3. Field Theory and Equipotentiality

The last chapter examined the proposition that perception is a unitary process, in all its aspects. We can turn now to an investigation of the two theories, Kohler's and Lashley's, that have resulted from that idea. In effect, this will be a critique of the theory of perception, since Kohler and Lashley are the only writers who both recognize the real problem of the neural mechanisms of perceptual integration and attempt an adequate solution. Others have avoided the crucial difficulty of specifying the way in which perceptual generalization occurs. Either like Pavlov (1928, 1932) they have not seen the difficulty at all, or like Hull (1943) have put it to one side for solution later. Hull has discussed the problem, but only to show that it is not necessarily insoluble for his theory of learning (Hull, 1945). For the present, Kohler and Lashley are the only ones who have attempted to say where and how perceptual generalization takes place.

For reasons already mentioned, however, the line of thought that they have chosen may be a blind alley, and it must be shown that there are other possible solutions. Until someone challenges the theory of innate stimulus equivalence and equipotentiality, and of neural fields and gradients as the only mechanism of integration, any theory with neural connections in its ancestry is damned before it is born. Being gravid, and so disapproving of infant damnation, let me look for another gospel.
CLARIFICATION OF TERMS AND ISSUES

As elsewhere, the issues here have sometimes been lost sight of in the dust of a prolonged battle. There is one main question whether recognition, or a selective discriminatory response, requires the excitation of specific neural cells or not.

The doctrine that it does not, we can refer to as *equipotentiality*. Kohler does not use the term, but his position is the same as Lashley's on this point, and a single term will do.

Explanations of the supposed equipotentiality differ greatly in some respects but agree in their stress on patterning in the central nervous system as against locus of excitation, so we can again use a single term, *configuration theory*, to designate Kohler's (1940, Kohler and Wallach, 1944) explanation by electrical fields in the cortex and Lashley's (1942a) alternative idea of irradiating waves of excitation and their interference patterns.

Next, *sensory equipotentiality* can be coined for Lashley's "equivalence of stimuli," which is ambiguous. One may want to refer to a limited degree of equipotentiality, in afferent structures, without accepting the whole theory of equipotentiality in central action, and this term can be used for the purpose.

"Equivalence of stimuli" has a double reference. It may mean only (1) that different stimuli can arouse the same response. This is an observed fact of behavior, whatever one's interpretation of the fact. But Lashley has also used the term to mean (2) that it does not matter what sensory cells are excited in order to get a certain response, and this is interpretation. The fact that two different patterns have the same effect may be due to eyemovement, or to multiple learning processes (the position adopted here), or it may be because similar patterns of excitation have the same ultimate motor effect, innately and necessarily, whether the receptors excited are the same or not. Let us then separate fact from interpretation, and let us refer (1) to the behavioral evidence in speaking of the *equivalence of stimuli*, and (2) to a particular interpretation in speaking of *sensory equipotentiality*.

Finally, the idea of equipotentiality itself should be made more explicit. "Potentiality" literally implies only that different
cells are able to acquire the same function in behavior. Anyone must agree that this can happen, with separate learning processes. It is the basic idea of conditioning. But Lashley has given the word another meaning, that all cells in a system necessarily do acquire the same function, when excited in a given pattern, so that separate learning processes are not necessary for habits involving different stimuli or different parts of a cortical field.

Suppose for example that a habit is set up in this way. The stimulus is a circle, let us say, that falls always on the same retinal cells in the lower right quadrant, the response is a movement of the left hand. The stimulation then is confined to one half of the visual area in the right hemisphere, and the response is determined by motor systems that are also in the right hemisphere. Equipotentiality implies (1) that any other retinal cells, excited in a circular pattern, will elicit the same response—whether left or right hand, (2) that the right hemisphere may be extirpated, and the left will be found then to have “learned” whatever the right did, and (3) that this transfer of learning from one set of cells, primarily excited, to other sets does not depend on an earlier experience that set up connections between them. The idea is that the transfer is an innate property of the central nervous system, so that it is not possible for a particular habit to depend on any particular neural cell within an anatomical system, in any circumstances. If a whole system is extirpated, habits will be lost (as when both visual areas are removed), but it is not possible for a habit to be dependent on one half of an anatomical system only (for the habit to depend on one visual area alone, or on any part of it).

This is obviously a rather extreme position. An alternative hypothesis to be proposed later recognizes a limited field action in the sensory projection areas, and something like a limited equipotentiality between cells that are in functional parallel in physiological systems (this will be clear later on), but in the main it derives transfer of response from earlier-established habits and specific connections.

