In the past I have used the adjective autonomous, but “semiautonomous” is better in view of much recent evidence showing that all higher activity depends continuously on a varied sensory background.

Observe that the theories in this class attempt to *analyze* the central process. They postulate complexity within it, and try to determine how the component subsystems relate to each other and to sensory and motor events. That is, they go further than the postulate of unitary “mediating responses,” each involved in a single action. The problem of thought, classically, concerns (a) the nature of the representative process, the idea, but also (b) how the sequence of ideas is determined (the “direction of thought”—Humphrey, 1940). The mediating process concerns the first question only. The theories I am now discussing are concerned with the second as well. This class is represented at present by Lashley (1958) on trace systems; Miller, Galanter, and Pribram (1960) on Plan and Metaplan; and my own cell-assembly and phase sequence (Hebb, 1949, 1958). These are all programmatic formulations, but at the present stage of knowledge, after all, a finished theory would be—finished.

Psychologically defined, then, the class consists

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of theories real or potential, present or future, which deal with an interaction of simultaneously present representative or mediating processes in control of behavior, as well as sensory-central interactions. Physiologically, the theories mean that transmission through the CNS (other than purely reflex transmission) is via a set of interacting closed systems and not linear.

For reasons given elsewhere (Hebb, 1959) my *Organization of Behavior* had to be specific to an unpalatable degree in order to show that a theory of neural connections in detail could still, in 1949, be consistent with the known facts both of behavior and neurophysiology; but the specific formulation, it was obvious, depended on too many arbitrary assumptions to have any hope of survival in that form; and in fact Moruzzi and Magoun (1949) and their reticular activating system made it obsolete in the year of its publication. If this was not enough, Brock, Coombs, and Eccles (1952) then presented decisive evidence, at last, of the cellular inhibition which earlier would have meant one more arbitrary assumption and which consequently I had to get along without. Such results put an end to that specific theory, but not the universe from which it was drawn—as we have seen, drawn more or less at random, as a sample should be. Instead these results opened up more powerful explanatory possibilities (Hebb, 1955; Lindsley, 1951; Milner, 1957). It is then on a class of theory that I recommend you to put your money, rather than any specific formulation that exists now.

Saying so, however, is not turning the other cheek; the critics have not all understood the issues involved, and there is still some vitality in the original approach. Let us first look at some data which concern the nature of the semiautonomous process of the cerebrum. We can then consider its nurture, and the archaic notion that perception and thought need no stimulus for their development but like Pallas Athene can spring fully armed from an adult brain.

STABILIZED-IMAGE PHENOMENA

The data in question are obtained by the method of stabilized images, in which the normal fluctuations of the image on the retina, due to tremor of the optic musculature, are prevented by mounting the perceptual object on a contact lens close to the eye, with a collimator to make it visible (Pritchard, 1961). The fundamental observation is the Ditchburn-Riggs phenomenon: the rapid disappearance of a simple visual object (a straight line) in these conditions of unvarying stimulation. The second major observation is that an object such as a circle or a square does not act as a whole, but breaks up in a way that provides information concerning the structure of perception. The method has been described elsewhere, together with a review of the literature leading up to this development (Pritchard, Heron, & Hebb, 1960).

Figure 1 shows what happens with square or triangle. The target, the diagram actually presented, is shown at the left. To the right of each is shown a typical series of changes in the target as perceived. The object may disappear as a whole, and regenerate part by part, for example, or it may disappear part by part. In any case, the separate part perceptions are all-or-none, and the total process is inconsistent with the idea that perception of the square or triangle is a unitary event.

In these results there is, for me, a further and more general meaning. This is the context: My earlier theorizing followed Hilgard and Marquis (1940), who pointed out that Lorente de Nó's (1938) closed pathways, re-entrant circuits in the brain, provide a way in which a central activity can become relatively independent of sensory input. Afferent excitation starts an excitation in one of these loops, which then continues round and round the loop even though the original stimulation has ceased. If, further, one loop can excite another, the independence from sensory input will be still greater. Thus the problem of the representative process, of imagery and ideation, might be solved in principle: An "idea" is the activity of a closed loop, and "thought" a succession of such activities. But the solution entails certain consequences, and it is these that relate to the stabilized-image results.

Lorente de Nó made it clear that the activity of a simple loop must be brief indeed, lasting for

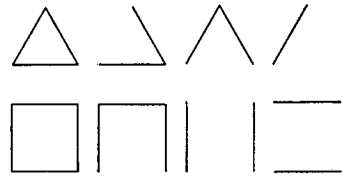


FIG. 1. All-or-none changes in perception with stabilized images. (The targets, the diagrams actually presented—triangle and square—are shown at the left. Three typical part perceptions in each case are at the right).

milliseconds only, and that transmission from one loop to another must be highly unreliable. More complex loops could act longer, and still more so several of them temporarily acting together; moreover, a statistical reliability of function could occur in this way. Consequently in developing these ideas (Hebb, 1949) I assumed that complex loops—which I called cell assemblies—would develop with repetitive stimulation, and that these, when active together, would develop interfascilitation, supporting one another's action. For various reasons it was necessary to assume that the single assembly would correspond not to the whole of a perceived object but to its component parts: in the visual domain, to single lines, intersections of line, colors, and so on. These were, as a result, regarded as perceptual elements. A "superordinate" assembly might later develop for an object such as a square, but the perception of the square in the first place would consist of the coordinated activities of four distinct systems (or eight, if both lines and angles are involved).

