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The Implications of Experiments on the Perception of Space and Motion

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THE IMPLICATIONS OF EXPERIMENTS ON

THE PERCEPTION OF SPACE AND MOTION

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I INTRODUCTION

Experiments on the perception of space and of motion in space have been carried out by the author since 1954 with the support of the office of Naval Research. The most obvious application of such research is to aviation. Before 1954 the experiments were supported by the Air Force and they go back even further to the Aviation Psychology Program of World War II. Experimental studies using motion pictures were part of that program and were described by the author in one of the volumes that culminated it (Gibson, 1947). The experiments were continued under Air Force sponsorship from 1947 to 1954 but in the latter year a shift was made to the Office of Naval Research since it was more willing to support basic research than was the Air Force.

During this postwar period the author wrote and published The Perception of the Visual World (Gibson, 1950) in which a beginning was made at reformulating the traditional problems of space perception. The psychology of the flier had a deep influence on the book. The importance for the ability to see space of seeing how to get around in space began to be evident.

The essay that follows this introduction is a report to the Office of Naval Research of twenty years of experiments carried out at Cornell University. But they were done in the context of other experiments by other psychologists, and in relation to the conclusions reached in the 19th century about the "cues" for perception. In order to understand the implications of our own studies, some of these other investigations will have to be described also. The focus will be on the new conclusions that can be drawn from our studies.

To an increasing degree the experiments at Cornell have been based on a different theory of stimulus information for vision than is usual, and on an unfamiliar level of optics called ecological optics. Vision is conceived as a perceptual system and not as a channel for sensations or sense-data (Gibson, 1966).

Despite the title, this report will not consist of two parts, one describing experiments on space perception and another on motion perception. In the end these supposedly different kinds of perception have proved to be inseparable. Neither space nor motion is ever perceived as such. These two traditional terms are very useful, and they are important concepts in physics and mathematics, but they are misleading for the psychologist. It will be argued, moreover, that a clear distinction must be made between the motion of an object and the motion of the observer himself. There is no single kind of visual motion perception as the first investigators assumed.

The experiments will be considered under four main headings, first, the evidence for the perception of surface layout, second, the discovery of visual kinesthesis, third, the experiments on the perception of changing surface layout and, fourth, the puzzle of the apprehension of hidden surfaces.

II EVIDENCE FOR THE DIRECT PERCEPTION OF SURFACE LAYOUT

Some thirty years ago, during World War II, psychologists were trying to apply the theory of depth perception to the problems of aviation, especially the problem of how a flier lands an airplane. Pilots were given tests for depth perception, and there was much controversy as to whether depth perception was learned or innate. The same tests are still being given, and the same disagreement still continues.

The theory of depth perception assumes that the third dimension of space is lost in the two-dimensional retinal image. Perception must begin with form perception, the flat patchwork of colors in the visual field. But there are <u>cues</u> or <u>clues</u> for depth which, if they are utilized, will add a third dimension to the flat visual field. A list of the cues for depth is given in most psychology textbooks: linear perspective, apparent size, superposition, light and shade, relative motion, aerial perspective, accommodation (the monocular cues) along with binocular disparity and convergence (the binocular cues). You might suppose that adequate tests could be made of a prospective flier's ability to use these cues, and that experiments could be devised to find out whether or not they were learned.

The trouble was that none of the tests based on the cues for depth predicted the success or failure of a student pilot, and none of the proposals for improving depth perception by training made it any easier to learn to fly. I was one of the psychologists who were deeply puzzled by this fact. The established theory of depth perception did not work. It did not apply to problems where one might expect it to apply. I began to suspect that the traditional list of cues for depth was inadequate. And in the end I came to believe that the whole theory of depth perception was false.

I suggested a new theory in a book on what I called the "visual world" (Gibson, 1950). I considered "the possibility that there is literally no such thing as a perception of space without the perception of a continuous background surface" (p.6). I called this a "ground theory" of space perception to distinguish it from the "air theory" that seemed to underlie the old approach. The basic idea was that the world consisted of a basic surface or set of adjoining surfaces, not of bodies in empty air. The character of the visual world was not given by objects but by the background of the objects. Even the space of the airplane pilot, I said, was determined by the ground and the horizon of the earth not by the air through which he flies. The notion of a space of three dimensions with three axes for Cartesian coordinates was a great convenience for mathemetics, I suggested, but an abstraction that had very little to do with actual perception (Ch. 10).

I would now describe the "ground" theory as a theory of the "layout" of surfaces. I mean by layout the relations of surfaces to the ground and to one another, their arrangement. The layout includes both entities like objects or enclosures and features like convexity and concavity. These surfaces are opaque or, at most, semitransparent; but never perfectly transparent like the ghostly planes of geometry. The theory asserts that the perception of surface-layout is direct. This means that perception does not begin with two-dimensional form perception. Hence there is no special kind of perception called depth perception, and the third dimension is not lost in the retinal image since it was never in the environment to begin with. It is a vague term. If "depth" means the dimension of an object that goes with height and width, there is nothing special about it. Height becomes depth when seen from the top and width becomes depth when seen from the side. If "depth" means distance from here, then it involves self-perception and is continually changing as the observer moves about. The theory of depth perception is based on confusion and perpetuated by the fallacy of the retinal picture.

I now want to say that there is information in ambient light for the perception of the layout of surfaces but there are no cues or clues for the perception of depth. The traditional list of cues is worthless unless they are redefined in terms of optical structure. I tried to reformulate them in 1950 as "gradients and steps of retinal stimulation" (p. 137 ff). The hypothesis of gradients was a good beginning, but it had the great handicap of being based on physiological optics and the retinal image instead of ecological optics and the ambient array.

Such is the hypothesis of the direct perception of surface layout.

What is the evidence to support it? Some experiments had been carried out even before 1950, outdoor experiments in the open air instead of laboratory experiments with spots of light in a darkroom, but they were only a beginning (Gibson, 1947). Much more experimental evidence has accumulated in the last 25 years. Part II of this report will be devoted to a survey of that evidence.

The Psychophysics of Space and Form Perception

The experiments to be described were thought of as psychophysical at the time they were performed. There was to be a new psychophysics of perception as well as the old psychophysics of sensation. For I thought I had discovered that there were stimuli for perceptions in much the same way that there were known to be stimuli for sensations. This now seems to me a mistake. I failed to distinguish between stimulation proper and stimulus information, between what happens at passive receptors and what is available to active perceptual systems. Traditional psychophysics is a laboratory discipline in which physical stimuli are applied to an observer. He is prodded with controlled and systematically varied bits of energy so as to discover how his experience varies correspondingly. This procedure makes it difficult or impossible for the observer to extract invariants over time. Stimulus prods do not ordinarily carry information about the environment. A perceptual psychophysics will have to be quite different from sensory psychophysics.

What I had in mind by a psychophysics of perception was simply the hypothesis that perception was a direct instead of an indirect process. I wanted to exclude an _extra_ process of inference or construction. I meant (or should have meant) that animals and men _sense_ the environment, not in the meaning of having sensations but in the meaning of _detecting_. When I asserted that a gradient in the retinal image was a _stimulus_ for perception I meant only that it was sensed as a unit instead of being a collection of points whose separate sensations had to be put together in the brain. But the concept of the stimulus was not clear to me. I should have asserted that a gradient is stimulus _information_. For actually it is an invariant property of an optic array. I should not have implied that a percept was an automatic response to a stimulus as a sense impression is supposed to be. For even then, I realized that perceiving is an act, not a response, an act of attention, not a triggered impression, an achievement, not a reflex.

So what I should have meant by a "psychophysical" theory of perception in 1950 and by perception as a "function of stimulation" in 1959 was the hypothesis of a one-stage process for the perception of surface-layout instead of a two-stage process of first perceiving flat forms and then adding the cues for depth.

I now believe that there is no such thing as flat form perception, and this follows if there is no such thing as depth perception. (There are drawings and pictures, to be sure, but these are not "forms." The theory of form perception in psychology is no less confused than the theory of depth perception). But this was not clear when I wrote my book in 1950, and I promised not only a psychophysics of space perception in Chapter 5 but also a psychophysical approach to form perception in Chapter 10. This sounded promising and progressive. Visual outline forms, I suggested, are not unique entities. "They could be arranged in a systematic way such that each form would differ only gradually and continuously from all others" (p. 193). What counts is not the form as such but the dimensions of variation of form. And psychophysical experiments could be carried out if these dimensions were isolated.

Here was the germ of the modern hypothesis of the distinctive features of graphic symbols. (It was also the beginning of a much more radical hypothesis, that what the eye picks up is a sequential transformation, not a form, but that is a different matter). The study of form discrimination by psychophysical methods has flourished in the last 30 years. Garner, Hochberg, Attneave and others have achieved the systematic variation of outline forms and patterns in elegant ways. My only objection to this research is that it tells us nothing about perceiving the environment, only about perceiving with a picture. It still assumes that vision is simplest when there is a form on the retina that copies a form on a surface facing the retina. It perpetuates the fallacy that form perception is basic. It holds back the study of invariants in a changing array. But the hypothesis that forms are directly perceived does not upset the orthodoxies of visual theory as does the hypothesis that invariants are directly perceived, and hence it is widely accepted.

The psychophysical approach to surface perception is much more radical than this, and it has not been widely accepted over the last 25 years. Has its promise been fulfilled? There are experiments which can be summarized and the evidence should be pulled together.

Experiments on the Perception of a Surface as Distinguished from Nothing

Metzger's Experiment. Is tridimensional space perception based on bi-dimensional sensations to which the third dimension is added or is it based on surface-perception? The first experiment bearing on this issue is that of Metzger in 1930. He faced the eyes of his observer with a large dimly lighted plaster wall which rendered the light coming to the visual system unfocusable. Neither eye could accommodate and probably the eyes could not converge. The total field (Ganzfeld) was as he put it, homogeneous. Under high illumination the observer simply perceived the wall and the outcome was so obvious as to be uninteresting. But under low illumination the fine-grained texture of the surface was no longer registered by a human eye and the observer reported what he called a "fog" or "haze" or "mist of light." He certainly did not see a surface in two dimensions and therefore Metzger was tempted to conclude that he saw something in three dimensions, that is, was perceiving "space." But the impression of depth was not based on any impression of form.

But I did not see depth in the "mist of light." A better way to get a homogeneous field is to confront the eyes with a hemisphere of diffusing glass highly illuminated from the outside (Gibson and Dibble, 1952). It is still better to cover each eye with a fitted cap of strongly diffusing translucent material worn like a pair of goggles (Gibson and Waddell, 1952). The structure of the entering light, the optical texture, can thus be eliminated at any level of intensity. What my observers and I saw under these conditions could better be described as "nothing" in the sense of no "thing." It was like looking at the sky. There was no surface and no object at any distance. Depth was not present in the experience but missing from it. What the observer saw, as I would now put it, was an empty medium.