The issue is thus defined. We must recognize that Kohler and Lashley have been completely successful in their original attack on stimulus-response theory such as Watson’s. No one
now believes that retinal cells acquire direct connections with muscle cells, as in a child’s learning to say “cat” when he sees a cat. The elimination of such oversimplified forms of switchboard theory was a fundamental contribution. One need only glance at the Journal of Neurophysiology to see that there are gradients of neural activity, there is some mutual influence of neighboring parts as an excitation is conducted from the retina to area 17. Electrophysiology has fully confirmed the value of such ideas, and they must enter into psychological theory.

But in fact, they have been generally accepted already, so this is no longer an issue. Pavlov’s irradiation and concentration is a field action. Hull and Spence both make use of the notion of gradients. The question now is whether gradients and fields are the only mechanism of a selective neural action or whether they are combined with an equally important mechanism of connections and specialized conduction paths.

In short, configuration theory represents two things. It has, been, first, a necessary corrective to earlier ideas, such as Titchener’s and Watson’s. This positive contribution must be recognized as an important step forward. But there is also an over-reaction from punctate sensory elements and sensorimotor connections that is about as extreme as these older ideas. As a theory, therefore, configurationism is just as vulnerable as structuralism and the early behaviorism were.

Now let us see what the evidence is by which we can accept or reject field theory and equipotentiality. Some of the evidence that has been used to support these ideas actually is not relevant to the question, and such evidence can be discussed first, leaving the more relevant evidence for later consideration.

THE PERCEPTION OF BRIGHTNESS, SIZE, PITCH

In the literature, the perception of relative intensity has been used almost entirely by writers on configuration theory for the embarrassment of learning theory. Actually, it is no easier for configuration theory to explain, and so is not really relevant to the debate between the two. It has been thought to be crucial, however, and so must be considered here. Man or animal tends to perceive relative rather than absolute intensity, extent, or fre-
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quency. One can readily train an animal to choose the larger of two surfaces; it is extraordinarily hard to get him to choose a particular size, except when differences are very great. These facts are psychologically important, and they provided a decisive answer to learning theory in its older forms, but we know too little of the physiology of the relative perception to use it as evidence for or against any currently maintained psychological theory.

The actual experimental evidence is as follows, taking the discrimination of brightness as an example.

A rat is trained to go toward a light of intensity 1 and to keep away from intensity 2 (i.e., twice as strong as intensity 1). He is then presented with intensities 2 and 4, and chooses (if he chooses at all) intensity 2—which he was taught to avoid (In the test, discrimination may break down, choices being made at random. This happens perhaps less than half the time, but a consistent choice of an absolute stimulus value never occurs in the rat in these circumstances, and the relative value is what the animal discriminates, when he does discriminate.)

Or, if the rat is taught in the first place to avoid an intensity 1 and to choose intensity 2, in the test with 2 and 4 he will reject the stimulus he was trained to respond to in favor of a new one, intensity 4. This happens even when the animal has been reared in total darkness and has never in his life seen that stimulus before. This is, so far as one can see, still a conclusive answer to any theory of behavior that deals in connections from specific rods and cones to specific muscle fibers (Hebb, 1937b).

But the perception of brightness is commonly cited in a broader context, as supporting configuration theory in general against "learning" theory in general. By implication, it is treated as support for the idea that discrimination is not dependent on the excitation of any particular cells, at any neural level. This does not follow. The truth is that no one has produced any good explanation of the relative perception, so it does not support one camp against the other, and the truth is, also, that

* This author, however, seems to have supposed that such experiments rule out all possibility that connections are the basis of learning. The hypothesis presented in the following section is enough to show that the conclusion was unwarranted.
we know little or nothing about the locus of the cells whose activity mediates the perception "brighter," or "smaller," or "higher" (in pitch), so the facts neither deny nor support the hypothesis of equipotentiality.

We know, with reasonable certainty, that the training process does not form a connection all the way from receptors to effectors, but it is still possible to suppose that connections may be formed at some intermediate levels. I shall outline here a possible hypothesis, mainly to show that we must know more of the physiology of sensation before brightness perception (for example) can be made an argument for or against any existent theory.

AN HYPOTHESIS OF BRIGHTNESS PERCEPTION

Marshall and Talbot's (1942) treatment of visual acuity makes it possible to assume that the perception "brighter" depends on the activity of particular cortical cells, but not of particular receptor cells. We take for granted that intensity 2 excites a larger population of first-order (retinal) neurons than intensity 1 and with a higher rate of firing. Intensity 4 excites a still larger population. The fact that 2 and 4 may elicit the same selective response is an argument for a sensory equipotentiality, but the equipotentiality may disappear farther on in the system.