Now you can see how the situation appeared to one who made these assumptions (but who was not used to such an agreement with data) when Pritchard and Heron invited me to put on a contact lens and observe the fragmentation already described (Figure 1). I would have predicted that the fragments would include angles, not only lines, and I could not have predicted that the retinal stabilization would be enough to cause the breakdown;² but the breakdown when it did occur fitted closely into the theory. Add that the

² The relations of cortical unit firing to retinal stimulation have only begun to be worked out. (cf. Burns, Heron, & Pritchard 1962; Hubel 1959; Jung 1961). On the mode of fragmentation, it should be noted that Eagle and Klein (1962) have found angles predominating over lines, and the reason for this discrepancy from the results of Pritchard et al. is not clear yet.

part of a diagram attended to remains visible longer (the theory says that attention is a supporting facilitation from other assemblies), and a meaningful object longer than a meaningless one (meaning in the theory is activity in a set of interconnected assemblies which, as a more complex system, can be active longer), and the correspondence becomes close indeed. I, at least, find it hard to avoid the conclusion that the cell assembly has become a much less speculative notion.

But—no leaping to conclusions. As we shall see, this is not a general confirmation. The same data that support the theory in one aspect, giving new force to the cell-assembly notion broadly conceived, deny it quite definitely in other aspects.

A theory of this general class, however, seems to be required. The data still indicate strongly that perception of square, triangle, or circle involves activity in a number of separable systems; and they make a strong case for localizing these systems outside the sensory projection, thus putting them into the category of mediating rather than sensory processes. And, if this is so, they provide a new and more direct source of information about the semiautonomous process.

Let us look at the implications in more detail. The all-or-none disappearance of a straight line cannot simply be a cessation of firing due to fatigue in independently functioning cells; nor can it be due to local areas of "satiation" (Köhler & Wallach, 1944) or of fatigue, either in the retina or in the geniculo-striate projection. We can immediately rule out the retina as the primary locus, since Krauskopf and Riggs (1959) and Cohen (1961) have demonstrated an interaction of the two eyes. The origin of the effect is central.

Now consider Figure 2. This represents what goes on between retina and visual cortex, when the subject is presented with a stabilized letter T. What happens when the subject reports that the crossbar of the T has vanished, all at once? It

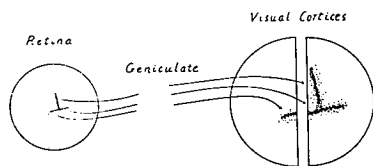


FIG. 2. Diagram of the cells excited by a stabilized letter T in the central retinal area, and in the two visual areas (left and right hemispheres).

is hard to suppose that all the hundred thousand or more cells (in two separate visual areas) become fatigued and cease firing simultaneously; still more, that their precise timing does not include cells excited by the upright bar of the T, but that these in turn, when they become fatigued, also cease firing simultaneously. A steadily increasing fatigue or cortical satiation should appear first as a fading, then a patchy disappearance spreading to include the whole figure. Neither accounts for the sudden transition from a full, clear perception of the crossbar to its complete absence, or for the sharp discontinuity at the point of intersection with the upright. It is known that the cortical effects of retinal stimulation fall off sharply if the stimulation is unchanging (Burns et al., 1962), but this cannot explain the disappearance of part of the target 10 to 15 seconds later; the loss is immediate, and includes the whole of the target (cf. the reference to Figure 4 below). Is the effect due to inhibition instead? But here again, we must ask how the thousands (at least) of inhibitory cells are capable of the same precise coordination in time.

A quite different kind of explanation would account for all these phenomena by slippage of the contact lens, which can and does occur when the lens is not tightly fixed on the eyeball. The idea is that with a perfectly fixed lens, and thus perfect stabilization of the image, the target would simply disappear as a whole. Reappearance of part of the target would occur when slippage moves the retinal image so that new retinal cells are excited. Slippage that is parallel to a line would have little effect, because the locus of the line on the retina would change little, but slippage transverse to the line would mean that the image of the whole line would be moved to a new set of retinal cells. With T as target, a slight vertical slippage, moving the image downward, would restore the crossbar to vision, because it would be moved as a whole to a new set of rods and cones; but the upright would still be exciting exactly the same rods and cones as before, except for a few at the end of the line, and so it would not reappear. With lateral slippage, of course, the upright would be seen instead; and with diagonal slippage, the whole letter T. But the evidence is opposed to this as a sufficient explanation of the way in which fragmentation occurs, though it may well be part of the story. When the target is for

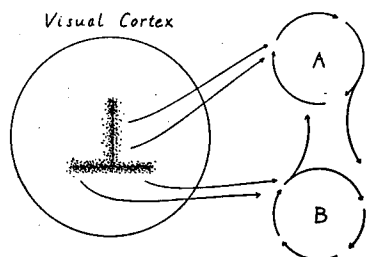


FIG. 3. The cell-assembly hypothesis. (A and B represent separate systems excited by upright and crossbar, and capable also of delivering excitations to each other.)

example the word BEER, among the products of fragmentation are the words PEER, PEEP, BEE, and BE, as well as some nonsense words such as BLLR or BFFR (Pritchard, 1961). When one loop of the B drops out and the other does not, leaving P instead of B, or when one E drops out but not the other, the slippage hypothesis by itself becomes inadequate. Slippage probably contributes to the amount of time the target remains visible, but does not determine the way breakup occurs.

The major determinant of the form of fragmentation, it seems clear, involves the internal properties of the sensory or perceptual process. If this is so, the cells whose activity is the percept of a part of the figure, one of the "fragments" that appears or disappears as a unit, must be interconnected in some way so as to form a system which can be active or inactive as a whole. Where would this system be formed? It can hardly be in the sense-dominated Layer IV of the visual cortex, and it would certainly not be produced by stable synaptic connections between cells of the optic radiation, or the cortical cells with which they synapse directly, because the coordinated action would then be found only in specific loci; when the target is presented in other orientations, breakup of the figure would have a different relation to its parts, changing with each change of locus. This is not observed. Small or large changes of locus, over the central 2° field examined in these studies, do not affect the mode of fragmentation.