The essence of Metzger's experiment and its subsequent repetitions is not the plaster wall or the panoramic surface or the diffusing glass globe or the eye-caps. The experiment is one that provides discontinuities in the light to an eye at one extreme and eliminates them at the other. The purpose of the experiment is to control and vary the projective capacity of light. This must be isolated from the stimulating capacity of light. Metzger's experiment points to the distinction between an optic array with structure and a non-array without structure. To the extent that it has structure it specifies an environment.

A number of experiments using a panoramic surface under low illumination has been carried out, although the experimenters did not always realize what they were doing. But they all involved more or less faint discontinuities in the light to the eye. What the observers said they saw is complex and hard to describe. One attempt was made by Cohen in 1957 and the other experiments have been surveyed by Avant (1965). It is fair to say that there are intermediate perceptions between seeing nothing and seeing something as the discontinuities become stronger. These are the polar opposites of perception that are implied by Metzger's experiment, not the false opposites of seeing in two dimensions and seeing in three dimensions.

The confusion over whether there is or is not "depth" in Metzger's luminous fog is what led me to think that the whole theory of depth, distance, the third dimension, and space is misconceived. The important result is the neglected one that a surface is seen when the array has structure, or differences in different directions. A perfectly flat surface in front of the eyes is still a layout, that is, a wall, an environment. And that is all that "seeing in two dimensions" can possibly mean.

The Experiment With Translucent Eye-Caps. The experiment of eliminating optical texture from the light entering the eye by means of translucent diffusing goggles has been repeated many times. The observer is blind, not to light, since the photoreceptors are still stimulated, but to the environment, since the ocular system is inactivated, that is, its adjustments are frustrated. The eye-caps have also been adapted for experiments with young animals on the development of vision. It was known that diurnal animals like primates reared from birth in complete darkness were blind by certain criteria when brought into an illuminated environment (although this was not true of nocturnal animals whose ancestors were used to getting around in the dark.) It was now discovered that such animals deprived of optical structure but not of optical stimulation were also partly blind when the eye-caps were removed. Crudely speaking, they could not use their eyes properly. Anatomical degeneration of the photoreceptors had not occurred, as with the dark-reared animals, but the exploratory adjustments of the visual system had not developed normally. The experiments are described in Chapter 12 of Perceptual Learning and Development by Eleanor J. Gibson (1969).

Experiments With a Sheet of Glass. It is fairly well known that a clean sheet of plate glass that projects no reflections or highlights to the observer's eye is, as we say, "invisible." This fact is not self-explanatory but very interesting. It means that one perceives air where a material surface exists, and this is because air is specified by the optic array instead of the surface. I have observed men try to walk through plate glass doors to their great discomfiture and I have seen deer try to jump through plate glass windows with fatal results.

An ideally "clear" sheet of glass transmits both light considered as energy and an array of light considered as information. On the other hand, a "frosted" or "pebbled" sheet of glass, one that "diffuses," transmits optical energy but not optical information. The former can be "seen through" as we say, but the latter cannot. The latter can be "seen" but the former cannot. An imperceptible sheet of glass can be made increasingly perceptible by letting dust or powder fall on it, or by spattering it. Even the faintest specks can specify the surface. In this intermediate case the sheet transmits both the array from the layout behind the glass and the array from the glass itself. We say that we see the farther surface through the glass surface. The optical structure of one is mixed or interspersed with the optical structure of the other. The transparency of the near surface, more properly its semi-transparency, is then perceived. One sees two surfaces, separated in depth, in the same direction from here or, better, within the same visual solid angle of the ambient array. At least one sees them separated if the interspersed structures are different, or if the elements of one move relative to the elements of the other (Gibson, Gibson, Smith, and Flock, 1959).

Many of the above assertions are based on informal experiments, not published. But the reader can check them for himself with little trouble. It is fairly evident that a surface is experienced when the structural information to specify it is picked up.

Experiments With a Pseudo-Tunnel. In the case of a sheet of glass a surface may exist and go unperceived if it is not specified. In the next experiment a surface may be nonexistent but be perceived if it is specified. The pseudo-surface in this case was not flat and frontal but was a semi-enclosure, in fact, a cylindrical tunnel viewed from one end. I called it an optical tunnel to suggest that the surface was not material or substantial but was produced by the light to the eye. Another way of describing it would be to say that it was a virtual but not a real tunnel.

The purpose of the experiment was to provide information for the perception of the inside surface of a cylinder without the ordinary source of this information, the inside surface of a cylinder. It was what I would now call a display. The fact that the perception was illusory is incidental. I wanted to elicit a synthetic perception; and I, therefore, had to synthesize the information. It was an experiment in perceptual psychophysics, more exactly psycho-optics. The observers were "fooled," to be sure, but that was irrelevant. There was no information in the array to specify that it was display.

My collaborators and I (Gibson, Purdy and Lawrence, 1955) generated a visual solid angle of about 30° at the point of observation. This array consisted of alternating dark and light rings nested within one another, separated by abrupt circular contours. The number of rings and contours from the periphery to the center of the array could be varied. At one extreme there were 36 contours and at the other, 7.

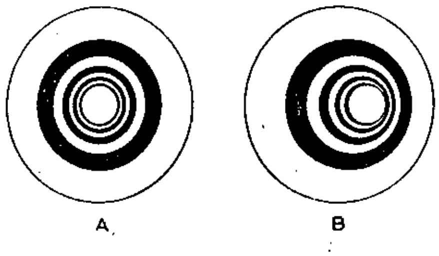

Fig. 2 from Gibson et al., 1955. Perspective cross sections of the optical tunnel of Fig. 1. Transitions are shown as white to black or the reverse. The picture on the right (B) represents a projection to a point to the right of the centered eye.

Thus the mean density of the contrasts in the array was varied from fine to coarse. The gradient of this density could also be varied; normally the density increased from the periphery toward the center.

The source of this array, the apparatus, was a set of large, very thin, plastic sheets, each hiding the next, with a one-foot hole cut in the center of each. They were indirectly illuminated from above or below. The contours in the array were caused by the edges of the sheets. The texture of the plastic was so fine as to be invisible. Black and white sheets could be hung in alternation one behind another or, as a control, all-black or all-white surfaces could be displayed. The observers looked into these holes from a booth, and extreme precautions were taken to prevent them from having any preconception of what they would see.

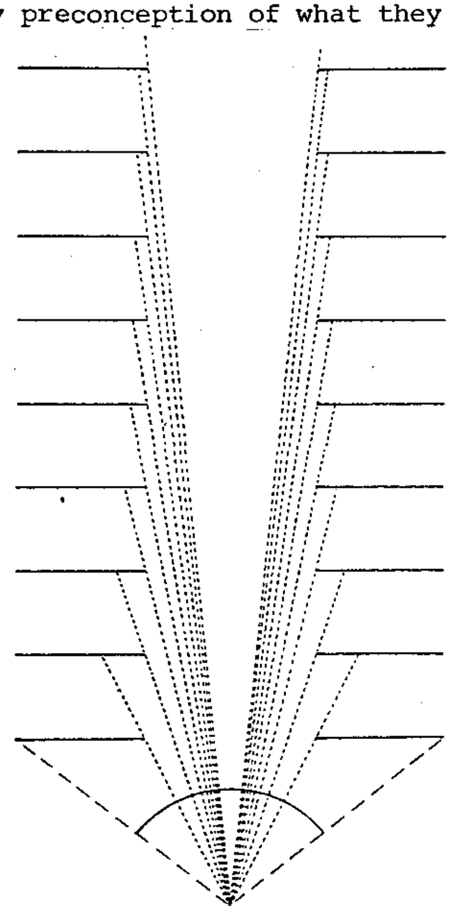

Fig. 1 from Gibson et al., 1955. Longitudinal section of an optical pseudo tunnel. Nine elements or transitions are shown as projected to a single centered eye. The increase in density of transitions from periphery to center of the array is evident on the angular cross section.

The principal result was as follows. When all-black or all-white surfaces were used the observers saw nothing. The area within the first hole was described as a hazy or misty fog, a dark or light film, without obvious depth. At the other extreme when 36 dark and light rings were displayed all observers saw a continuous striped cylindrical surface, a solid tunnel. No edges were seen and "a ball could be rolled from the far end to the entrance."

When 19 contrasts were displayed, two thirds of the observers described a solid tunnel. When 13 contrasts were displayed half did so, and when 7 contrasts were displayed only one third did so. In each case the remainder said they saw segments of surface with air in between, or else a series of circular edges (which was, of course, correct). With fewer contrasts, the experience became progressively less continuous and substantial. The proximity of these contours had proved to be crucial. Surfaciness depended on their mean density in the array.

What about the cylindrical shape of the surface, the receding layout of the tunnel? This could be altered in a striking way and the tunnel converted into a flat surface like an archery target with rings around a bullseye simply by rearranging the sheets in the way illustrated.

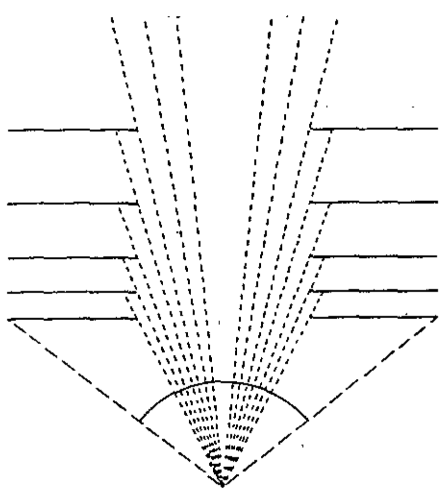

Fig. 3 from Gibson et al., 1955. Arrangement of a pseudo tunnel which provides a constant density of transitions from periphery to center.

The gradient of increasing proximity toward the center of the array gives way to an equal proximity. But the target surface instead of the tunnel surface only appeared if the observer's head was fixed and one eye covered, that is, if the array was frozen and single. If the head was moved or the other eye used, the tunnel shape was again seen. The frozen array specified a flat target, but the dual or transforming array specified a receding tunnel. This is only one of many experiments in which perception with monocular fixed vision is exceptional.

Conclusion

These experiments with a dimly lighted wall, with translucent eyecaps, with a sheet of glass, and with a pseudo-tunnel seem to show that the perception of surfaciness depends on the proximity to one another of discontinuities in the optic array. A surface is the interface between matter in the gaseous state and matter in the liquid or solid state. A surface forms as the matter on one side of the interface becomes more substantial. The medium is insubstantial. Mists, clouds, water, and solids are increasingly substantial. These substances are also increasingly opaque, except for a substance like glass which is rare in nature. What these experiments have done is to vary systematically the optical information for the perception of substantiality.