Marshall and Talbot point out that the whole visual system, from receptors to the several layers of the cerebral cortex, must act to damp strong stimulations, amplify weak ones. See what this means for the behavioral problem. At each synaptic stage in transmission the damping, or amplification, increases. A level $X$ may be reached in the system where the degree of activity is constant for an adapted retinal area, with a brightness in the middle range. The amount of activity at $X$, then, will be the same whether the eye is adapted to intensity of 1, 2, or 4.

This of course is true only in adaptation. The system must remain very sensitive to changes of stimulus intensity. Therefore, when the eye after being adapted to 1 is focused on 2, the level of activity at $X$ goes up, say, from $n$ impulses per second to $1.5n$. The same would happen when the eye adapted to 2
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is focused on 4 In one part of the system, the same central neurons might thus be active in the perception "brighter," regardless of absolute stimulus values (In other parts, the cells that are aroused would still correspond partly to the absolute intensity, and this helps to account for the fact that absolute values can be detected, though the tendency is toward the relative discrimination.)

My point is not that this has explained the relative perception. I do not suppose that the problem is as simple as this has made it sound. The point is that the known facts are not inconsistent with the idea that the perception "brighter" involves an activity in specific neural cells at some point in the system, and thus that a perception of relative values is not incompatible with some specific neural connections. Something else, a "sensory equipotentiality" akin to neural gradients and field action, is certainly involved; but the establishment of new synaptic connections is not ruled out as the basis of an animal's learning to respond to the brighter of two surfaces.

In the next two chapters some neurophysiological ideas are proposed about visual perception. They could be elaborated speculatively to deal with the perception of brightness and size, though I have not done so. The perception of size, brightness, and pitch should be written down for the present as not yet accounted for, by any theory. Before they are, we shall have to know much more about the physiology of sensation.

THE PERCEPTION OF PATTERNS

In the perception of patterns we reach surer ground, having more of the relevant physiological information available and being better able to see what bearing the facts have on the theory of equipotentiality.

An animal that has learned to look for food behind a small square and not behind a small circle will, without further training, choose a large square and reject a large circle. Small and large squares are "equivalent stimuli." If each is perceived with a single fixation, it is impossible for the total pattern in each case to excite the same set of retinal cells. The current treatment of perception disregards eyemovement, the inference has
been drawn, therefore, that the locus of the cells stimulated by
the square is unimportant. Only the pattern of stimulation
matters.

But we have seen, in the last chapter, that eyemovement can-
not be disregarded. A rat trained to jump toward a square does
not respond only to the total figure, but at times clearly responds
to parts of it, as separate entities. Eyemovement has never
been controlled in animal studies of perception. If the rat fix-
ates parts of the figure in succession the result is to superimpose
its parts, one after another, on the same retinal cells that were
involved in the original training. The locus of excitation may
then be essential. Evidently the equivalence of similar patterns
different in size does not in itself provide support for the theory
of equipotentiality.

For the evidence to become crucial, two conditions must be
fulfilled. The equivalent stimuli must be shown to be con-
ducted to different parts of the central nervous system, and
there must have been no previous experience that could have
set up connections between those parts before the experiment
began.

One of these conditions has been met by Levine's (1945a,
1945b) experiment with pigeons. The optic chiasm in birds is
completely crossed, so one eye conducts to one optic lobe, the
other to the other. Levine achieved the truly surprising result
of finding transfer for patterns in the lower half of the visual
field and not in the upper half. For configuration theory, the
best that can be said with these results is that there is a limited
equipotentiality, between parts of the optic lobes (from the op-
posite point of view, these results definitely refute the theory
of a general equipotentiality in vision).

Levine's experiment did not, however, control earlier experi-
ence in his birds, and it remains possible that even the limited
equipotentiality suggested by his results is illusory. It is pos-
sible that patterns in the lower half of the bird's visual field—
as when the bird is foraging on the ground, or alighting on a
perch that extends into the field of vision of both eyes—had
established, by perceptual learning before the experiment began,
tericoconnections between the two optic lobes which might later
be a basis of transfer. This would not require that the bird
have learned beforehand the actual patterns used in the experiment, only that the parts of those patterns (or “perceptual elements” see Chapter 5) have been learned.* That this possibility is not far-fetched will be seen as we consider next the instantaneous recognition of patterns by man, where it is evident that the perception of one or two parts of a figure may be the clue to recognizing the whole.

Human beings readily recognize certain patterns in the tachistoscope. The time of exposure of the pattern is of the order of one-fifth of a second. The subject cannot fixate the separate parts of the pattern one after another, and cannot adjust his eyes in advance so as to make it fall on any particular retinal cells. This might be interpreted as equipotentiality, but there are facts that make that interpretation practically impossible.