Figure 3, however, diagrams a mechanism which does provide both for a unitary action of parts of the figure, and for some coordination of the parts, to permit the greater-than-chance frequency of perception of the whole figure. This of course is the cell-assembly hypothesis. The original analysis

of anatomical relations (Hebb, 1949) showed that a precise localization of the stimulus pattern is not necessary for such an assembly to function. In the diagram, System A is fired by visual-cortex cells whose firing, in turn, is controlled by the vertical bar of the T, System B by those corresponding to the crossbar. A and B, further, facilitate one another's firing, so that their activities are correlated.

What now happens when the T is presented as stabilized image? As far as one can judge from the physiological evidence, the first change is one represented by Figure 4: a sharp decrease in the number of visual-cortex cells firing under control of the retinal stimulation. It may seem hardly necessary to prepare a special diagram to make the point, but I have done so in order to emphasize that this loss, which occurs throughout the whole figure area in the first fraction of a second following stabilization, does not in itself constitute the disappearance now of one part of the target, now another, after 10 or 15 seconds of exposure.

From the data of Burns et al. it seems clear, however, that stabilization results in a sharp diminution of the number of cortical cells being controlled by the retinal stimulus. The phenomena can be understood on the assumption that there is normally a considerable margin of safety in the visual-cortex control of the closed Systems A and B in Figure 3. With stabilization the margin is reduced; any slight further reduction, or fatigue in A and B ("summation of subnormality"), could then lead to the sudden cessation of activity in these systems, singly or together. These ideas are consistent with the fact that almost any disturbance which might be thought to raise the level of activity in the arousal system, and hence of cortical activity, is likely to regenerate the perception of a target that has disappeared: an unexpected noise, for example, or even the action of pressing a key to signal disappearance of the target (Dicara & Barmack, 1962; Kader, 1960).



FIG. 4. Attenuation of cells fired in visual cortex in stabilization conditions. (Left, cells fired at first presentation of the target; right, after a second or so of exposure.)

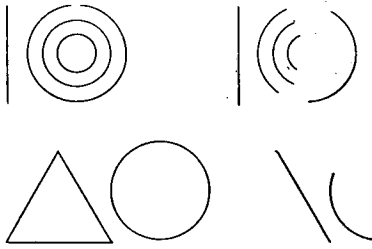


FIG. 5. Two targets (left) and a repeatedly observed form of fragmentation for each (right) showing the influence of a neighboring straight line upon the percept of a circle.

FURTHER ANALYSIS

Other phenomena of very considerable interest have been observed with the stabilization method, some of which allow us to carry analysis further and say something about the relation between-cell assemblies. One of these is the frequent occurrence of visual field effects, where one excitation modifies the effects of another close to it. In Figure 5 two targets are shown at the left and a frequent mode of fragmentation, for each, is shown on the right. In each condition the presence of a straight line near the circle affects the apparent behavior of the latter.

The clearest case is found in the tendency of parallel lines to act—that is, to appear or disappear—together. With a stabilized line seen by one eye, an unstabilized line parallel to it seen by the other, Cohen (1961) found that the stabilized line was visible a greater percentage of the time than when seen alone. The closer the two, in

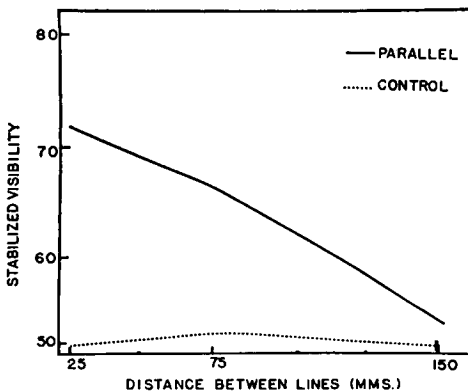


FIG. 6. Percentage of time a stabilized straight line is seen by one eye when an unstabilized parallel line is seen by the other eye, at separations of 25-, 75-, and 150-minute arc. (Broken line, percentage of time visible when the parallel line is not present to the other eye.)

terms of retinal angle, the greater the effect. Figure 6 gives these values for the fifth minute of viewing for one subject, for separations of 25', 1° 15', and 2° 30' (together with control data: percentage of time visible without the unstabilized parallel line). The same general result is obtained when both lines are stabilized, and the time recorded when both are present in vision, as contrasted with one only: the closer the lines, the closer the relation between their activities.

This reciprocal influence, diminishing with distance, satisfies the criteria of a field effect, and there is no reason to doubt that there are such effects in visual cortex. However, there are two ways in which the interaction might occur, one a field process, one not. Figure 7 represents the activity of two parallel lines in visual cortex, and also the cell assemblies which they may be supposed to activate. Spread of excitation from the

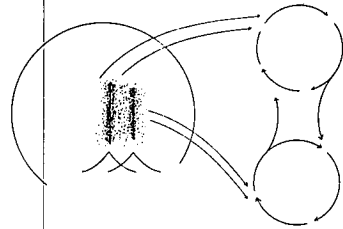


FIG. 7. Two possible modes of interaction of parallel lines—at left, visual cortex, overlap of the two frequency distributions; at right, two cell assemblies aroused by the two lines, acting directly on each other.

two "ridges" of activity (regions of maximal firing caused by the two lines—Marshall & Talbot, 1942) results in an overlap in the visual cortex, represented in cross section at the lower left. This, in effect a field action, means a primary influence from one visual-cortex activity on the other. Another possible mechanism, however, is the direct connection that is shown between the two cell assemblies. This connection would be expected because of the high frequency of parallel excitations of the retina in ordinary experience, whenever there is eye movement in an unequally lighted environment (Hebb, 1949, p. 82f).

