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ECOLOGICALLY STIMULATING COGNITIVE PSYCHOLOGY: GIBSONIAN PERSPECTIVES[1]

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STIMULATION AND COGNITIVE PROCESSES

On the face of it, James Gibson's approach to perception is not a likely candidate for concern in a volume devoted to problems in cognitive psychology. There is no need to argue for including perception itself under the heading of cognitive psychology, but Gibson's brand of perception is another matter. His most distinguished research efforts have focused on analyzing stimuli; and the data from his studies have been used to promulgate a theory of *direct* perception—two seemingly very uncognitive enterprises. Gibson, as anyone familiar with him knows, will never present testable processing proposals that could be recognized as normal cognitive psychology. Yet I shall offer his position, and some research on kinetic depth perception guided by it, as not only appropriate for cognitive psychologists to consider but featuring considerations without which cognitive psychology would be certainly incomplete and probably unsuccessful.

In current practice, it is the *processes* associated with a problem which tend to determine its assignment to a psychological category. Sensation, perception, and cognition are labels which suggest a scale of increasingly complex operations performed by the organism on stimuli as they journey from the peripheral receptors to the cortex, and are eventually transformed to a state of "knowledge." Ulric

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Neisser, whose *Cognitive Psychology* (1967) stands as the closest thing to a manifesto in the field, defines cognition as follows:

> As used here, the term "cognition" refers to all the processes by which the sensory input is transformed, reduced, elaborated, stored, recovered and used. It is concerned with these processes even when they operate in the absence of relevant stimulation, as in images and hallucinations. Such terms as *sensation, perception, imagery, retention, recall, problem-solving,* and *thinking,* among others, refer to hypothetical stages or aspects of cognition [Neisser, 1967, p. 4, my emphasis].

In the developed common usage the inclusion of phenomena under the heading of cognition usually depended on associating them with more or less "complicated" processing concepts. Thus, in showing the role of memory and attention in domains previously counted as more or less "perceptual" (e.g., metacontrast), Neisser could bring new topics into cognitive psychology. Phenomena previously regarded as "sensational," such as pure tones, have been elevated to perceptual or cognitive status as a result of findings which show that the listener must do more integrative work on the input stimulus than previously thought in order to experience what was formerly believed to correspond directly to the excitation of receptors (Creel, Boomsliter, & Powers, 1970). The new cognitive literature shows that relating a topic to memory (and to attentional processes in particular) can guarantee admission into the cognitive empire. The reader may sense that, with processing issues guiding the grouping of phenomena, old topics studied under old categories will soon be carved up properly as the new army of cognitivists cuts through the camouflage of traditional subdivisions to attack the vital centers of information processing. However, the enthusiasm for studying the processes that transform input from the world into perception (or, in the opinion of some, knowledge) of the world has been matched by scorn for the stimulation on which the processing is based.

This dual attitude of exalting the mind's processing power and denigrating the riches of external stimuli unites modern cognitive psychologists with their predecessors in epistemology throughout history, at least since the rejection of the belief that copies of objects emanate from them and "fly" to us for immediate apprehension. This attitude is richly overdetermined (see Gibson, 1960); but, in general, it is a consequence of sharply demarcating man as knower from the things he knows. The division of scientific labor has followed these lines, with geometers and physicists studying the stimulus on one hand; and psychologists, philosophers, physiologists, etc., studying the understanding of stimuli on the other hand. The problem of how man (or any organism) knows his world belongs to the second group, whose representatives take the results achieved by the first group as their starting point – as one should if the division of labor is proper. In the case of less concerned or sophisticated epistemologists, commonsense views of stimuli rather than the views of physics and geometry might be used. In either case, the person asking how knowledge of the world is acquired does not himself ordinarily question the nature of the variables of stimulation available from the world.

The unanimity concerning where to begin work on the problem of knowledge (exemplified by visual perception throughout this chapter) qualifies as one of the more significant achievements of interdisciplinary cooperation in the history of psychology. Geometry was successfully developed and applied by the use of ideal points as basic units; physics found these concepts adequate for optics, and physiology forged the necessary link in the causal chain from world to beast by discovering units that appeared to respond systematically to the physicists' variables (see the work of Bell, Müller, and Volta described in Boring, 1942; Gibson, 1960). These developments reinforced one another sufficiently to leave no doubt that the analysis of the "light to the eye" was in good hands. One might say that the challenges for explaining knowledge of the world really began after the light got into the eye; hence the problems of epistemology were written on the retina.

But these introductory remarks have been vague and assertive. Some representatives of the attitudes toward external stimulation to which I have alluded deserve to be quoted in detail.