I propose that the human capacity for recognizing patterns without eyemovement is possible only as the result of an intensive and prolonged visual training that goes on from the moment of birth, during every moment that the eyes are open, with an increase in skill evident over a period of 12 to 16 years at least. The evidence supporting this proposition is found (1) in the errors made in tachistoscopic recognition, (2) in the way in which reading skill develops, and (3) in the rate and course of learning by the congenitally blind after operation.

1. The recognition of patterns momentarily perceived, as in the tachistoscope, is extremely defective except with simple and conventional patterns, or very simple combinations of them. The process is exactly that named by Woodworth (1938) “schema with correction.” This means that the pattern is perceived, first, as a familiar one, and then with something missing or something added. The something, also, is familiar; so the total perception is a mélange of the habitual.

* In particular, the slope of lines as they extend into the fields of both eyes simultaneously. This explanation by a learning process is not, however, the only alternative to equipotentiality. Another possibility is that part of each optic lobe, receiving impulses from the upper hemiretina only, has a point-to-point projection to the other lobe. Transfer would thus be innately provided for. The explanation by learning, however, should be examined first, by repeating Levine’s experiment with pigeons reared in darkness and kept in darkness except when they are actually being trained or tested.
The subject's reports are such as "a triangle with the top cut off" or "a square with a crooked bottom." It is thus clear that the subject is not only responding to the diagram as a whole, he perceives its parts as separate entities, even though presentation is so brief. Errors are prominent, and such as to show that all the subject really perceives—and then only with rough accuracy—is the slope of a few lines and their direction and distance from one another. A drawing or a report of what is seen tachistoscopically is not unlike a paleontologist's reconstruction of early man from a tooth and a rib. There is a clear effect of earlier experience, filling in gaps in the actual perception, so that the end result is either something familiar or a combination of familiar things—a reconstruction on the basis of experience.

All that is needed to account for the recognition of pattern without special adjustment of the eyes, to the extent that such recognition really occurs, is ability to recognize roughly (1) the slope of line, (2) degree of separation of points, in any retinal projection. These in combination cover intersection of lines, sharp inflections of line (or corners), and curvature of line (by the variation of slope at different points, and the absence of sharp inflection). What I am proposing, then, is that, during the continuous, intensive, and prolonged visual training of infancy and childhood, we learn to recognize the direction of line and the distance between points, separately for each grossly separate part of the visual field.*

* Here, as elsewhere, I have chosen to be more definite than is actually required by the theory to be developed, this is done with the conviction (1) that vagueness is not desirable as such, in psychological theory, and (2) that the more definite assumption may turn out to be correct, and at any rate should be explored.

If it is not correct, however, the later theorizing is unaffected. An alternative possibility to be considered is that some form of Lashley's interference-pattern theory may hold in modified form. Lashley has used the theory to explain perception of a square or triangle as a single unified process, and it will be seen in the later part of this chapter that the theory in this form cannot be maintained, but it might be feasible to apply it to single lines instead. Then the stimulation from a line of a certain slope in one part of the visual field would be materially equivalent to the stimulation of a similar line in another part of the field. I know of no evidence that opposes this, but no evidence for it either.

Psychologically, such an explanation might be more plausible, what it
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I have already admitted that such ideas sound unlikely. The perception of simple objects seems so simple and direct, so obviously immediate, that it cannot be complex and the result of a long-drawn-out learning process. But it was shown in Chapter 2 that the learning process is necessary, that the end result is complex, whatever it may seem.

It should be noted that these ideas do not mean that each separate row of single cells in the retina must be separately conditioned for the perception of horizontal line (e.g.) in different retinal projections. Marshall and Talbot's discussion of visual acuity implies a significant overlap of excitation, at the level of the primary visual cortex, from lines that have distinct retinal projections. It is not clear how great this spread is, in terms of retinal angle, but it may be of the order of 2° in central vision and considerably greater at the periphery. Accordingly, the number of separate visual habits necessary to ensure the recognition of horizontal line in any retinal projection might be, perhaps, not greater than 10 or 15. Lines of slightly different slope, likewise, would not have to be learned separately (remember that accuracy in discriminating slope tachistoscopically is not great). The number of separate perceptual habits needed to account for tachistoscopic vision, consequently, is by no means infinite.

Now consider how intensive and prolonged the human visual training is. As we shall see later, every single movement of the eyes, when exposed (as they always are) to an unevenly lighted field, produces a number of excitations corresponding exactly to the excitation from a number of lines all parallel to the direction of eyemovement. The eyes are constantly moving when the subject is awake. Every waking moment then provides the necessary conditions for reinforcing the perception of lines, in every part of the retina.