The experiments with the pseudo-tunnel also seems to show that the perception of a surface as such involves the perception of its layout, such as the front-facing layout of a wall or the slanting layout of a tunnel. Both are kinds of layout and the traditional distinction between two-dimensional and three-dimensional vision is a myth.

Experiments on the Perception of the Surface of Support

The ground outdoors or the floor indoors is the main surface of support. Animals have to be supported against gravity. If the layout of surfaces is to be substituted for depth and space in the theory of perception, this fundamental surface should get first consideration. How is it perceived? Animals like us can always feel the surface of support except when falling freely. But, not only that, we can also see the surface of support under our feet if we are, in fact, supported. The ground is always specified in the lower portion of the ambient array. The standing infant can always see it and can always see his feet hiding parts of it. This is a law of ecological optics. Kittens, for example, seem to perceive the layout of this surface, that is, the convex or concave departures from flatness that do or do not afford "footing."

The Glass Floor Apparatus. A floor can be experimentally modified even if the surface of the earth cannot. At the time when the "visual cliff" was being constructed for experiments with young animals by Gibson and Walk (1960) observations were made with a large sheet of glass that was horizontal instead of vertical, that is, a glass floor instead of a glass wall. The animal or child can be put down on this surface under two conditions: when it is visible, by virtue of textured paper placed just under the glass and when it is invisible, with the paper placed far below the glass. There is inertial support in both conditions but optical support only under the first. Likewise there is equal mechanical contact with the feet in both conditions but optical contact with the feet only in the first.

The animals or babies tested in this experiment would walk or crawl normally when they could both see and feel the surface but would not do so when they could only feel the surface, freezing or crouching and showing signs of discomfort. Some animals even adopted the posture they would have when falling (Gibson and Walk, 1960, pp. 65-66). The conclusion seems to be that some animals require optical information for support as well as inertial and tactual information in order to walk normally. For my part, I should feel very uncomfortable if I had to stand on a large observation platform with a transparent floor through which the ground was seen far below.

The optical information in this experiment is contradictory to the haptic information. One sees oneself not in contact with a surface of support, as being "up in the air," but one feels oneself in contact with a surface of support and, of course, one feels the normal pull of gravity in the vestibular organ. In such cases of contradictory or conflicting information which gets picked up? Perception is indefinite or uncertain, although sometimes one perceptual system wins out over the other.

Note that the perception of the ground and the co-perception of the self are inseparable in this situation. One's body <u>in relation to</u> the ground is what gets attention. Perception and proprioception are complementary. But the commonly accepted theories of space perception do not bring out this fact.

<u>The Visual Cliff Apparatus</u>. The visual cliff experiments of Gibson, Walk, and subsequently others are very well known. They represented a new approach to the ancient puzzle of depth perception, and the results obtained with newborn or dark-reared animals were surprising since they suggested that depth perception was innate. But the sight of a cliff is <u>not</u> a case of perceiving the third dimension. The cliff is a feature of the terrain, a highly significant special kind of dihedral angle in ecological geometry, a falling-off place. Only the edge at the top of a cliff is dangerous, not the concave corner at the bottom. This is an "occluding edge." But it has the special character of being an edge of the surface of support, unlike the edge of a wall or a tree. One can walk around the edge of a wall but not off the edge of a cliff. To perceive a cliff is to detect a layout but, more than that, it is to detect an "affordance", a negative affordance for locomotion, a place when the surface of support ends.

An affordance is <u>for</u> a species of animal. It is a layout <u>relative to</u> the animal and commensurate with its body. A cliff is a drop-off that is <u>large</u> relative to the size of the animal and a step is a drop-off that is small relative to its size. A falling-off edge is dangerous, but a stepping-down edge is not. What animals need to perceive is not layout as such but the affordances of the layout.

Gibson and Walk (1960; Walk and Gibson, 1961) constructed a virtual cliff with the glass-floor apparatus. They tested animals and babies to determine whether or not they would go forward over an edge that was specified only in the optic array. Actually they provided two edges on either side of a narrow platform, one a falling-off edge and the other a stepping-down edge appropriate to the species of animal being tested. The animal's choices were recorded. Nearly all animals that walk on the ground chose the shallow edge instead of the deep one.

The results have always been discussed in terms of depth perception and the traditional cues for depth. But I suggest that they are more intelligible in terms of the perception of layout and affordances. The separation in depth at an edge of the surface of support is not at all the same thing as the depth dimension of abstract space. As for innate versus learned perception, it is much more sensible to assume an innate capacity to notice falling-off places in terrestrial animals than it is to assume that they have innate ideas or mental concepts of geometry.

The Perception of an Object Resting on the Ground. I suggested that one sees the contact of his feet with the ground. This is equally true for other objects than feet. We see whether an object is on the ground or up in the air. How is this contact with or separation from the ground perceived? The answer is suggested by an informal experiment described in my book on the Visual World (Gibson, 1950, Figure 72, p. 178 ff). It might be called the invisibly supported object experiment. I did not clearly understand it at the time but the optics of what I call occluding edges now makes it more intelligible.

A detached object of some sort can be attached to a long rod that is hidden at the point of observation. The rod can be lowered so that the object rests on the floor (or table) or raised so that it stands up in the air. The object can be a cardboard rectangle or trapezoid, or a ball, but it must be large enough to hide the rod and its base. An observer who stands at the proper position and looks with two eyes, or with one eye and a normally moving head, perceives a resting object as resting on the surface of support and a raised object as raised above the surface of support. The size and distance of the object are seen correctly. But an observer who looks with one eye and a fixed head (as with a peephole, or a biting board) gets an entirely different perception. A resting object is seen correctly but a raised object is also seen to be resting on the surface. It is seen at the place where its contour adjoins the texture of the surface. It appears farther away and larger than it really is.

This illusion is very interesting. It appears only with monocular frozen vision--a rare and unnatural kind of vision. The increments and decrements of the texture of the ground at the edges of the object have been eliminated, both those of one eye relative to the other and those that are progressive in time at each eye. In traditional terminology, binocular parallax and motion parallax are absent. But it is just these increments and decrements of the ground-texture that specify the separation of object from ground. The absence of this accretion/deletion specifies contact of the object with the ground. A surface is perceived to "stand up" or "stand forth" or "stand out" from the surface that extends behind it only to the extent that the gap is specified. And this depends on seeing from different points of observation, either two points of observation at the same time or two points of observation at different times.

A flat surface that "goes back to" or "lies flat on" the ground will have a different size, shape, and even reflectance from what it has when it stands forth in the air. This feature of the illusion is also very interesting, but the only published study of it is that of Hochberg and Bedk (1954).

Experiments With the Ground as Background

Investigators in the tradition of space perception and the cues for depth have usually done experiments with a background in the "frontal plane," that is, a surface facing the observer, a wall, a screen, or a sheet of paper. A form in this plane is most similar to a form on the retina, and extension in this plane might be seen as a simple sensation. This follows from retinal image optics. But investigators of environment perception should do experiments with the ground as background, studying surfaces instead of forms, and using ecological optics. Instead of studying distance in the air, they should study recession along the ground. Distance as such cannot be seen directly but only inferred or computed. Recession along the ground can be seen directly.

Distance and Size Perception on the Ground. Although the linear perspective of a street in a painting had been known since the Renaissance, and the converging appearance of a parallel alley of trees in a designed landscape had been discussed since the 18th century, no one had ever studied the perception of a naturally textured ground. Linear perspective was an obvious cue for distance but the gradient of density or proximity of the texture of the ground was not so obvious. Boring has described the old experiments with artificial alleys (1942, pp. 290-296). But the first experiment with an ordinary textured field outdoors, I believe, was published at the end of World War II (Gibson, 1947). A plowed field without furrows receding almost to the horizon was used. No straight edges or lines were visible. This original experiment required the judgment of the height of a stake planted in the field at some distance up to half a mile. At such a distance the optical size of the elements of texture and the optical size of the stake itself were extremely small.

Up until that time the unanimous conclusion of observers had been that parallel lines were seen to converge and that objects were seen to be smaller "in the distance." There was a tendency for "size constancy" of objects, to be sure, but it was usually incomplete. The assumption had always been that size constancy must "break down," that an object will cease to be even visible at some eventual distance and that presumably it ceases to be visible by way of becoming smaller. (See Gibson, 1950, p. 183 for a statement of this line of reasoning). With the naive observers in the open field experiment, however, the judgments of the size of the stake did not decrease, even when it was a 10-minute walk away and becoming hard to make out. The judgments became more variable with distance but not smaller. Size constancy did not break down. The size of the object only became less definite with distance, not smaller.

The implication of this result to me is that certain invariant ratios of figure and ground were picked up unawares by the observers, and that the size of the retinal image went unnoticed. No matter how far away the object was it intercepted the same number of texture elements of the ground, and the proportion of the stake extending above the horizon to that extending below the horizon was invariant for any distance. These invariants are not cues but information for direct size-perception. The observers in this experiment were aviation trainees and were not interested in the perspective appearances of the terrain and the objects. They could not care less for the patchwork of colors in the visual field that had long fascinated painters and psychologists. They were set to pick up information that would permit a size-match between the distant stake and one of a set of nearby stakes.

The perception of the size and distance of an object on the ground had proved to be unlike the perception of the size and distance of an object in the sky. The invariants are missing in the latter case. The silhouette of an airplane might be a fifty-foot fighter at a one mile altitude or a hundred-foot bomber at a two mile altitude. Airplane spotters could be trained to estimate altitude, but only by the method of recognizing the shape, knowing the size by having memorized the wing-span, and inferring the distance from the augular size. Errors were considerable at best. This kind of indirect knowledge is not characteristic of ordinary perception.

Comparing Stretches of Distance Along the Ground. The size of an object on the ground is not entirely separable from the sizes of the objects that compose the ground. The terrain is made of clods and particles of earth, or rocks and pebbles, or grass-clumps and grass-blades. These nested objects might have "size-constancy" just as much as orthodox objects. In the next set of experiments on ground-perception the very distinction between size and distance breaks down. What had to be compared were not stakes or objects but stretches of the ground itself, distances between markers placed by the experimenter. In this case distances between here and there could be compared with distances between there and there. These open field experiments were conducted by Eleanor J. Gibson (Gibson and Bergman, 1954; Gibson, Bergman, and Purdy, 1955; Purdy and Gibson, 1955).