In his *New Theory of Vision* of 1709 Bishop Berkeley gave the puzzle of depth its present form on the foundation of Molyneux's Premise (Molyneux, significantly enough, published the first text on optics in English), which obviously takes its givens from perspective geometry: "For distance of itself, is not to be perceived; for 'tis a line (or a length) presented to our eye with its end toward us, which must therefore be only a *point,* and that is invisible [cited by Pastore, 1971, p. 68]." Pastore compares this initial state to that of a blindfolded man being touched by the end of a pole and asked to judge how long it is. This seems a perfectly adequate place to begin perceptual investigations if we regard the retina as the picture plane onto which the world's light rays are projected. One surely receives only the points of the rays directed endwise toward one's retina. But then the distance that one does experience must come through some means. For Berkeley, these means are primarily association with touch and, secondarily, the sensations of convergence and accommodation of the eyes. Therefore perception is accomplished by adding to and correcting the stimulation so that, in this case, a three-dimensional world can be perceived from a two-dimensional stimulus.

Many modern theorists make similar assumptions about what *cannot* be in stimuli. Koffka asserted that, "Since the mosaic of proximal stimulation possesses no unity, the unity within our behavioral world cannot be explained by a corresponding unity in the proximal stimulation [1935, p. 84]."

Boring described the function of perception as that of transforming "*chaotic sense experience* into the relative stability of permanent objects . . .," then proclaimed that "Perhaps the greatest perceptual achievement of this organism is the way in which it receives on bidimensional retinas optical projections of the tridimensional world, losing, it would seem, all the tridimensionality, and then, taking immediate physiological account of the disparity of binocular parallax and other clues when they are available, instantaneously *puts the solid object together again* in perception, recovering the tridimensionality of the real object which had seemed irrevocably lost [in Leibowitz, 1965, p. 85, my emphasis]."

A major figure in current perceptual psychology, Richard Gregory, recently described his position as one which supposes

> that perceptions are constructed, by complex brain processes, from *fleeting fragmentary scraps* of data signalled by the senses and drawn from the brain's memory banks – themselves constructions from *snippets of the past.* On this view, normal everyday

> perceptions are not selections of reality but are rather imaginative constructions – fictions – based . . . more on the stored past than on the present. On this view *all perceptions are essentially fictions:* fictions based on past experience selected by present sensory data. Current sensory data (or stimuli) are *simply not adequate* directly to control behavior in familiar situations . . . . The fact is that sensory inputs are not continuously required or available, and so we cannot be dealing with a pure input-output system [Gregory, 1972, p. 707, my emphasis].

Finally, returning to Neisser, "We have no direct, immediate access to the world, nor to any of its properties. The ancient theory of *eidola,* which supposed that faint copies of objects can enter the mind directly, must be rejected [Neisser, 1967, p. 3]." And at the end of his introduction he asserted, "No shorter route seems to do justice to the *vicissitudes* of the input, and to the continuously creative processes by which the world of experience is constructed [p. 5, author's emphasis]."

Whenever one assumes that stimulation is meager but that experience and accomplishment are rich, some rather fancy supplement to the stimulus must be provided. Examples of such supplementation were included in the above quotations. The customary model of supplementation has been logic, which has impressed many thinkers with its power to derive significant conclusions from very simple premises and rules of inference. Thus we have Helmholtz's famous appeal to "unconscious inference." A thinker who hesitates to be as specific as those who appeal to rational inference might settle for a term like "construction," which carries the connotations of building great structures from minor constituents without an early commitment to the type of detail involved. This provides another way of accounting for the inclusion of perceptual problems in the domain of cognitive psychology. Only processes common in discussions of obviously cognitive topics like problem solving and logical inference could possibly "bridge the gap between sensory stimulation and our experience of external objects [Gregory, 1972, p. 707]."

Boring has been fairly explicit in this regard:

> An object can be regarded as an as-if theory of experience. Experience would be as it is if there were permanent objects. And the properties of objects thus become generalizations about experience. So perception, in getting back of experience to the objects, is performing even in primitive man and the animals the same function that science performs in man's civilization. As the purpose of scientific theories is economy of thought, so the purpose of perception is economy of thinking [Boring, 1946, pp. 84-85].

In the 1950's, Jerome Bruner was advocating the "new look" perception, reflecting his work in cognition, where the essential event was an act of categorization.

> The use of cues in inferring the categorical identity of a perceived object . . . is as much a feature of perception as the sensory stuff from which percepts are made. What is interesting about the nature of the inference from cue to identity in perception is that *it is in no sense different from other kinds of categorical inferences based on defining attributes* [Bruner, 1957, p. 123, my emphasis].

Paul Kolers remarked, "In resolving disparities, the visual system uses mechanisms that seem to be markedly similar to those usually reported only at a more ideational level [Kolers & Pomerantz, 1971, p. 108]."

Let me close with Gregory's recent echo of Boring's analogy between perception and science:

> The notion of perceptions as predictive hypotheses going beyond available data is alien and suspect to many physiologists. Cognitive concepts appear unnecessary . . . but there are surely strong reasons for believing cognitive concepts to be necessary . . . . More basically, what are essentially cognitive concepts are very familiar in all the sciences, but hidden under a different guise – the method of science.
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> Generalizations and hypotheses are vital to organized science, for the same reasons they are essential for brains handling data in terms of external objects . . . . Scientific observations have little or no power without related generalizations and hypotheses. Cognitive concepts are surely not alien to science, when seen as the brain's (relatively crude) strategies for discovering the world from limited data—which is very much the basic problem of all science. Scientific observations without hypotheses are surely as powerless as an eye without a brain's ability to relate data to possible realities – effectively blind [Gregory, 1972, p. 707].