A simple experiment (Tees, 1961) shows that both mechanisms operate. The target (Figure 8) is made up of two parallel lines, A and B, on each side of a third line, C, with a slope of 45°. (The identifying letters were not present in the experiment.) Recording only part perceptions

(i.e., disregarding periods when the whole figure is present, or when none of it is), we can take one parallel line, A, and ask how often it is accompanied by C, nearer to it, and how often by the more distant line (B). Taking the means for four observers, we find that A is accompanied by C in 22% of the reports; by B in 73%. (About the same result is obtained when B is treated as the reference object.) Any field effect extending from one parallel to the other must have even greater effect for the intervening line; it therefore follows that the correlated activity of parallel lines is only partly due to a field action.

In this discussion I have emphasized the all-or-none action of the perceived parts of a line diagram. It is important to observe that another mode of change, analogical instead of digital, can also be observed when one uses solid instead of outline figures. The changes shown in Figure 9

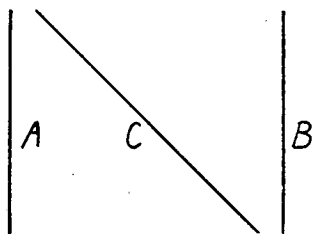


FIG. 8. Test diagram. (See text.)

(Pritchard, 1961) are exactly those that would be expected from a local, spreading satiation or fatigue in the visual cortex. The fading that begins in the middle spreads outward to engulf the middle part of one or more of the sides, leaving corners intact. (Here it is not the line edges of the square that act as perceptual entities, but corners—angles, not lines. The corners are either present and well marked, or absent.) This demonstration of a graduated fading points up the contrast between what would be predicted from (a) local fatigue in the sensory field, and (b) the failure of a cell assembly or group of assemblies. Showing that a gradual disappearance *can* occur strengthens the argument that the quite different all-or-none disappearances discussed earlier must be due to the failure of a system or systems.

But now a difficulty: how to account for this graduated change? The theory makes perception the digital activity of cell assemblies and does not provide for analogical processes within them.



FIG. 9. Mode of fading of solid square, as contrasted with behavior of outline figures.

In ordinary conditions of perception continuous gradations of course are common, but they might be accounted for in terms of the amount of background activity of other visual assemblies. Such an explanation is ruled out in the stabilized-image condition. The same difficulty is found in the fact that there is a progressive loss of intensity, or contrast, with prolonged viewing of other targets.

Another difficulty of a similar kind appears in certain completion phenomena. Fortunately they have an intrinsic interest, and are worth referring to even if you are already convinced that the cell-assembly theory is not perfect. Completions in the form of hallucinatory additions or substitutions that may make the target a "better" figure in the Gestalt sense, or make it more meaningful, are observed in a number of conditions. Some of these are illustrated in Figure 10: One limb of an ameba-like pattern (top left) drops off, but the wound is healed by a closure of the lines from either side (top right); below, in a similar pattern, a limb is replaced by one that is more in proportion to the others (Pritchard, 1961). Figure 11, the missing parts of a triangle are supplied by the subject's own perceptual processes (left, targets; momentary perceptions of each at the right).

The difficulty referred to appears in Figure 11, where the completions are, so to speak, incomplete. They do not make the triangle appear,

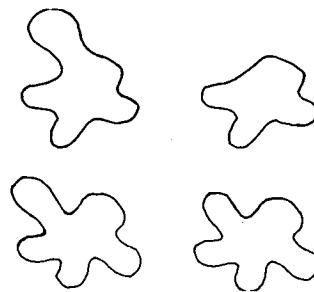


FIG. 10. Left, targets; right, occasional perceptions. (Above, a limb has disappeared, but a completion process heals the gap. Below, a limb—upper left quadrant—is replaced by one more in proportion.)

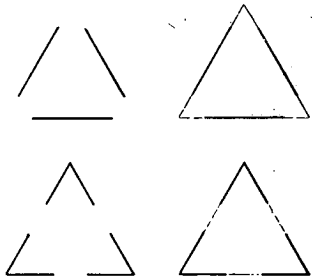


FIG. 11. "Incomplete completions." (Left, targets; right, occasional perceptions, the filled-in parts more tenuous, less "solid" than those composing the targets.)

even momentarily, as though it had been drawn whole; the gaps are filled in with weaker, more fragmentary, or insubstantial lines. Here again is a difference of degree in the postulated cell assembly activity.

There are other results which cannot easily be accounted for by the theory as it stands. Replogle (1962) for example has found an interaction between similar patterns which is not reducible to linearity effects or the interaction of parallel lines, and I have no doubt that such difficulties will increase as research goes on. One could of course make changes to bring theory and data closer together. It might be proposed that the perception of a straight line, for example, is not the activity of a single assembly, nor two or three, but many; and it may be feasible to develop the notion of superordinate assemblies much further, distinguishing between so-to-speak "sensory" assemblies (tied closely to afferent input) and higher-order ones. But I have no confidence that these are the right changes, and it is premature to elaborate such ideas without better criteria of fact by which to judge them.³

The data that have been presented, then, strongly indicate that something like the postulated cell assembly exists and plays a major role in perception. The data also allow us to make some reasonable inferences concerning locus, separating for example field effects in the visual cortex from

³ A paper by Hubel and Wiesel (1962), for example, has just come to hand while this was being written. It provides information concerning the organization of receptive fields in the visual cortex and a selective effect of patterns of retinal excitation upon cortical units which suggests a relation to Lashley's (1942) interference-pattern hypothesis of visual perception. The "sensory" assemblies referred to may turn out to lie within visual cortex itself, though presumably not in Layer IV.

the activity of the closed systems that also function. At the same time the data contradict the theory as it stands, showing clearly that it needs modification or extension if it is to comprehend the facts. (It has already been "disproved" by other behavioral evidence—Hebb, 1955, p. 247 f.—but the present disproof should be clear enough to put an end to rumors that the theory is too vague to be testable.)