Markers could be set down and moved anywhere in a level field of grass up to 350 yards away. The most interesting experiment of the series required the observer to bisect a stretch of distance, which could extend either from his feet to a marker or from one marker to another (Purdy and Gibson, 1955). A mobile marker on wheels had to be stopped by the observer at the halfway point. The ability to bisect a length of surface had long ago been tested with a stick (called a Galton bar) but not with a piece of ground on which the observer stood.

All observers could bisect a stretch of distance without difficulty and with some accuracy. The farther stretch could be matched to the nearer one although the visual angles did not match. The farther visual angle was compressed relative to the nearer and its surface was, to use a vague term, "foreshortened." But no constant error was evident. A stretch from here to there could be equated with a stretch from there to there. The conclusion must be that observers were not paying attention to the visual angles; they must have been noticing information. They might have been, without knowing it, detecting the amount of texture in a visual angle. The number of grass-clumps projected in the farther half of a stretch of distance is exactly the same as the number projected in the nearer half. It is true that the optical texture of the grass becomes denser and more vertically compressed as the ground recedes from the observer but the rule of equal amounts of texture for equal amounts of terrain remains invarient.

This is a powerful invariant. It holds for either dimension of the terrain for width as well as depth. In fact it holds for any regularly textured surface whatever, that is, any surface of the same substance. And it holds for walls and ceilings as well as floors. To say that a surface is regularly textured is only to assume that bits of the world tend to be evenly spaced. They do not have to be perfectly regular like crystals in a lattice but only "stochastically" regular.

The implications of this experiment on fractionating or scaling the ground are radical and far-reaching. The world consists not only of distances from here, my world, but also of distances from there, the world of another person. These intervals seem to be strikingly equivalent.

The rule of equal amounts of texture for equal amounts of terrain suggests that both size and distance are perceived directly. The theory that the perceiver allows for the distance in perceiving the size of something is unnecessary. The assumption that the cues for distance compensate for the sensed smallness of the retinal image is no longer persuasive. Note that the pickup of the amount of texture in a visual solid angle of the optic array is not a matter of counting units, that is, of measuring with an arbitrary unit. The other experiments of this open field series required the observers to make absolute judgments, so called, of distances in terms of yards. They could learn to do so readily enough (Gibson and Bergman, 1954; Gibson, Bergman, &

Purdy, 1955). But it was clear that one had to see the distance before one could apply a number to it.

Observations of the Ground and the Horizon. To the extent that the terrain is flat and open, the horizon is present in the ambient optic array. It is a great circle between the upper and the lower hemisphere separating the sky and the earth. But this is a limiting case. The farther stretches of the ground are usually hidden by frontal surfaces such as hills, trees, and walls. Even in an enclosure, however, there has to be a surface of support, a textured floor. The maximum coarseness of its optical texture is straight down, where the feet are, and the density increases outward from this center. These radial gradients projected from the surface of support increase with increasing size of the floor. The densities of texture do not become infinite except when there is an infinitely distant horizon. Only at this limit is the optical structure of the array wholly compressed. But the gradients of density specify where the outdoors horizon would be even in an enclosure. There exists a sort of implicit horizon even when the earth-sky horizon is hidden.

The concept of a vanishing point comes from artificial perspective, converging parallels, and the theory of the picture plane. The vanishing limit of optical structure at the horizon comes from natural perspective, ecological optics, and the theory of the ambient optic array. The two kinds of perspective should not be confused, although they have many principles in common

The terrestrial horizon is thus an invariant feature of terrestrial vision, an invariant of any and all ambient arrays, at any and all points of observation. The horizon never moves, even when every other structure in the light is changing. This stationary great circle is, in fact, that to which all optical motions have reference. It is neither subjective nor objective; it expresses the reciprocity of observer and environment; it is an invariant of ecological optics.

The horizon is the same as the skyline only in the case of the open ground or the open ocean. The earth-sky contrast may differ from the true horizon because of hills or mountains. The horizon is perpendicular to the pull of gravity, and to the two poles of the ambient array at the sky-center above and the center of maximum structure below; that is to say, the horizon is horizontal. With reference to this invariant, all other objects, edges and layouts in the environment are judged to be either upright or tilted. In fact the observer perceives himself to be in an upright or tilted posture relative to this invariant. (For an early discussion of visual uprightness and tilt in terms of the retinal image, see Gibson, 1952, on the "phenomenal vertical").

The facts about the terrestrial horizon are scarcely mentioned in traditional optics. The only empirical study of it is one by Sedgwick (1973) based on ecological optics. He shows how it is an important source of invariant information for the perception of all kinds of objects. All terrestrial objects, for example, of the same height are cut by the horizon in the same ratio no matter what the angular size of the object may be. This is the "horizon ratio relation" in its simplest form. Any two trees or poles bisected by the horizon are the same height, and they are also precisely twice my eye-height. More complex ratios specify more complex layouts. Sedgwick showed that judgments of the sizes of objects represented in pictures were actually determined by these ratios.

The perceiving of what might be called eye-level on the walls, windows, trees, poles, and buildings of the environment is another case of the complementarity between seeing the layout of the environment and seeing oneself in the environment. The horizon is at eye-level relative to the furniture of the earth. But this is my eye-level and it goes up and down as I stand and sit. If I want my eye-level, the horizon, to rise above all the clutter of the environment I must climb up to a high place. Thus the perception of here and the perception of infinitely distant from here are linked.

Experiments on the Perception of Slant

From the beginning of these experiments on direct perception in 1950 the crucial importance of the density of optical texture was evident. How could it be varied systematically in an experiment? Along with the outdoor experiments I wanted to try indoor experiments in the laboratory. I did not then understand ambient light but only the retinal image, and this led me to experiment with texture density in a window or picture. The density could be increased upward in the display (or downward or rightward or leftward) and the virtual surface would then be expected to slant upward (or downward or whatever). It should slant away in the direction of increasing texture density, that is, it should be inclined from the frontal plane at a certain angle that corresponded to the rate of change of density, the gradient of density. Every piece of surface in the world, I thought, had this quality of slant (Gibson, 1950a). The slant of the apparent surface behind the apparent window could be judged by a method such as putting the palm of the hand at the same inclination from the frontal plane and recording it with an adjustable "palm board." This appeared to be a neat psychophysical experiment, for it isolated a variable, the gradient of density.

The first experiment by Gibson (1950a) showed that with a uniform density over the display the phenomenal slant is zero, and that with increases of density in a given direction one perceives increasing slant in that direction. But the apparent slant is not proportional to the geometrically predicted slant. It is less than it should be theoretically. The experiment has been repeated with modifications by Gibson and Cornsweet (1952), Beck and Gibson (1955), Bergman and Gibson (1959) and by many other investigators. It is not a neat psychophysical experiment. Phonemenal slant does not simply correspond to the gradient. The complexities of the results are described by Flock (1964, 1965) and by Freeman (1965, 1966).

In consideration of the theory of layout we can now understand, I think, what was wrong with these experiments. The kind of slant studied was optical, not geographical, as noted by Gibson and Cornsweet (1952). It was relative to the frontal plane perpendicular to the line of sight, not relative to the surface of the earth. It was thus merely a new kind of depth, a quality added to each of the flat forms in the patchwork of the visual field. I had made the mistake of thinking that the experience of the layout of the environment could be compounded of all the optical slants of each piece of surface. I was thinking of slant as an absolute quality whereas it is always relative. Convexities and concavities, both planar and curved, are not made up of elementary impressions of slant.

The impression of slant cannot be isolated by displaying a texture inside a window. The perception of the occluding edge comes into it, and the surface is slanted relative to the surface with the window in it. The separation of these surfaces is underestimated, as the experimental results showed.

The supposedly absolute judgment of the slant of a surface behind a window becomes more accurate when a graded decrease of <u>velocity</u> of the texture across the display is substituted for a graded increase of <u>density</u> of the texture. This fact was demonstrated by Flock (1964). The virtual surface "stands back" from the virtual window. It slants away in the direction of decreasing flow of the texture but is perceived to be a rigidly moving surface if the flow gradient is mathematically appropriate. But this experiment belongs not with experiments on surface layout but on <u>changing</u> surface layout, and these experiments will be described later. Before that, however, the experiments should be reported that led to the discovery of what I call <u>visual kinesthesis</u>.

III THE DISCOVERY OF VISUAL KINESTHESIS

At the time when great numbers of students were being trained to fly airplanes, and considerable numbers were failing, it seemed like a good idea to try and find out whether or not a student could see what was necessary in order to land one before putting him up there and trying to make him learn to see. One thing he had to see was the aiming point of a landing glide, the direction in which he was going. A test was devised consisting of a series of motion picture shots with a camera dollying down toward a model runway (Gibson, 1947, Chapter 9). The testee had to say whether he was aiming at spot A, B, C, or D, all marked on the runway. It was called a test of "landing judgment." This was the beginning of an inquiry that went on for years.

It turns out that the aiming point of any locomotion is the center of the centrifugal flow of the ambient optic array. Whatever object or spot on the ground is specified at that null point is the object or spot you are approaching. This is an exact statement. But since I could not conceive of the ambient optic array at that time, only the retinal image, I first tried to state the flow in terms of retinal motion and gradients of retinal velocity. Such a statement cannot be made exact and leads to contradictions. Not until later were the principles of the two foci of radial outflow and inflow in the whole array at a moving point of observation described precisely (Gibson, Olum, & Rosenblatt, 1955).

We gave a mathematical description of what we called "motion perspective" in the optic array, for any direction of locomotion relative to a flat earth. All optical flow vanishes at the horizon and also at the two centers that specify <u>going toward</u> and <u>coming from</u>. Motion perspective was much more than the "cue" of motion parallax. As this was formulated by Helmholtz it was no more than a rule for "drawing conclusions" about the distance of an object and, in any case the rule did not hold for an object on the line of locomotion. Motion perspective did not refer to "apparent" motions of objects but to the layout of the earth. And it "told" the observer not only about the earth but also about himself, the fact of his locomotion and the direction of it. The focus of outflow (or the center of optical expansion or magnification) is not a sensory cue but an optical invariant, a non-change in the midst of change. The focus is formless, and is the same for any form of structure, for grass, trees, a brick wall or the surface of a cloud.

Student pilots see where they are going on the basis of this invariant (and get better with practice). Drivers of cars see where they are going if they pay attention. Viewers of a Cinerama screen see where they are going in the represented environment. A bee that lands on a flower must see where it is going. And all of them at the same time see the layout of the environment through which they are going. This is a fact with extremely radical implications for psychology. For it is difficult to understand how a train of signals coming in over the optic nerve could explain it. How could it have two meanings at once, a subjective meaning, and an objective one? How could it yield an experience of self-movement and an experience of the external world at the same time? How could visual motion sensations get converted into a stationary environment and a moving self? The doctrine of the special senses and the theory of sensory channels comes into question. A perceptual system must be at work which extracts invariants. Exteroception and proprioception must be complementary.