Would postulate is that parallel "ridges" of activity in area 17 would tend to excite the same, diffusely arranged, cells in 18. My neural schematizing, in the following two chapters, would then go on as before, but without the necessity of supposing that different parts of the retina must be separately conditioned to produce recognition of the slope of line in any part of the field.

The question is, I believe, open to experimental test, and will be investigated.
The Perception of Patterns

We have already seen that it takes months for the first direct apprehension of a figure such as a plain, well-marked triangle to be established. The normal human infant, apparently, reaches this stage quite early in life, but his further training continues every moment that his eyes are open, and must extend his capacity for prompt recognition of patterns falling outside the macula.

2. Speed of reading increases up to the age of 12 to 16 years, or perhaps even later. This speed, depending on the instantaneous recognition of larger and larger blocks of letters, may be regarded as a function of the training of peripheral vision, it implies that learning to recognize a word immediately (as a single Gestalt) in direct central vision does not mean recognizing it immediately at any angle of regard, though this is what the theory of equipotentiality requires.

It might be argued that the theory does not require this, because of differences of acuity between central and peripheral vision. Peripheral recognition of a word may depend, for example, more on its general outline and less on particular letters. It might be argued, that is, that an increased speed of reading depends on learning to use different cues from those used in central vision. This may well be true, but such an argument abandons the fundamental thesis of equipotentiality, namely, that the same properties of form are perceived whatever the retinal projection, provided acuity is sufficient. Above all, such an argument would imply that peripheral recognition and central recognition depend on separate visual habits, which is the argument of this chapter.

More direct evidence on the point is available, however, than when the above considerations were first formulated. These considerations suggested the need of a re-examination of some features of tachistoscopic perception, and this has been begun by M Mishkin and D Forgays. Their experiments show directly that reading does not train all parts of the retina in the same way, even when acuity does not enter the picture. The work will be reported in detail elsewhere. In summary, the following results have been obtained. Fluent readers of English, used to reading from left to right, are able to recognize words to the right of the fixation point between two and three times.
as well as words to the left of fixation. Readers of Jewish (in which the words run from right to left) recognize more words to the left of fixation. These results can be obtained when the subject does not know whether an English or a Jewish word is coming next, or whether it will fall to the right or the left of fixation—that is, with a random order of left-right, English-Jewish presentations. The subjects that have been studied were not very fluent in Jewish, apparently, and the left-right difference, with Jewish words, was not statistically significant ($P = 0.15$); but it is certain that whatever factors make the English word better recognized to the right of fixation does not affect recognition of Jewish words. Either there is no difference, or the Jewish word is recognized better to the left.

This shows directly that there is not an equipotentiality between the left and the right hemiretina, or left and right occipital cortex, in man. A learned response can be more readily elicited from one part of the retina than another, which provides a good deal of support for the assumption, made in these chapters, that a separate learning process is necessary for the elicitation of the same response by stimulation of a separate region in a receptor surface.

3. Finally, there are Senden's explicit reports of the way pattern vision develops after the congenitally blind are operated on (see Chapter 2). The patient, despite weeks of practice, must still count corners to distinguish a square from a triangle here there is no unimportance of the locus of stimulation, no evidence of equipotentiality. When the patient first gets to the point of naming an object promptly, the recognition is destroyed by putting the object into a new setting or by changing its color. The significant fact is that the characteristic normal generalization shows up later, after a prolonged and arduous training process. The evidence is clearly that the apparent equipotentiality is the sequel and product of learning, the perception of pattern is specific and limited at first, generalized only with further practice; and in these cases there is no support whatever for the idea that when a pattern is recognized at one angle of regard it will be at once recognized in any other.
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We can turn next to the clinical and anatomical evidence that is related to the question of equipotentiality in perception.

The conclusions reached already are supported by the perceptual completions in hemianopia, described by Fuchs (1920) and Lashley (1941). When one occipital pole of the brain has been destroyed by injury, or temporarily loses its function in a migraine attack, the patient becomes blind in half the visual field. If a simple symmetrical object such as a solid white square or a billiard ball is fixated in the midline, the patient nevertheless reports that he sees all of it. That he does not, but instead completes in his perception what he really sees only half of, is shown by another fact. If half the object is presented instead, in such a way that the missing half would have fallen in the blind side, the patient still "sees" the whole object.

What is the basis of this completion? Koffka (1935) treats it as a further evidence of Gestalt principles—an instance of the operation of field forces. This, however, is precisely what it cannot be.