So much for the nature of the semiautonomous process, and the cell assembly as a component part. Now, nurture, and some of the criticism that has been directed at these ideas.

THE QUESTION OF NURTURE: CRITICISMS

The theory proposed that the cell assembly must have a developmental history as a function of early experience and not physical maturation alone. This idea helped stimulate a number of studies of intrinsic value (cf. Beach & Jaynes, 1954; Riesen, 1961; Thompson & Schaefer, 1961), but it also ran afoul of ideas about the heredity-environment question that Attneave (1962) has called medieval, and that at least should not have survived Beach's (1955) clarifying analysis.

For some students,⁴ apparently, it was offensive even to use the word "learning" in the context of perception. These critics still ask the archaic question, whether perceptual organization is wholly dependent on learning during growth, or wholly independent of it. For them, consequently, the notion that figure-ground segregation is present in first vision ("primitive unity") but still develops and changes with experience, or the notion that a perceptual object may have, innately, a low-level property of "identity" or identifiability which increases radically with repeated exposure (cf. "the acquired distinctiveness of cues"—Lawrence, 1950)—for them, such notions are both incomprehensible and distasteful. In the eyes of one critic I had "straddled" the issue, unable to make up my mind; for others, I was clearly saying that learning is the whole explanation of perception. Neither is true. Perhaps I may say again, that perceptual organization does not start at zero, but also that adult perception includes experience among its determinants, as it includes

⁴ Followers—at a distance—of Gestalt psychology, a movement that now has more to fear from its friends than its enemies.

the genetic factor and the nutrient environment. These factors work closely together. It is grossly misleading to say either that normal adult perceptual organization is learned, or that it is unlearned.

That loaded term, learning, also relates to a substantive criticism, and my use of the data of Senden (1932) and Riesen (1947) in support of the view that experience is an essential factor in the development of the cell assembly and hence of thought and perception. Fortunately, the study of congenitally blind patients, given vision later by surgical means, is now available in English translation (Senden, 1960) and you can judge for yourselves whether it is merely a collection of old wives' tales or instead an invaluable compendium of case reports: cases of a kind that are almost never seen today because of improved knowledge of prevention or early treatment (but see the report of Pokrovskii—London, 1960). It is true that Senden compiled the popular as well as the technical reports, for completeness, but he evaluated the evidence critically and competently; it is true that the nineteenth-century investigators did not make some of the tests that current theory would dictate, but the cases contain factual data whose meaning is unmistakable, reported by competent workers in such sources as the *Zeitschrift für Psychologie* and the *Psychological Review Monographs*, or in a *Festschrift* for Helmholtz's seventieth birthday—not places where one would look for anecdotalism.

As for Riesen's study of chimpanzees reared in darkness: The results (which at the time were frankly astonishing to all concerned) indicated that the animals had no visual perception at all when first exposed to a normal environment and, apparently, then learned slowly to see. However, it was next discovered that rearing in darkness results in the failure of some neurons to develop, and chemical deficiencies in others (cf. Riesen's review, 1961). Presumably, congenital cataract in man might have similar effects. This discovery was advanced to rebut any interpretation of the defects in terms of a lack of learning: It was said that the lack is physical, not psychological.

However, the loss of neural cells was from the first a very limited argument, since perceptual capacity developed with experience and, since cells in the central nervous system once lost do not reappear, it is clear that the subjects had the

necessary neurons for a fair level of perception immediately after the cataract operation or on being brought out of darkness, when perception was grossly abnormal. Some of the perceptual lack undoubtedly can be ascribed to constitutional causes, especially since it is clear that the cataract cases are worse off than the newborn infant; but not all of it.

But there is a more fundamental point. These newer data, concerning cell loss and chemical deficiencies, offer no support for nativism. They are not in opposition to the idea that learning is part of perceptual development, but point in the same direction—that is, to the importance of sensory stimulation in development. For what does "learning" mean, at that histological level? It must refer to *either* the finer outgrowth of the neurofibril and its synaptic knobs, *or* changes in the chemical properties that make synaptic transmission possible (or, of course, both). My hypothesis was that one or other depends on prior sensory stimulation, and the trouble with the hypothesis is not so much that it was wrong, as that it did not go far enough. Not only synaptic endings but the growth and viability of the whole cell are dependent on such stimulation.

The only way in which an avowed nativist can regard these later results as an acceptable alternative to learning, as far as I can see, is for him to think of "learning" as solely the acquisition of specific overt responses to specific stimulus objects. This of course must be rejected as the basis of perceptual integration. But the alternative view, that perceptual integration can proceed normally in the absence of sensory stimulation, is equally unacceptable. "Learning" is indeed a loaded term, and a multivalent one: Communication perhaps will not be possible until we start using special terms for its various aspects, including particularly (for the benefit of learning theorists as well as nativists) the nonspecific organizing effects of sensory stimulation upon neural development and integration during the period of growth. For the present, at least, we may note that the interactions of experience with the genetic and nutritive factors have become really inextricable. It is no longer possible to distinguish sharply between constitutional and experiential factors in the development of behavior. It certainly goes beyond any usual meaning of the term *learning* to include in it the whole outgrowth of the cell; but if we

grant this, grant that there is more than a lack of learning in the dark-reared chimpanzee's failures of perception, there is, still to be accounted for, the perceptual development that does occur with the neural equipment that is still present; even more, there are also the perceptual deficiencies of Riesen's (1961) animals reared in diffuse light. Such evidence still shows that there is a role of experience in the development of a normal perceptual capacity.