There are various ways of putting this discovery, although old words must be used in new ways since age-old doctrines are being contradicted. I suggest that vision is kinesthetic in that it registers movements of the body just as much as does the muscle-joint-skin and inner ear system. Vision picks up both movement of the whole body relative to the ground and movement of a member of the body relative to the whole. Visual kinesthesis goes along with muscular kinesthesis. The doctrine that vision is "exteroceptive," that it obtains "external" information only, is simply false. Vision obtains information about both the environment and the self. In fact, all the senses do so when they are considered as perceptual systems (Gibson, 1966).

Vision, of course, is also statesthetic if one wants to be precise about words, in that it picks up non-movement of the body and its members. But since non-movement is actually only a limiting case of movement, the term kinesthesis will do for both. The point is that a flowing and a frozen optic array specify respectively an observer in locomotion and an observer at rest relative to a fixed environment. Motion and rest are in fact what an observer experiences with flow and non-flow of the array.

Motion perspective should not be confused with visual kinesthesis. Motion perspective is a way of describing the information in an abstract array to specify both layout and locomotion. If the information is picked up, both visual layout perception and visual kinesthesis will occur. But motion perspective is analyzed for an ambient array at an unoccupied point of observation. The field of view and the body of the observer are not in the ambient array. In visual kinesthesis, on the other hand, the nose and the body are visible. It is the awareness of movement of one's own particular body relative to the earth as specified by the flow pattern of the array within the field of view.

Another preliminary point should be made. It is most important not to confuse visual kinesthesis with visual "feedback," a term that has currency in psychology and physiology today, but one that is not very clear. The term is used with reference to voluntary movement in connection with the control of purposive action. If a movement is caused by a command in the brain, the efferent impulses in motor nerves are followed by afferent impulses in sensory nerves that are actually reafferent, i.e., impulses that are fed back into the brain. "Feedback," therefore, comes with an active movement. But not all movements are active; some are passive, as when a bird is moved in the wind or a man is moved in a vehicle. Visual kinesthesis is the same for a passive as for an active movement, but visual feedback is absent with a passive movement. The problem of the information for a given movement should not be confounded with the additional problem of the control of movement. Visual kinesthesis is important in the control of locomotion but is not the same thing. It is true that one often needs to see how he has just moved in order to decide how to move next. But the first question is how does he see how he has just moved?

The current confusion between kinesthesis and feedback helps to explain why visual kinesthesis is not recognized as a fact of psychology. But it is a fact as the following experiments show, experiments on the inducing of the experience of passive movement.

Experiments with Visual Kinesthesis.

Until very recently most of the evidence about induced ego movement had to come from motion pictures, or simulators for training, or amusement park devices. The flow of the optic array in a glide-path can be represented, more or less, in a motion picture (Gibson, 1947, p. 230 ff.) and the observer will see himself moving down toward a pseudo-airfield however much he is still aware of being seated in a room and looking at a screen. With a "Cinerama" screen the virtual window may sample as much as 160° of the ambient array, instead of the mere 20° or 30° of the usual movie setup, and the illusion of locomotion may then be compelling, uncomfortably so. There have been training devices having a panoramic curved screen of 200° from side to side, for example one that simulates flight in a helicopter, with which the experience of rising, flying, banking, and landing is so vivid that the "illusion of reality" is almost complete, although the observer's body is all the time anchored to the floor. There have also been attempts to simulate automobile driving.

In the best of these displays the laws of both natural angular perspective and of motion perspective have been observed. The virtual world, the layout of earth and objects, appears to be stationary and rigid. Only the observer moves. But if the projection system or the lens system that creates the display is imperfect, stretching or rubbery motions of the layout will be seen. Then the non-rigid appearance of the environment is not only disconcerting but also sometimes sick-making.

The laws of motion perspective for flight over the earth with its horizon can even be set into a computer which then generates a display on a television screen that simulates any desired maneuver. But all these experiments, if they can be called that, have been done in the interests of the aviation industry rather than those of understanding perception, and the reports are only found in the technical engineering literature.

The reader may have observed for himself that what is called a dolly shot in cinematography will give him the experience of being a spectator who is following behind or moving ahead of one or more characters that are walking along. The arrangement of the surfaces and other persons in the scene is more vividly given than it is in a stationary shot. The dolly shot is to be distinguished from the panning shot where the viewer gets the experience not of locomotion but of turning the head while keeping the same point of observation.

The gliding room experiment. Recently a laboratory apparatus has been constructed for the stated purpose of investigating visual kinesthesis during locomotion, and separating it from the kinesthesis of the muscle-joint-skin and vestibular system (Lishman and Lee, 1973). The flow of the ambient array is produced by a moving enclosure, a room of sorts with walls and ceilings that can be made to glide over the real floor since it is hung by its corners from a great height barely above the floor. I am tempted to call it an invisibly moving room since, except for the floor, there is no information for its motion relative to the earth. It is a pseudo-environment. If contact of the feet with the surface of support is obscured and if the floor is hidden, the illusion of being moved forward and backward in the room is compelling. This is accomplished by what Lishman and Lee call a "trolley" in which the observer stands.

Rotations of the body. Swinging, tilting, turning. Besides the linear locomotions of the body there are the movements of rotation, which can occur on a lateral axis, a front-back axis, or a head-foot axis. The movement of a child in a swing has a component of rotation on a lateral axis, like a somersault. The movement of tilting sideways is a rotation on a front-back axis. The movement of being turned in a swivel chair or of turning the head is rotation on a head-foot axis. Pure visual kinesthesis of all these rotations can be induced with an invisibly moving room, that is, by putting the observer in an enclosure with a surface of support attached to the earth that is inconspicuous, and then rotating the enclosure.

An amusement-park device called the "Haunted Swing" used to be popular. A boy (and usually his girl) entered what appeared to be an ordinary room and were seated in a swing hanging from a bar running horizontally across the room. The room then began to swing, not the seat, on the shaft from which the seat was suspended. When the room eventually made a complete revolution the occupants felt themselves go head over heels. What a sensation! It should be noted that the illusion vanished instantly, however, if the eyes were shut, as visual kinesthesis would be expected to do. An account of the experience, and the original reference, is given by Gibson and Mowrer (1938).

An experimental room can be made so as to tilt on a front-back axis, with an observer in an upright seat. Tilting rooms of this sort have been built in laboratories, and they produced a large literature some 20 years ago (e.g., Witkin 1949). As the room invisibly rotates both one's body and the chair seem to rotate in the room. Some part of the experienced body tilt usually remains even after the room has become stationary. This latter fact, the feeling of one's posture as dependent on both the visual sense and the bodily senses was what aroused the greatest interest of experimenters. The arguments in terms of sensations were inconclusive, however. For a discussion of the "phenomenal vertical" in terms of stimuli and cues, see Gibson (1952).

Finally, an experimental room can be made to rotate on a vertical axis. This is a common apparatus in many laboratories, going under the name of an optokinetic drum. (See, for example, Smith and Bojar, 1938.) It has usually been thought of as a device for studying the eye-movements of animals instead of visual kinesthesis, but it can be adapted for the human observer. A textured enclosure, usually a vertically striped cylinder, is rotated around the animal and his head-eye system then shows the same compensatory movements that it would if he were really being turned. Optically, although not inertially, he is being turned. The human subject usually says that he feels himself being turned. There has to be a real surface of support, however, and, in my experiments, the illusion seemed to depend on not seeing it, or not paying attention to the floor under one's feet. You could anchor yourself to that, if you tried, and then you become aware of the hidden environment outside the room.

What is picked up in these three cases of swinging, tilting and turning? It must be a relation between the ambient optic array specifying the world and the edges of the field of view specifying the self. As already suggested, the upper and lower edges of the field of view sweep over the ambient array in swinging; the field of view wheels over the array in tilting; and the lateral edges of the field sweep across the array in turning.

It should be noted that, insofar as the three rotations of the body occur without locomotion through the environment, motion perspective does not arise and the ambient array does not flow. The information for the perception of layout is thus minimal.

> To speak of the environment being rotated relative to the observer in these cases (instead of his body being rotated relative to the environment) would be simply nonsense. The environment (in the sense of the persisting environment, the world) is that with reference to which objects move, animals move, and surfaces deform. There has to be an underlying non-change if change is to be specified. The principle of the relativity of motion cannot be applied to rotation of the body.

Visual kinesthesis with movements of the limbs and hands. Linear locomotions of the whole body and rotations of the body are seen, and this fact has been demonstrated by experiment. But, not only that, elastic movements of some parts of the body are seen, especially the legs and arms and particularly the hands. The layout of these parts of the body surface is specified as well as the layout of the external surfaces, and change of bodily layout is just as evident as change of environmental layout. The surfaces of the extremities of the body are revealed when one looks downward. This also is a kind of visual kinesthesis. The sight of the hands and the fact of so-called "eye-hand coordination" is so familiar that it scarcely needs demonstration.

There are "squirming five-pronged shapes" that specify the hands and that protrude into the field of view. They contract or expand as one extends or flexes the arms. The movements are guided, steered, and controlled by transformations of these shapes. These movements are seldom passive as the movements of locomotion and rotation of the whole body often are, and visual kinesthesis of the hands cannot be experimentally isolated from muscle-joint-skin kinesthesis, the traditional sort that accompanies the action of the haptic system. Thus visual kinesthesis of the extremities is usually a case of feedback. It is

MAKE SURE SPECIFICITY GETS OUT IN CONTRAST TO "Similarity", isomorphism.

possible, of course, to attach a relaxed arm to a lever system which then moves it. The method is used to isolate kinesthetic information from the joints of the arm, but it would also yield visual kinesthesis which is not a case of feedback. But this is rare.

IV EXPERIMENTS ON THE PERCEPTION OF CHANGING SURFACE LAYOUT

Along with the traditional assumption that form perception in the frontal plane is basic and is thus simpler to understand goes the assumption that motion perception in the frontal plane is basic and is simpler to understand. The fallacy of the retinal image and the cues for depth underlies the second assumption as much as the first. But the concept of retinal motion as a "scratching of the retina with pencils of light" (Gibson, 1968) is so deep-lying that it is even harder to get rid of than the concept of retinal form. Only gradually and reluctantly did I give it up, and only when forced to do so by experiments. My present hypothesis is that the perception of events depends upon nothing less than disturbances of structure in the ambient array. Disturbances of structure can specify events without being similar to them.