In two of Fuchs' cases at least (Fuchs, 1920, pp 424, 436) the gunshot wound that produced hemianopia must have destroyed one area 17. The bullet entered the skull in the midline at the occipital pole, and could hardly have destroyed the optic radiations on one side without also destroying the visual cortex on that side. But area 17 is the cortical tissue in which a dynamic completion, due to field forces, would have to occur, there is no isomorphism in the excitation conducted beyond 17—conduction is diffuse, and the topological relationship to the visual stimulus that is retained from the retina to area 17 is lost beyond it, so the field-force idea is applicable only at this level. The completion, then, is not the result of a field process.

In the case of migraine scotoma described by Lashley, the symptoms indicate that one visual area was nonfunctional during an attack. But this again is the tissue in which interference patterns would have to originate, to produce the perception of a complete object fixated in the center. According to the theory
of interference patterns (Lashley, 1942a) the perception of a semicircle lying immediately to the right of the fixation point depends on the particular interference pattern that is set up by that diagram in the left visual cortex. A whole circle, fixated in the middle, sets up two such interference patterns, one from each visual area. In the hemianopic, only one can occur, why, then, should the patient see a whole circle? The conclusion is

![Diagram](https://example.com/diagram.png)

**Figure 4** Diagramming roughly the changes in cortical projection of a square when the fixation point only is changed based on the data of Polyak, 1941, and his figure 100, for a square subtending a visual angle of 18° 20' (the size of the "central area" of the retina). 1, fixation on the upper right corner of the square, which thus falls in the lower left visual field and produces an excitation in the upper right cortex only. 2, fixation on the lower right corner. 3, bilateral projection with fixation on the center of the square. 4, bilateral, fixation on the midpoint of the top line of the square. 5, fixation on midpoint of bottom line. \( F \), projection of fixation point, \( VM \), vertical meridian.

inevitable that the "completion" occurs in some other, undamaged, area of the cortex—and that it cannot, consequently, be a field process.

Next, consider some of the anatomical properties of the visual cortex in which the field process must occur.

There is a point-to-point correspondence between the retina and the visual cortex, or area 17 (Lashley, 1934, Polyak, 1941). Thus there is an isomorphism of the visually stimulating surface with the resulting excitation in area 17, and both the theories that we are examining assume that this fact accounts for the perception of a square, for example, as a distinctive whole. But there are definite limits to what isomorphism can account for in perception.

Figure 4 represents roughly the patterns of cortical excitation.
Clinical and Anatomical Evidence

that are aroused by looking at different parts of the same pattern, a square. These show how hard it is to suppose that the perception of a square, as distinct from other quadrilaterals, is determined by the shape of the isomorphic excitation in area 17, since that shape varies so much.

(There are undoubtedly major errors in the figure, which is based on a diagram by Polyak [1941, figure 100]. Polyak made no attempt to represent corresponding retinal and cortical points in detail, the necessary evidence for doing so is actually not in existence. Polyak's diagram also deviates in some respects from his text, which is presumably the more accurate, his figure 100, for example, shows the "central fovea" half as wide as the "central area," whereas his text, pp. 230-231, suggests that the difference in size is greater. Figure 4 follows his diagram, not the text. Finally, the human visual area cannot be represented accurately as a flat sheet.)

Figure 4 is only roughly accurate, it seems certain, however, that the distortion, in the pattern of cortical excitation, that occurs when fixation is changed from one to another part of the figure is at least as great as that diagrammed. If the data of Marshall and Talbot (1942, p. 134) had been used instead, the discrepancies would have been greater. Furthermore, a difference in size (in terms of retinal angle) between two squares would produce even greater changes in the shape of the cortical excitation, although squares of different size are also supposed to derive their common identity from the distribution of excitation in area 17.

The reproduction of retinal patterns in area 17 is topological, not topographical, and if identity were completely determined by the shape of the cortical excitation, regardless of what the fixation point may be, the perceived properties of a square with shifting gaze or at different distances should change markedly—remaining, it is true, a quadrilateral (topological reproduction provides for this), but not retaining the same proportions. That is, errors should occur in the perception of quadrilaterals, which do not occur, or in the perception of triangles, and so on. A circle in one angle of regard should be confused with an oval in another angle. One might postulate a differential cell density to correct the distortion of topographical relations which I
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have diagramed, but there is no support from histological study of area 17 to support this *ad hoc* assumption, and I can find no way of doing it for one pattern which would not mean an even greater distortion for another.

However, another way of dealing with this obstacle for field theory has been proposed by Kohler and Wallach (1944), who suggest that a differential “satiation” or fatigue would correct the anatomical distortion of the cortical retina. This proposal will be considered with other aspects of their theory of figural after-effects.