Finally, among these criticisms, Harlow, Harlow, Rueping, and Mason (1960, p. 119) believe that their study of the young monkey has refuted another aspect of the theory. If the cell assembly develops with experience, this implies that the process should be slower in the brain of the higher animal, because of the greater number of neurons and a higher "noise" level. The first learning of a higher animal, then, should be slow compared to that of a lower one, to the extent that the higher learning is "cognitive" and perceptual and thus dependent on assembly function (Hebb, 1949). Harlow et al. believe that they have shown this to be untrue.

I find the conclusion puzzling, since they do not show that the monkey's first learning is faster than that of the rat or slower than the chimpanzee's or man's. They have not made such comparisons at all, and they did not really study the monkey's first learning. Their youngest subjects were 60 days old, not reared in darkness, and a great deal of visual learning must have occurred before formal training began. This point is reinforced by the fact that what they have reported is a set of preference rather than learning scores (20% of their animals made zero errors, for example), which must of course reflect the earlier learning.

The related experiment by Zimmerman (1961) actually used neonate monkeys as well as 11-day-old ones. His success in training at these ages is an achievement which commands admiration, but the result still does not show that the monkey's first learning is faster than that of lower species, or slower than higher ones'. For example, his neonate subjects took 20 days of training, or a total of 203 trials, to reach a criterion of 85% correct discrimination of horizontal from vertical striation. Rats reared in darkness took only 12 days and 118 trials, and this to a criterion of 100% (Hebb, 1937). The rats were adult, the monkeys immature, it is true, and I do not mean to press

this comparison too far; my only point is that what data there are do not oppose the conclusion that the first learning takes longer in the larger brain. If anything they tend to support it, though the support may not be decisive.

PHYSIOLOGICAL PSYCHOTHERAPY

I know that uninhibited theory is distasteful, and physiology worse. Psychologists today pride themselves on not indulging in theory or, if temptation gets too strong, taking it in the form of a "model" that no one will mistake for reality. Using physiological conceptions makes things too explicit: As one writer has pointed out, the theory might then be disproved by some "irrelevant" physiological experiment (though how it can be irrelevant, if so, is not clear). Models are safer. And they take less work: There is not so much literature to master.

But there is a penalty to pay. The stimulus-response learning theorist still has an infantile neurology in his unconscious, which makes attention, sensory-sensory associations, imagery and ideation all animistic notions, scientifically incomprehensible—as indeed they were, in 1920 (Hebb, 1951). They are not incomprehensible now, but the phobia persists. It must, till the patient is persuaded to relive the trauma, go back to his infantile neurology, and find out that things are not what they seemed in his youth.⁵

But the configurationist who is just agin perceptual learning, on principle, suffers from the same neurosis. He too has an unconscious neural fantasy. At a purely behavioral level, excluding physiological ideas, there seems no reason to deny perceptual learning. From Leeper (1935) onward the evidence has accumulated in the modern period (cf. Solley & Murphy, 1960). But historically a theoretical and neurological origin for this attitude becomes evident. In its attack on atomism, early Gestalt psychology used the available anatomical ideas, and very effectively. The argument for an intrinsic *sensory organization* (Köhler, 1929) on psychological grounds fitted precisely into the developing knowledge of topographical organization in sensory cortex, this in turn reinforcing—or making possible—the conception of psychophysical isomorphism. Visual

⁵ S. Freud. *Die Kastrationskomplexe bei den Psychologen*. A little-known work.

perception, in effect, was identified with visual-cortex activity. Its innate organization, determined by the distribution of fibers in the afferent projection, was sensory as much as perceptual, and no useful distinction between these latter terms remained. Learning could have no role in the organization, for it was hardly reasonable to suppose that experience could rearrange afferent fibers. Unquestionably, these ideas were a great step forward, but not the final step. Their theoretical nature, partly of neurological origin, must be recognized when they begin to restrict further steps.

Teuber (1960) has pointed out again, as I did earlier (Hebb, 1949), that the completions found clinically with scotomata make it impossible to localize perception in sensory cortex (though Teuber, Battersby, & Bender, 1960, seem to make such an assumption in their analysis of the distribution of projection fibers to visual cortex). If not there, where is it? The data discussed earlier indicate that it is the activity of systems developed by experience and lying in the divergent-conduction areas of the brain, part of the semi-autonomous process. In that case a real distinction of sensation from perception exists, physiologically, and in psychological terms we may define perception as the activity of mediating processes directly excited but not fully controlled by sensory input, and so characterized by the "fluidity" that Osgood (1953) has emphasized.

A considerable freedom for psychological analysis results. The ambiguous figure need no longer be the theoretically ambiguous item it has usually been in the textbooks: always mentioned as important, but what its importance not explained, except by Osgood. If, in Rubin's famous example of vase and faces, the percept of the vase is one mediating process or set of processes, the percept of two faces another set, the phenomenon takes on meaning. The enigma vanishes when we distinguish between the invariant consequences of

FRANCE

FRANCE

FIG. 12. Left-right reversal.

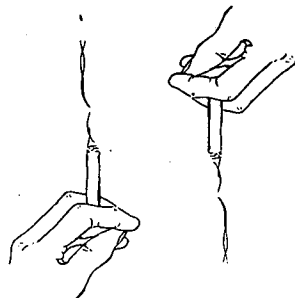


FIG. 13. 180° rotation of visual field.

stimulation (properly considered sensory), and the labile perceptual consequences which are subject to set and, to a major extent, developed and modifiable by experience.