Apparatus for the Study of Motion in the Frontal Plane

In order to study perception an experimenter must devise an apparatus that will "stimulate" perception or, as I would now want to put it, that will display the information for perception. Until recently the principal types of apparatus devised for the perception of motion were as follows.

1. The stroboscope and its variants. This is a device that exposes or flashes different stationary patterns in succession. Cinematography developed from it. Since each successive "stimulus" was motionless and the retina was thus never "stimulated" by motion, the motion perceived was said to be only "apparent," not "real." But this assertion is an example of the muddled thinking to which stimulus theory can lead. The stimulus-information for motion is the change of pattern, and the information is the same for an intermittent as for a continuous change.

The stroboscope only demonstrates that the motion of an object in the world from one place to another does not have to be copied by a corresponding motion of an image on the retina from one point to another in order to perceive that event. But we should never have supposed in the first place that it did have to be copied on the retina.

2. The moving endless belt. A striped or textured surface behind a window can be made to move continuously in a certain direction and at any chosen speed. Many experiments were carried out with this device. But the results for speed and velocity, far from being simple, were complex and puzzling. The just-noticeable speed, for example, could not be determined, although if motion on the retina were a stimulus it should have an absolute threshold. Eventually I came to suspect that what the eye was picking up was not the "motion" of the surface relative to the window but the progressive revealing and concealing of the elements of the surface at the occluding edges of the window (Gibson, 1968).

3. The rotating disk apparatus. If a color wheel was made to rotate slowly instead of rapidly the motion of the surface of the disk could be seen. The disk could be displayed either behind a circular window or in front of a background. If the observer fixated the center of the disk no eye movements would occur to complicate the retinal image, which would be a circle and its surroundings. But does this retinal change constitute a "motion" as the term is understook in physics, a rotary spin measured in terms of degrees of arc per second of time? I finally came to understand that actually the wheeling of the circle in its surrounding is a shearing of the texture of the array at the contour of the circle.

A disk of this sort can also be used as a turntable for a blank circular sheet of paper on which forms are drawn. With rotation of the disk the forms undergo orbital motions, and sometimes very curious perceptions result.

4. The disk-and-slot apparatus. If a spiral line is drawn on such a disk instead of a texture, a perception of expansion (or contraction) is induced when it is rotated slowly. And if the disk is screened except for a slot, there will occur the perception of a thing moving along the slot. Michotte (1946) has used this device to study the perception of

one thing bumping another, for example. In these cases the optical motions in the array of light from the display are radically different from the mechanical motions of the apparatus that produced them. This radical difference has seemed very puzzling to believers in retinal image optics; it becomes intelligible only with the acceptance of ecological optics. The perception of what might be called slot-motion with Michotte's apparatus is particularly interesting, for it seems to depend on what happens optically at the edges of an aperture or window.

5. The method of shadow-projection. Beginning with the Chinese shadow-plays of antiquity, moving shadows have been cast on a screen so as to induce the perception of moving objects or persons. The light source must be either very small or very distant to make the contour of the silhouette sharp. The opaque object, the shadow-caster, is properly said to be projected on the screen by radiant light, that is, by rectilinear rays. (The light from the screen to the point of observation, however, should not be said to be projected since it is ambient light, and its array consists of visual solid angles, not rays.) Projection from a very small near source is polar in that the rays diverge from a point. Projection from a very distant source like the sun is parallel inasmuch as the rays do not diverge.

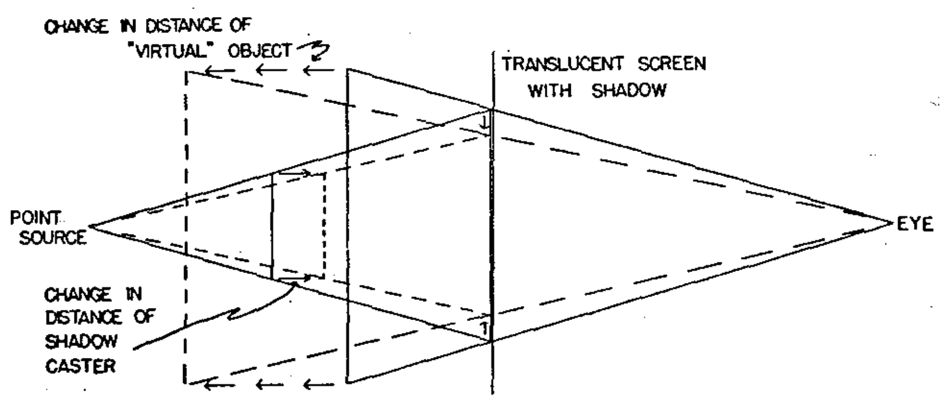

Fig. 1. from Gibson, 1957. The shadow transformer.

With an opaque screen the radiant light and the ambient array are on the same side of the screen and the observer can see the shadow-caster. With a translucent screen, however, the light to the screen and the array from the screen can be on opposite sides, and the observer cannot see the shadow-caster. The visual solid angle of the shadow surrounded by light constitutes information for perceiving an object on an empty background, that is, a virtual object seen as if against the sky.

The shadow-caster, an opaque surface or object, can be mounted on a transparent sheet and caused to move by the experimenter. Or the mount can be treated so as to be opaque in some parts and transparent in others, or to vary from opaque to transparent. The last case is essentially that of the photographic "slide." The projection of photographic pictures, either singly or in sequence, is in principle no more than the casting of shadows on a screen corresponding to the varying opacity of the film.

The motion of the virtual object seen behind the screen corresponds to the motion of the shadow-caster, but with certain inverse relationships. Motion away from the observer corresponds to motion away from the point-source of light. But the "motion" of the shadow itself on the screen (if it can be called that) is a size change, a minification.

This last method, the fifth listed, is vastly more flexible and powerful than the others. But how to use it for studies of perception is only now beginning to become clear. The art and technology of the "picture show" as the man in the street rightly names it have become fully and elaborately developed in modern times, but without any scientific discipline on which to base them. The production of moving displays with "animated" film, and by means of computer-controlled motions of a cathode-ray beam on the screen of an oscilloscope, are both complex elaborations of this method of projection (e.g., Green 1961 and Braunstein, 1962a and b).

Experiments on the Kinetic Depth Effect, or Stereokinesis.

Musatti (1924) demonstrated many years ago that a drawing composed of circles or ellipses which looked flat when stationary would go into depth when it underwent an orbital motion on a turntable. Everybody knew that a pair of flat forms having binocular disparity would go into depth when they were looked at in a stereoscope, but the idea of flat drawing being given depth by motion was surprising. Musatti called it the stereokinetic phenomenon.

The fact seemed to be that certain motions in the frontal plane could generate a perception of motion in depth. That would be consistent with Helmholtz's idea that apparent motions on the retina could combine to give the experience of a real motion in space, the latter being of an entirely different sort from the former. Ten years later Metzger (1934) reported what he called "appearances of depth in moving fields" and much later Wallach described what he called the "kinetic depth effect" (Wallach and O'Connell, 1953). No one imagined that a moving volume could be perceived directly, the motion and the volume at the same time, for they assumed that retinal sensations were the necessary basis of perception.

Wallach's kinetic depth effect is obtained when the shadow of a configuration made of bent wire is projected on a translucent screen and observed from the other side. Without motion the lines appear flat, as if drawn on the screen. But when the wire-object is turned the disposition of the wires in space becomes evident. The shift from picture to moving bent wires is very striking. Why should this occur? Wallach's formula was that the flat pattern went into depth when the lines on the screen changed in both direction and length concurrently (Wallach and O'Connell, 1953; Wallach, Weisz, and Adams, 1956).

This formula is not very illuminating. A better one was being worked out at about that time by Johansson (1950, 1964). It was something like this: if a set of several separate motions in the frontal plane can be <u>resolved</u> into some single motion of a rigid volume then this rigid motion will be perceived in depth. This formula is reminiscent of one of Wertheimer's laws of the supposed organization of sensory elements in the brain, the law of "common fate." It says that a collection of spots will be grouped to form a gestalt if they <u>move in the same way</u>. But Wertheimer never said exactly what he meant by "the same way."

Johansson's experiments were carried out at first with moving spots or lines projected on a translucent screen. But he later used a set of luminous elements on the screen of a cathode ray tube, which could be programmed to move in any direction, up, down, right, and left. He used vector analysis to determine the "common motion" in the cluster of elements. If the motions were "coherent," or if the cluster were coherent under motion, the elements would be perceived as an object in depth instead of a mere frontal pattern. They would appear to be a rigidly connected set of elements like a three-dimensional lattice in space or a polyhedron of solid geometry.

The hypothesis that individual sensory elements are grouped or made to cohere in the process of perception is an axiom of Gestalt theory. It is assumed that sensations are the necessary basis of perception. If it were not for the process of organization the individual sensations of motion would yield individual perceptions of object motion in the frontal plane. The theory of organization with reference to motion is adopted by Metzger (1953) as well as by Johansson (1950). But there is another theoretical possibility, namely that an optical transformation which is already coherent does not have to be made coherent in the process of perception; it is simply picked up.

Experiments with Progressive Magnification or Minification

The first results that began to suggest a direct perception of motion in depth were those of Schiff, Caviness, and Gibson in 1962. A point-source shadow-projector was used with a large translucent screen six feet square and with the point of observation close to the screen. A small dark silhouette at the center of the screen can be magnified over an interval of several seconds until it fills the screen. The observer sees an indefinite object coming at him and coming up to his face. He gets an experience that might justly be called visual collision. Without any mechanical contact the information for optical contact has been provided. The observer has no sensation of touch, but he blinks his eyes and may duck or dodge involuntarily. It seemed to me that this optical change, whatever it was, should be considered a "stimulus" for the blink reflex as much as a puff of air to the cornea of the eye should be. But it was surely not a stimulus in the ordinary meaning of the term. It was an optical expansion or magnification of a visual solid angle toward its theoretical limit of 180°.

Experiment showed that the size and the distance of the virtual object were indefinite but that its approach was perfectly definite. After the shadow filled the screen the virtual object seemed to be "here," at zero distance. It did not look like a shadow on the screen but an object in the sky. The object in fact came out of the screen. This was only to be expected for, by the laws of natural perspective, the closer an object comes to the point of observation, the closer its solid angle will be to a hemisphere of the ambient array.

There seemed to be a direct perception of an event that could only be described as *approach-of-something*. This perception was not based on a sensation of expansion or enlargement. Observers reported that the object did *not* seem to get larger like a rubber balloon, and that they did not notice the increasing size of the shadow as such unless the magnification was quite slow. The object appeared to be rigid, not elastic.