**THE THEORY OF FIGURAL AFTER-EFFECTS**

Kohler and Wallach (1944) have proposed a new treatment of perception which is in some respects very attractive and deals with a certain set of facts very efficiently. On the other hand, I believe that the theory is inconsistent with some of the facts reported by Kohler and Wallach themselves, and that it cannot be considered to have disposed of the anatomical difficulty that has just been discussed.

Following some original experiments by Gibson, Kohler and Wallach have studied certain peculiar fatigue effects resulting from prolonged inspection of visual objects. The method of experiment is this: The subject keeps his eyes fixed, for some two to three minutes, on a single “fixation point” in a large surface on which a diagram is also presented. The fixation point is determined by the experimenter so that the “inspection figure” stimulates a particular part of the retina. Then, after the inspection period, the subject looks at a second surface, again with a fixation point determined by the experimenter, which is placed so that a “test figure” on this second surface also stimulates a particular part of the retina. Thus in figure 5 are represented both an inspection and a test figure, superimposed so that one can see the relationship between the retinal projection of each. The fixation point is shown by a small x, the inspection figure (at which the subject stared first) is the large rectangle marked I, and the test figure is the set of four small squares each marked T. These four squares are so placed that two fall in a region of greater fatigue or “satiation,” near the boundary of the
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rectangle, and two fall farther away, allowing one to discover the changes that occur in the neighborhood of the inspection figure, where satiation has occurred.

When a test figure is projected on the retina near the place at which the inspection figure was projected, several effects can be observed in the test figure. These effects include a color effect (fading), a depth effect (apparent increase of distance from the observer), and displacement effects (apparent change in direction of line, in position, or in size). It is the displacement effects with which we are concerned here. Kohler and Wallach propose, first, that the basis of perception is the occurrence of electric currents aroused by the visual excitation in the cortex of area 17, secondly, that the flow of these currents in the neighborhood of the excitation increases the resistance of the tissue through which they flow. This increase of resistance is satiation, and the changes it causes in current flow account for displacement effects. This is on the assumption that perceived distance between two objects varies with the degree to which corresponding cortical objects [i.e., the excitations in area 17] are interrelated by their figure currents (Kohler and Wallach, 1944, p 334). Just what this interrelation is and how it operates is not specified, but its degree decreases when the tissue between two areas of excitation has an increased resistance. The two corresponding objects in the visual field then look farther apart. It is also important to observe that Kohler and Wallach state explicitly (p 337) that apparent displacements are not dependent on actual changes in the locus of the excitations in area 17, only on an increased resistance to current flow in the tissue between them.

These assumptions account economically for some of the phe-
nomena described by Kohler and Wallach. Other phenomena, however, seem inconsistent with the theory. Consider first the perceived size of a figure within an area of satiation, including the special case where the test figure coincides with the inspection figure. According to the theory, the perceived size of a square or circle should increase with increased satiation, since the greater resistance to current flow between opposite sides should produce an overestimation of the distance between them. In fact, the opposite is frequently reported by Kohler and Wallach when a test figure, such as square or circle, coincides exactly with the locus of the previously seen inspection figure, its apparent size diminishes. Again, in figure 5, the excitations produced by the two small test squares on the right, near the boundary of the inspection rectangle, are separated by tissue that is more satiated than that separating excitations from the two small squares on the left—yet the two on the right appear closer together than those on the left. This difficulty for the theory is found in the data obtained by Kohler and Wallach with their figures 1, 2, 3, 4, 5, 7, 10, and so on.

The problem has been recognized by the authors and is briefly discussed at the end of their monograph (pp 351, 356). They suggest two possible solutions: one is that apparent size may be affected by the presence of a large area of satiation surrounding the figure, as well as by the degree of satiation within it (p 351), another is the possibility that an immediate self-satiation, by the test figure itself, may have a greater effect than immediate self-satiation plus a pre-existent satiation (p 356). If I understand these explanations, they may undermine all the preceding argument, much as “afferent neural interaction” undermines Hull’s theory by removing the definiteness of inference from his other postulates (p 15). Certainly these two influences, of immediate self-satiation and of a surrounding area of satiation, would have to be reckoned with in the production of other phenomena, else their existence would become an ad hoc postulate to be used only where difficulty appears. Thus of course may mean only that the theory needs to be worked out in more detail, as Kohler and Wallach themselves have pointed out.
Next, a difficulty of a somewhat different kind, related to the anatomical distortions of the cortical retina (the difficulty for configuration theory that was discussed in the preceding section).