Let me conclude by reference to another enigma. The work of Stratton (1897) on adaptation to distorted vision, and later that of Ewert (1930), has had a curious status in the minds of psychologists. Everyone knew about it, and everyone forgot it. It was obviously important, but yet irrelevant in almost any discussion of theory. At least, it was rarely mentioned. Because of Kohler's (1951) more recent work, and that of Snyder and Pronko (1952), again confirming the tenor of Stratton's results and providing valuable new information on the course of adaptation, we are more aware of the phenomena than we used to be. The puzzle has remained, however, and now perhaps we can see why it should seem more of a puzzle than some of our other inadequately-accounted-for perceptual phenomena.

To show what I have in mind let me first draw attention to the transfer that occurs when one learns to read mirror-image letters—the task of the printer who sets type by hand. In Figure 12, the word FRANCE is presented, with its mirror image below. What common features do these have, as visual configurations, that are not common to other words? Not much, considering them as static wholes. But as one moves his gaze from left to right in the upper word, or right to left in the lower, an identical series of events occurs: a vertical line, with two attached parallel lines following, in the upper half of the field (the F); a separate vertical line, followed by an attached semicircle above and a bar sloping downward below (R); and so on: an identical series of events, described in these terms.

Now look at Figure 13, which represents a hand

with a cigarette, and the same inverted. Movement of the eyes upward in one case, and downward in the other, result in a series of impressions which have the same kind of communality as the two words in the preceding case. So much is obvious.

Now suppose that perception of the burning cigarette (or the word FRANCE) is a serially ordered activity of mediating processes or cell assemblies, even when the object is recognized at a single glance, and that the orientation is a function of the serial order. What change is required in order to see the inverted cigarette upright? A change in the order of firing of cell assemblies, only, and such a change is easily understood. The enigma, the source of the incredulity with which Stratton's experimental results were received (until verified beyond question), lies in the idea that the *sensory* representation of the upside-down object must somehow get turned around when it is seen right side up. In its crudest form, the apparent implication here is that the innate organization, the structure, of the visual cortex is changed, which of course is nonsensical. But if *perception* of the cigarette is not the activity of visual cortex, the implication does not hold, and there is no inherent contradiction in the data. The fact that reorientation is for specific situations or objects, not a general reordering of the visual world, familiar and unfamiliar equally, is in accord with this suggestion; and the evidence of Held (1961), indicating that adaptation to other forms of sensory distortion is closely related to voluntary movement and the feedback from it, is also in accord.

What I mean to emphasize here is the new possibilities of explanation that open up when one separates sensory from perceptual processes, and recognizes that identifying the two had a purely theoretical origin, and neurological to boot. This can be a psychological distinction, based on behavioral criteria, and need not be contaminated by other ideas. Yet it is clear that there is such contamination, historically, running all through psychology; and the only way of dealing with it may be to get the anatomical and physiological element out in the open and show that current ideas are less crippling than the older ones. Psychology cannot be reduced to physiology (psychology is a more difficult discipline?), but it

seems evident that the psychologist may need every now and then a short bout of physiological psychotherapy, just to permit him to get on with his own business.

REFERENCES

- ATTNEAVE, F. Perception and related areas. In S. Koch (Ed.), *Psychology: A study of a science*. Vol. 4. New York: McGraw-Hill, 1962. Pp. 619-659.
- BEACH, F. A. The descent of instinct. *Psychol. Rev.*, 1955, **62**, 401-410.
- BEACH, F. A., & JAYNES, J. Effects of early experience upon the behavior of animals. *Psychol. Bull.*, 1954, **51**, 239-263.
- BROCK, L. G., COOMBS, J. S., & ECCLES, J. C. The recording of potentials from motoneurons with an intracellular electrode. *J. Physiol.*, 1952, **117**, 431-460.
- BURNS, B. D., HERON, W., & PRITCHARD, R. M. Physiological excitation of visual cortex in cat's unanesthetized isolated forebrain. *J. Neurophysiol.*, 1962, **25**, 165-181.
- COHEN, H. B. The effect of contralateral visual stimulation on visibility with stabilized retinal images. *Canad. J. Psychol.*, 1961, **15**, 212-219.
- DICARA, L., & BARMACK, J. E. The effect of reporting procedures on the stabilized retinal image. Paper read at Eastern Psychological Association, Atlantic City, April 1962.
- EAGLE, M. N., & KLEIN, G. S. Fragmentation phenomena in the stabilized retinal image. Paper read at Eastern Psychological Association, Atlantic City, April 1962.
- EWERT, P. H. A study of the effect of inverted retinal stimulation upon spatially coordinated behavior. *Genet. Psychol. Monogr.*, 1930, **7**, 177-363.
- HARLOW, H. F., HARLOW, MARGARET K., RUEPING, R. R., & MASON, W. A. Performance of infant rhesus monkeys on discrimination learning, delayed response, and discrimination learning set. *J. comp. physiol. Psychol.*, 1960, **53**, 113-121.
- HEBB, D. O. The innate organization of visual activity: I. *J. genet. Psychol.*, 1937, **51**, 101-126.
- HEBB, D. O. *Organization of behavior*. New York: Wiley, 1949.
- HEBB, D. O. The role of neurological ideas in psychology. *J. Pers.*, 1951, **20**, 39-55.
- HEBB, D. O. Drives and the CNS (conceptual nervous system). *Psychol. Rev.*, 1955, **62**, 243-254.
- HEBB, D. O. *A textbook of psychology*. Philadelphia: Saunders, 1958.
- HEBB, D. O. A neuropsychological theory. In S. Koch (Ed.), *Psychology: A study of a science*. Vol. 1. New York: McGraw-Hill, 1959. Pp. 622-643.
- HELD, R. Exposure history as a factor in maintaining stability of perception and coordination. *J. nerv. ment. Dis.*, 1961, **132**, 26-32.
- HILGARD, E. R., & MARQUIS, D. G. *Conditioning and learning*. New York: Appleton-Century, 1940.
- HUBEL, D. H. Single unit activity in striate cortex of unrestrained cats. *J. Physiol.*, 1959, **147**, 226-238.