The magnification of the visual solid angle of an object normally *accelerates* as it approaches the limit of a hemispheric angle, as the object comes up to the eye. The accelerated portion of this sequence was called "looming" by Schiff *et al.* (1962). It specifies *impending collision*, and the *rate* of magnification is proportional to the *imminence* of the collision. Schiff (1965) adapted the looming apparatus to test the behavior of animals. He used monkeys, kittens, chicks, frogs, and fiddler crabs. All of them showed avoidance behavior or withdrawal analogous to the ducking or dodging of the human observer. As a control, the animals were presented with minification of the shadow, that is, the temporal reverse of magnification. The animals showed either no response or one that could be interpreted as curosity. Presumably what they saw was something going away in the distance but nothing that threatens danger or affords injury. When the screen was simply darkened (or lightened) the animals did not respond. And, of course, the unchanging silhouette on the screen caused no response.

The flinching of the human observer in this experiment usually extinguished after a few repetitions, but that of the animals mostly did not. However, although the human behavior changed, the human perception did not, that is, the awareness of *something approaching* did not extinguish with repetition. The perception evidently did not depend on the learning of a conditioned withdrawal response reinforced by mechanical collision.

In other experiments it was established that when the magnification of the shadow was not symmetrical but skewed, the animal (a crab) dodged appropriately to the left or right according as the path of the virtual object would be to the right or left of its position (Schiff, 1965, pp. 16-18). The human observer sees something approaching but approaching "that" point of observation instead of "this" point of observation, and he can judge how far the ghostly object would pass by him on the right or left. Presumably it is this sort of optical information that one uses in dodging a thrown rock (or catching a thrown ball, for that matter).

The fact that an arthropod like a fiddler crab behaved as if it perceived the same event as the vertebrate animals and the human observers was very suggestive. The crab does not have a camera eye nor a retinal image, and retinal image optics cannot be applied to it. But ecological optics works very well for the compound eye, for it is constructed of tubes pointing in different directions (Gibson, 1966, p. 164).

Experiments with Progressive Transformations

In geometry the magnification or minification of a form is sometimes called a size transformation (or a similarity transformation). But the ordinary meaning of the term is change of form, and the most familiar transformation is a perspective transformation. In the theory of perspective drawing, (what I designate as artificial perspective) it is called foreshortening. It is the parameter of transformation that converts a rectangle into a trapezoid when the rectangular surface is slanted away from the frontal plane. If a progressive transformation was a "stimulus" for space perception, as I thought (Gibson, 1957) then it was more fundamental than the kinetic depth effect and one should carry out a proper psychophysical experiment with this slant transformation. I was still thinking of slant as a basic variable in the perception of layout, and I still had in mind all the experiments that had been done on the perceiving of a constant form with varying slant, the puzzle of form constancy. I was still assuming vaguely that the perceiving of "forms," whatever they were, was basic to other kinds of perceiving.

So my wife and I collaborated in an investigation of what people see with a systematic variation of the amount of foreshortening, using the shadow projection apparatus (Gibson and Gibson, 1957). The shadow

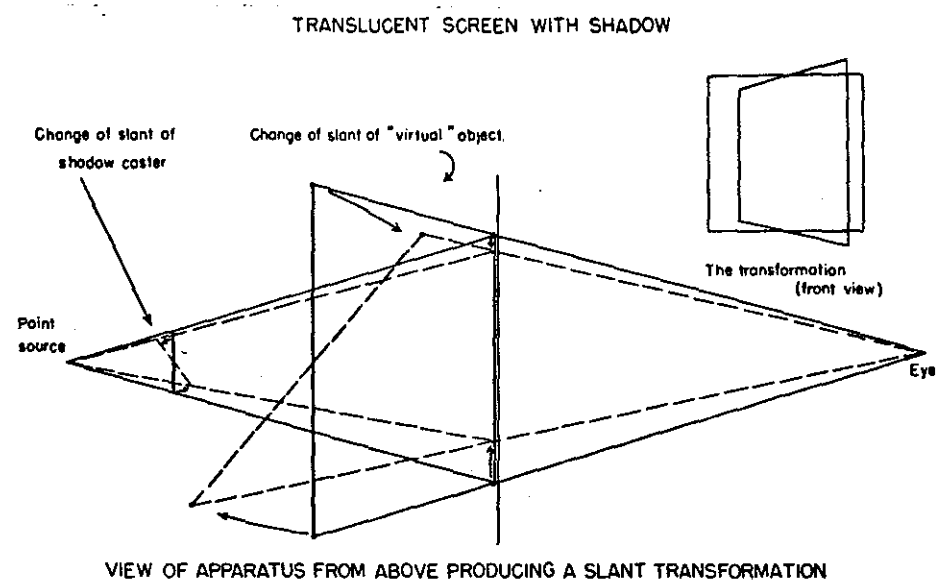

Fig. 1. from Gibson and Gibson, 1957. The shadow transformer.

projected on the screen was either a regular form (a square), a regular texture (a square of squares), an irregular form (ameboid shape), or an irregular texture (a potato-shaped group of small ameboid shapes). Each of these silhouettes underwent cycles of transformation, the shadow caster being turned back and forth through an angle that varied from 15° to 70°. The observer had to indicate the amount of change of slant he perceived, using an adjustable protractor.

All subjects without exception perceived the changing slant of an unchanging rigid surface. It was not an object, to be sure, only the face of an object, a sheet, but its shape was definite and it was not in the least elastic. It simply turned back and forth. One could say that the shadow on the screen was squeezed or compressed if one paid attention to it, but not the surface. There was no difference between the regular and the irregular silhouettes in this respect. The angle of the change of slant could be judged with considerable accuracy. The regular patterns, however, did not show more accuracy than the irregular, and there was no difference between what I called the forms and the textures.

These results did not fit with the traditional concepts of form and depth perception. They were upsetting. They implied that a certain change of form could yield a constant form with a change of slant, but this is surely a muddle of thought. Evidently the meaning of the term form is slippery and, if so, it is nonsense to talk about form perception (Gibson, 1951). What emerged over time during the cycles of change was a distinctive object. The hypothesis that began to suggest itself was that an object is specified by invariants under transformation. Far from being forms, these invariants are quite "formless"; they are invariants of structure. Presumably the four different surfaces in this experiment were specified by different invariants under foreshortening and the different changes of slant were specified at the same time by different amounts of foreshortening.

An optical transformation, then, was not a set of optical motions, nor was it a cause of depth perception. It was a single, global, lawful change in the array that specified both an unchanging object and its changing position.

The puzzle of phenomenal rigidity. It began to be clear that the heart of the problem lay in the perception of rigidity, and the information to specify rigidity, not in the perception of form and of depth. Could it be that certain definable transformations in the optic array were specific to rigid motions and that others specified non-rigid motions? More precisely the hypothesis would be that certain invariants specified rigidity and that other invariants specified elasticity. This line of thinking had great promise. The elastic bending of a sheet or a stick preserves connectivity but not proportionality. So does the stretching of a sheet or stick. But the breaking of it does not even preserve connectivity, except in the broken parts. And the crumbling of a surface does not even preserve the surface which, by disintegrating, ceases to exist. The invariants in this hierarchy are linked both to the meaningful substances of the environment and to abstract mathematics.

What experiments are possible? It is not easy to think of a way to isolate and control an invariant. Fieandt and Gibson (1959) did a more modest experiment. They presented observers with the transformation of compressing followed by its inverse (stretching) and then the transformation of foreshortening followed by its inverse to see if observers would spontaneously notice the difference, and perceive an elastic event in the first case which gave way to a rigid event in the second case. They defined stretching as change in one dimension only, width or height but not both, as exemplified by square-into-rectangle. Foreshortening was exemplified by square-into-trapezoid, as in the Gibson and Gibson experiment described above (1957).

They projected on the translucent screen the shadow of an irregular elastic fishnet. This was stretched on a frame mounted between the point source and the screen. One end of the frame could be made to slide inward and outward or the whole frame could be turned back and forth. The frame was invisible and the texture filled the screen. The motions of the elements on the screen were very similar in the two cases. But observers had no difficulty in distinguishing between the virtual surface in the two cases, elastic in the first and rigid in the second.

Johansson (1964) studied the effects of changing the height and width of a rectangle in a highly ingenious way. He generated a luminous figure on an oscilloscope screen with independent control of its height and width. He could stretch and then compress either dimension in repeated cycles. When both dimensions were increased or decreased at the same time he got magnification and minification, which yielded clear perception of a rigid object approaching and then receding. But he was interested in elastic motion. So he made the cycles of changing height and width out of phase. But he did not then obtain perceptions of the elastic motions of a variable rectangle as one might expect. Instead there was a strong tendency to see a virtual rectangular object with <u>three</u> parameters of rigid motion, not two, an object turning on a vertical axis, turning on a horizontal axis, and moving forward and backward, <u>all at the same time in different cycles</u>.

We do not yet know the exact basis for the perception of rigidity-elasticity although research is progressing at both Uppsala in Sweden and at Cornell in the U.S.A. They are curious and interesting experiments that have already produced some surprising discoveries.

An Experiment on the Perception of Separation in Depth

What information specifies the connectedness of an object, its unbroken character? The Gestalt theorists had emphasized the unity or coherence of the parts of a <u>form</u> but it began to be evident that the unity or coherence of a <u>substance</u> was a more basic fact. How do we see the <u>singleness</u> of a detached object, that is, its separation from other objects? A single object has a topologically "closed" surface, defined as a substance completely surrounded by the medium or, in mathematical terms, as a surface which returns upon itself. A line drawn on such a surface will eventually connect with itself. The object can be moved without breaking the continuity of its surface with the ground. Its substance is separated from adjacent substances by air or, at the very least, a "crack." One object becomes two only when its substance has been ruptured. How do we see this unbroken connectedness?

The first experiment to suggest that this basic fact might be specified optically was one by Gibson, Gibson, Smith, and Flock (1959). It was supposed to be an experiment on motion parallax and depth perception but it turned out to be an experiment on the perception of separation in depth. The point-source shadow projector was set up to throw on the screen two random textures intermixed and filling the screen. Actually there were two transparent sheets of glass each sprinkled with

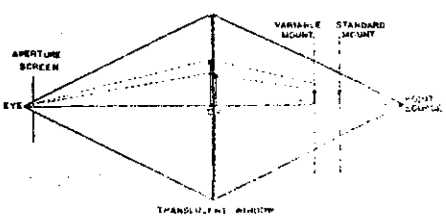

Fig. 1 from Gibson et al., 1959. The shadow projector viewed from above. In a unit of time, the shadow of a spot at the center of the standard mount sweeps through a certain angle and that of a corresponding spot on the variable mount sweeps through a lesser angle, as shown. The two mounts roll on the same carriage. If they are close together, there is no difference in angular velocity, but as the variable mount is positioned farther from the point source and closer to the screen, the angular velocity of its shadow decreases. With this apparatus, it can decrease to about one half of the angular velocity of the standard. By trigonometry, the ratio of the lesser (V) to the greater (S) angular velocity is equal to the inverse of the ratio of the distances of their respective mounts from the point source. In the diagram above, it is about 0.7.

talcum powder. This kind of texture yields the perception of a surface but not one whose elements are geometrical forms. The phenomenal surface is coherent and continuous but without lines, contours, or definite spots. It looks like the surface of a plaster wall or a cloud.