To deal with the anatomical fact that objects equally spaced in the visual field are not equally spaced for the cortical retina, the key assumption is made by Kohler and Wallach that a permanent satiation in the peripheral cortical retina would be greater than in the cortical fovea, and, accordingly, that anatomical distortion such as has been diagrammed in figure 4 would be corrected functionally. The greater peripheral satiation is thought to occur as follows: Contours equally spaced on the retina would not be equally spaced in area 17, because the peripheral retina has a smaller cortical representation. So an equal distribution of contours in the visual field, and consequently on the retina, would result in a bunching of contours in the peripheral part of the cortical retina, and a wide spacing of contours in the cortical macula. On this argument, the closer spacing of contours would produce a higher level of satiation in peripheral vision, which would compensate for the fact that a given retinal angle here is represented by a much shorter cortical distance than the same angle in central vision (Kohler and Wallach, 1944, p. 345). Although the cortical distance is shorter, resistance is higher too; so perceived distance remains the same.

But all this has overlooked an essential fact. Acuity in central vision is very much higher than in peripheral vision, and so there is no basis for assuming that the central cortical retina would be the site of a smaller number of excitations per centimeter. Kohler and Wallach’s argument assumes, essentially, that all contours in the visual field are supraliminal for the periphery, given this assumption, their argument stands, but we know actually that the assumption cannot be made, that many contours are supraliminal for central vision and subliminal for peripheral vision. These contours, subliminal at the periphery, would fill in the gaps between the more widely spaced excitations in central vision that originate from contours which are supraliminal for the whole visual system, peripheral as well as central. There is no evidence permitting one to be certain that the frequency of contours that are subliminal for the peripheral field is such
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as to compensate exactly for the greater spacing of other contours in the macular area of the visual cortex, but what evidence we have implies this, since differences of acuity appear to be closely related to the ratio of centimeters of cortex per degree of visual angle (Marshall and Talbot, 1942).

The theory of figural after-effects, therefore, has not avoided the difficulties for configuration theory that are to be found in the anatomy of the visual cortex.

CONCLUSIONS

The conclusions that have been arrived at concerning field theory and equipotentiality, in this and the preceding chapter, may be summarized as follows. The examination of these theories of perception was undertaken in the first place because no way could be found of dealing with attention and attitude, in terms of the configurational hypothesis. This still stands as an essential weakness, which by itself would justify a search for some other approach to perception. On closer inspection, however, one also finds that configuration theory has flaws even in the treatment of perception itself, and some of the flaws are serious. It is not possible to reject the theory finally, for an answer may be found for each of the objections that have been raised, and the alternative treatment that is proposed in the two following chapters has its own difficulties and shortcomings— it is in fact because my theorizing about perception is in some respects implausible that I am obliged to emphasize weaknesses in other theories, just as later (in Chapter 8) a rather vague treatment of motivation is bolstered up by trying to show that Hull’s alternative is not as precise as it seems.

The fundamental difficulty with configuration theory, broadly speaking, is that it leaves too little room for the factor of experience. It makes difficulty in seeing how learning can occur (particularly, perceptual learning), and in seeing how autonomous central processes can exist and influence behavior. Association (as distinct from conditioned reflex) theory, on the other hand, may make it possible to adopt a halfway position in which one can take advantage of some of the obvious values both of configurationist and of connectionist theories.
Conclusions

It is important as psychology comes of age to avoid, if possible, the extreme positions that have often been adopted in the discussions of the past. It is consequently relevant here to point out that a large part of the original thesis of the configurationists (the Gestalt group, Lashley, Tolman, et al.) has already been accepted and is fully made use of in current theorizing, and also that the theory to be presented in the following chapters is explicitly designed to deal from the first with the problems of form perception and attention or set—problems whose existence has been most insisted on in recent years by the configurationists—as well as those problems of learning and memory emphasized by learning theorists. Thus the present argument is based at least as much on Gestalt as on learning theory.

The current debate between these two theories cannot be understood on the assumption that they are utterly opposed to each other. It is rather to be seen as like the running battle between the Left and the Right in governmental policy. the Left continually insisting on the inadequacy of present theory, working to broaden and elaborate the conceptual armament of the experimenter, the Right insisting always on the dangers of being too free with changes that may open the doors to animism, on the importance of demonstrable evidence and intelligible definitions, and on the necessity of fully exploring the ideas we have now before rejecting them as insufficient. But in this conflict, as in the political one, it will be found over any great period of time that the Right (in current psychology, learning theory) has adopted again and again the ideas originally advocated by the Left (currently configurationism), once they are properly aged and found not to have spoiled. Thus modern learning theory is "sophisticated," it deals freely in ideas that were once anathema to it—ideas of gradients, generalizations, interaction among afferent processes as well as the classical afferent-efferent action, and so on. Obviously both Left and Right are needed for the development of psychology, though it may be easy for the configurationist to forget the value of the systematizing and search for rigor made by his opponent, or for the learning theorist to forget where some of his ideas originally came from.