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HUBEL, D. H., & WIESEL, T. N. Receptive fields, binocular interaction and functional architecture in the cat's visual cortex. *J. Physiol.*, 1962, 160, 106-154.

HUMPHREY, G. The problem of the direction of thought. *Brit. J. Psychol.*, 1940, 30, 183-196.

JUNG, R. Korrelationen von Neuronentätigkeit und Sehen. In R. Jung & H. Kornhuber (Eds.), *The visual system: Neurophysiology and psychophysics*. Berlin: Springer-Verlag, 1961. Pp. 410-434.

KADER, F. J. Target complexity and visibility in stabilized images. *Undergrad. Res. Proj. Psychol.*, 1960, 2, 46-52. (McGill University)

KOHLER, I. *Über Aufbau und Wandlungen der Wahrnehmungswelt*. Vienna: Rohrer, 1951.

KÖHLER, W. *Gestalt psychology*. New York: Liveright, 1929.

KÖHLER, W., & WALLACH, H. Figural after-effects: An investigation of visual processes. *Proc. Amer. Phil. Soc.*, 1944, 88, 269-357.

KRAUSKOPF, J., & RIGGS, L. A. Interocular transfer in the disappearance of stabilized images. *Amer. J. Psychol.*, 1959, 72, 248-252.

LASHLEY, K. S. The problem of cerebral organization in vision. *Biol. Symp.*, 1942, 7, 301-322.

LASHLEY, K. S. Cerebral organization and behavior. *Res. Publ. Ass. Nerv. Ment. Dis.*, 1958, 36, 1-14.

LAWRENCE, D. H. Acquired distinctiveness of cues: II. *J. exp. Psychol.*, 1950, 40, 175-188.

LEEPER, R. W. A study of a neglected portion of the field of learning—the development of sensory organization. *J. genet. Psychol.*, 1935, 46, 41-75.

LINDSLEY, D. B. Emotion. In S. S. Stevens (Ed.), *Handbook of experimental psychology*. New York: Wiley, 1951. Pp. 473-516.

LONDON, I. D. A Russian report on the postoperative newly seeing. *Amer. J. Psychol.*, 1960, 73, 478-482.

LORENTE DE NÓ, R. Analysis of the activity of the chains of internuncial neurons. *J. Neurophysiol.*, 1938, 1, 207-244.

MARSHALL, W. H., & TALBOT, S. A. Recent evidence for neural mechanisms in vision leading to a general theory of sensory acuity. *Biol. Symp.*, 1942, 7, 117-164.

MILLER, G. A., GALANTER, E., & PRIBRAM, K. H. *Plans and the structure of behavior*. New York: Holt, 1960.

MILNER, P. M. The cell assembly: Mark II. *Psychol. Rev.*, 1957, 64, 242-252.

MORUZZI, G., & MAGOUN, H. W. Brain stem reticular formation and activation of the EEG. *EEG clin. Neurophysiol.*, 1949, 1, 455-473.

OSGOOD, C. E. *Method and theory in experimental psychology*. New York: Oxford Univer. Press, 1953.

PRITCHARD, R. M. Stabilized images on the retina. *Scient. American*, 1961, 204(6), 72-78.

PRITCHARD, R. M., HERON, W., & HEBB, D. O. Visual perception approached by the method of stabilized images. *Canad. J. Psychol.*, 1960, 14, 67-77.

REPLOGLE, ANNE. The effect of similarity on the behavior of perceived figures in a stabilized retinal image. Paper read at Eastern Psychological Association, Atlantic City, April 1962.

RIESEN, A. H. The development of visual perception in man and chimpanzee. *Science*, 1947, 106, 107-108.

RIESEN, A. H. Stimulation as a requirement for growth and function in behavioral development. In D. W. Fiske & S. R. Maddi (Eds.), *Functions of varied experience*. Homewood, Ill.: Dorsey Press, 1961. Pp. 57-80.

SENDEN, M. v. *Raum- und Gestaltauffassung bei operierten Blindgeborenen vor und nach der Operation*. Leipzig: Barth, 1932.

SENDEN, M. v. *Space and sight*. London: Methuen, 1960.

SNYDER, F. W., & PRONKO, N. N. *Vision with spatial inversion*. Wichita, Kans.: McCormich-Armstrong, 1952.

SOLLEY, C. M., & MURPHY, G. *Development of the perceptual world*. New York: Basic Books, 1960.

STRATTON, G. Vision without inversion of the retinal image. *Psychol. Rev.*, 1897, 4, 341-360, 463-481.

TEES, R. C. The role of field effects in visual perception. *Undergrad. Res. Proj. Psychol.*, 1961, 3, 87-96. (McGill University)

TEUBER, H. L. Perception. In J. Field, H. W. Magoun, & V. E. Hall (Eds.), *Handbook of physiology: Neurophysiology*. Vol. 3. Washington, D. C.: American Physiological Society, 1960. Pp. 1595-1668.

TEUBER, H. L., BATTERSBY, W. S., & BENDER, M. B. *Visual field defects after penetrating wounds of the brain*. Cambridge: Harvard Univer. Press, 1960.

THOMPSON, W. R., & SCHAEFER, T. Early environmental stimulation. In D. W. Fiske & S. R. Maddi (Eds.), *Functions of varied experience*. Homewood, Ill.: Dorsey Press, 1961. Pp. 81-105.

ZIMMERMANN, R. R. Analysis of discrimination learning capacities in the infant rhesus monkey. *J. comp. physiol. Psychol.*, 1961, 54, 1-10.

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