The two shadow-casters could be either motionless or moving. When motionless or when moving across the window at nearly the same speed only one virtual surface was perceived. But when there was a difference in speed between the two optical textures there resulted a splitting of the surface in two, a separation in depth. The perception was of twoness instead of oneness but not of two forms instead of one. It was as if the formerly coherent surface had become layered. The striking fact was that although this separation was "in depth" the difference in depth was equivocal. The faster motion was not necessarily seen in front of the other as the law of motion parallax would predict. The surface in front had to appear semitransparent, of course, but every now and then the front-back relation between the two surfaces would spontaneously be reversed.

Wherein lay the information for this splitting? One half of the interspersed elements of texture moved with the same velocity and the other half moved with a different velocity. But the important fact was that the two sets did not move in the same way. More exactly, there had been a permutation of the adjacent order of the texture elements. When some caught up with and passed others the adjacent order was destroyed. The permutation was not complete, to be sure, for each set of elements preserved adjacent order but the original connectivity had been destroyed. Hence the phenomenal continuity of the original surface gave way to the perception of two continuous surfaces, the nearest being transparent (Gibson, Gibson, Smith and Flock, 1959, 45ff.). Thus the available information in an optic array for continuity could be described as the preservation of adjacent order, or the absence of its permutation.

A permutation of adjacent order is a mathematically more radical change than a transformation that leaves adjacent order invariant. A size transformation and the rigid transformation of foreshortening, as well as the non-rigid transformation of stretching, leave order invariant.

A still more radical change than permutation is possible, however, and this was suggested by the next experiment. It is a change that subtracts elements of the array on one side of a contour or adds them on, and I have called it progressive deletion or accretion of structure. But this belongs in the next section.

V. THE APPREHENSION OF HIDDEN SURFACES

The most radical implication of experiments on the perception of the layout of the environment is the paradoxical assertion that hidden surfaces are in some sense perceived. At a given fixed point of observation some parts of the total layout have visual solid angles and the remainder do not, by the laws of natural perspective. Some surfaces are "in sight" and all the others are "out of sight." A hidden surface is one without a corresponding solid angle; it has no perspective representation in the array at that point. But hiddenness is temporary. As the point of observation moves (or as an object moves) what was hidden becomes unhidden and what was unhidden becomes hidden. Any surface has a solid angle in the ambient array at some point of observation. Whatever goes out of sight with a given movement will come into sight with the reverse movement and whatever comes into sight with a given movement will go out of sight with the reverse. Over time, therefore, the hidden and the unhidden interchange. The change from hidden to unhidden is reversible, and the hidden surfaces are thus connected with the unhidden. To apprehend the layout of surfaces at a temporary point of observation implies the ability to perceive surfaces that are hidden at that point.

Traditional theories of vision are only concerned with unhidden surfaces. They begin with the assumption that what is seen, properly speaking, is no more than a patchwork of colors in the visual field. They take for granted a fixed point of observation. If they recognize the existance of the far side of each object and the background that extends behind each object traditional theories do not recognize it as a problem for perception. If hidden surfaces are apprehended this is a problem for the theory of imagination or memory, or it is a matter of knowledge, or it involves the development of the concept of "object-permanence." It is true that the empiricist theory of depth perception appeals vaguely to cues for the perception of "solidity" and a special cue of "superposition." The gestalt theory of figural perception asserts that the ground is seen to extend behind the figure without interruption. Traditional theories may be said, therefore, to hint at the perception of hidden surfaces but not to face up to the problem.

The theory of the perception of layout, in contrast, ought to consider this paradox at the outset. For it takes for granted a moving point of observation, not a stationary point. Locomotions of the observer and motions of objects are typical, not exceptional. In normal visual perception surfaces are continually going out of and coming into sight. The connection between the hidden and the unhidden, the reversible interchange, should be a central concept of the new theory. But this was not clear to me at first, and it did not become clear until a series of experiments forced the development of a quite new idea, that of the occluding edge. The occluding edges of the world are the boundaries between the unhidden and the hidden surfaces, the lines of separation between them and at the same time the connections between them.

The Discovery of the Occluding Edge

The notion of depth at an edge is not especially novel and is not a very radical departure from the traditional theories of depth perception. But the notion of an occluding edge is both novel and radical. The visual cliff described in Part II of this report is an instance of depth at an edge. Another case of it is the experiment I described on the perception of an object that stands out from the ground when vision is either binocular or unfrozen but lies flat on the ground when vision is monocular and fixed. In both these cases there is occlusion as well as depth but this fact was not understood at the time. The important fact about an occluding edge is not the depth, the third dimension, but the seeing of one surface behind another. This fact went unrecognized because it seems paradoxical. It contradicts the ancient and unquestioned dogma that two things cannot be seen in the same direction, that is, on the same line of sight. But if one thing can be seen behind another the hidden thing must be seen in some sense of that ambiguous term.

The notion of an occluding edge was partly inspired by certain experiments on what Michotte called "covering" or "screening" (Michotte, Thines, and Crabbe, 1964) but it was first clearly conceived in a motion picture film entitled The change from visible to invisible: a study of optical transitions (Gibson, 1968) and an article by Gibson, Kaplan, Reynolds, and Wheeler (1969) together with an experimental paper by Kaplan (1969).

The hypothesis was that an occluding edge in the environment is specified by a reversible optical transition in the array. This transition can be experimentally displayed with an experimental motion picture, that is, it can be isolated and controlled. The optical transition in question is the progressive deletion (or accretion) of optical structure on one side of a contour with preservation of structure on the other side. Note that this transition is not a transformation mathematically since it involves gain or loss of components and the array after transition does not correspond to the array before transition.

The display for this experiment consisted of a set of motion picture shots made by single-frame photography. But instead of drawings, (as with ordinary animated films) photographs were taken of a randomly textured paper. Successive frames were modified by paper-cutting. No contour was ever visible on any single frame but progressive decrements of the texture were produced on one side of an invisible line by cutting off thin slices of the paper successively. Increments of the texture could be obtained by reversing the film. This particular kind of subtracting of structure (or adding of it) had not previously been produced in a visual display so far as we knew.

Kaplan then demonstrated (1969) that what observers saw with these displays was in fact an occluding edge. One surface was always concealing (or revealing) another. Deletion always caused the perception of covering and accretion always caused the perception of uncovering. The surface going out of sight was not seen to go out of existence and the surface coming into sight was not seen to come into existence; it was seen to go behind or come from behind.

When the display was frozen (by stopping the film) the edge disappeared and a wholly continuous surface took its place; when the film was started again the edge reappeared. But the "motion" of the display had nothing to do with the phenomenal edge; what counted was accretion or deletion of texture and whether it was on one side or the other.

The results of this experiment were unusual in that there were no uncertainties of perception or judgment. There was no guessing as in the usual psychophysical experiment. It seems possible to conclude that the occluding edge and the whole complex of surface phenomena connected with it are specified by the optical transition described and that all observers can pick up this optical information.

These results seem to contradict the doctrine that two surfaces cannot be seen in the same direction because only one sensation can come from the same retinal point. A surface that was progressively covered was phenomenally "there" after being covered and a surface that was progressively uncovered was phenomenally "there" before being uncovered. The perception of hiddenness was entailed in the perception of the occluding edge, and the optical information to specify the edge also specified the hidden surface. If this information was picked up sensations would have nothing to do with the perception.

The full implications of these facts remain to be worked out but they are upsetting for traditional theories, not only the theories of space and motion perception but also the theories of memory and imagination (Gibson, 1966). If perception is based on information and if information consists of invariants, then the appeal to memories and images in order to explain sense perception is not necessary.

VI CONCLUSIONS

1. The "layout" of surfaces relative to one another and relative to the observer is what gets perceived. To say that "space" is perceived can only refer to the medium in which an observer lives and gets about.

2. The old distinction between two-dimensional and three-dimensional perception is false.

3. The perception of "surfaciness" (substantiality) depends on the proximity to one another of contrasts or contours on the optic array. It also depends on the preservation over time of the adjacent order of these discontinuities.

4. For an extended open terrain the rule is that there are equal amounts of texture for equal amounts of surface. The available optical information for amount of surface is amount of optical texture. The perception of sizes and distances on the ground surface probably depends on the pickup of such information.

5. Conclusions about perception should not be drawn from experiments with monocular vision at a fixed point of observation. Such laboratory experiments are ecologically invalid.

6. The visual perception of the layout of the environment cannot be studied without taking into account visual kinesthesis. Thus the perception of the world and the perception of the self go together and only occur over time.

7. The laboratory experiments on sensations of motion in the visual field are not the only way to study the perception of events in the environment. Event perception can be treated as the perception of changing surface layouts. In that case the information consists of local disturbances of structure in the ambient optic array.

8. The perception of the "layout" of the environment implies a sort of perception of hidden surfaces connected to the unhidden surfaces at occluding edges. It is based on the interchange between hidden and unhidden during locomotion of the observer.

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INDEX OF TECHNICAL REPORTS

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MOTION PICTURE FILMS

In addition to the above publications about a dozen motion picture films have been produced with the support of the contract, four of which have been made available for distribution by the Psychological Cinema Register of University Park, Pennsylvania (16mm films for rental or purchase).

1. Optical Motions and Transformations as Stimuli for Visual Perception, 1957
2. Further Experiments on Optical Motion and Visual Depth, 1958
3. The Change from Visible to Invisible: A Study of Optical Transitions, 1966
4. Reversible and Non-Reversible Events, 1970

UNPUBLISHED PhD DISSERTATIONS

Some of the experimental research carried out under this contract has not yet been written up in a form suitable for publication in a scientific journal, or not fully written up, but only in the form of a doctoral thesis in the Cornell University Library. There are five such unpublished reports.

1. James Caviness, 1964, Visual and Tactual Perception of Solid Shape
2. Horace Reynolds, 1967, The Perception of Hidden Motion
3. James Farber, 1972, The Effects of Angular Magnification on Perception
4. Harold Sedgewick, 1973, The Visible Horizon
5. Hubert Dolezal, 1975, Adaptation to Up-Down Reversal of the Field of View