I would suggest again, then, that the cognitive-constructivist approaches to perception go hand in hand with a commitment to the essential poverty of stimulation, and the belief that the essential features of perception bear no similarity to the available stimulation.

GIBSON'S ALTERNATIVE

Framework. James Gibson has recognized the fundamental reciprocity between characterizations of stimulation and characterizations of processing. For him, perceptual knowledge is first and foremost an adaptive relation between perceived and perceiver. A full understanding of perceptual knowledge requires simultaneously apprehending both terms of the relation. The answer to the question of how perception is possible requires showing how the nature of the perceiver's environment makes it possible, as well as the nature of the perceiver. There *is* an environment to be known, one whose properties determine what there is to be perceived and, indeed, what there has always been to be perceived so far as the evolution of any particular species is concerned. We know, from the varieties of adaptive behavior observed in organisms (as well as from their continued survival as species), that whatever perceptual processes they are using work pretty well to keep individuals in contact with their surroundings.

Gibson claims that geometers and physicists have not provided the analyses of stimuli that are relevant to perception – not from diabolical motives, but because they have never been forced to think about the structure of environments. The usual analyses of stimuli based on geometric points and lines leave a little-noticed gap between these ideal basic entities and the world of perceived surfaces, a gap that is every bit as problematic as the widely discussed one between emotive vocalizations and referential speech. In Gibson's estimation the psychologist who tries to build processing models that do justice to what we know organisms can do in the world is doomed to fail – not because he lacks the cleverness to build brilliant models, but because he is working with the wrong raw materials.

Gibson's significance for cognitive psychology lies not only in his having clearly stated what I have tried to say above, but also in his attempt to construct an alternative approach to the study of stimuli. He has made two key steps. The first is to keep in mind the physical environment to be perceived. A theorist should ask whether or not his own scheme of optical analysis could ever carry the visual information to keep an organism in contact with the significant properties of its environment. The second key step is to realize that a visual "stimulus" (or stimulus in any other modality) could easily be a "higher order" relation defined over spatial and temporal changes of pattern in the appropriate medium. Gibson has shown that when one searches, it does seem possible to discover variables of stimulation which are not only specific to significant environmental properties (e.g., slant of a surface), but also are invariant across normal environmental changes of pattern such as those arising from a change of viewpoint.

Because of the stimulus-processor reciprocity in the perceptual knowledge relation, significant changes in the description of effective stimuli will necessarily require changes in the job description and subsequent modeling of perceptual processing. Such established fields as psychophysics and anatomy would be included in the realms-for-rethinking (see Gibson, 1966). To the extent that stimulus properties which correspond to environmental properties can be found, the character of proposed processes would reasonably change from the intellectualized detective-like inferences described in the first section to the information intake processes Gibson has discussed in so many places.

A further consequence of discovering stimulus properties specific to environmental properties is the justification of Gibson's philosophical direct realism. This position is meant to emphasize the observation that animals are adaptive, rather than fallible beasts. How difficult is it to accept such a position? Admittedly organisms make mistakes sometimes–like traveling an arduous route to drink at a mirage–which could be fatal to an individual–and they could be said to have mistaken appearance for reality. But how wrong was our mistaken drinker? He did not take the sand under his feet to be water. Could he not have been mistaken because the desert heat reflected light in the same way as an oasis? Then perhaps there was no optical basis for any discrimination between mirage and oasis–in which case the "illusion" was a *necessary* consequence of the animal's being attuned to the invariant information specifying water. Furthermore, such events could not be typical in one's life as a whole. Surely there is a long list of properties and events that an organism must perceive accurately and reliably for his species to have survived. This is Gibson's initial stance, and I fail to see how even the staunchest constructivist could quarrel with it. Would the constructivist ever assert that he was seeking "the mechanisms of perception," but that the ones he might find could conceivably be *mal*adaptive mechanisms? But if the mechanisms are held to be adaptive, would not the job description of what they do (which includes a structural description of their input and surely must precede how they work) contain as full a description as possible of the environment to be perceived?

The environment. The first task in Gibson's approach to visual perception is to

ask *what there is to be perceived.* His working hypothesis is that there are descriptions of light which do in fact correspond to significant properties of environments. For such correspondences to be possible he must assume (*a*) that organization does exist outside of an organism, both in the physical world of opaque surfaces and in the light structured by multiple reflections from these surfaces in a medium; and (*b*) that environmental properties at a given level of analysis can be uniquely and invariantly specified at a comparable level in this reflected light. Working on (*b*) constitutes the enterprise Gibson calls ecological optics. If fruitful, these assumptions would provide a case for rejecting the more commonly held assumptions of stimulus-environment caprice.

Physical properties of an environment which might have correspondences in light could be suggested endlessly. One could get indefinitely microscopic or macroscopic. Gibson maintains that the levels of analysis of highest priority should be ecological. Atoms and their motion are much too fine-grained an analysis of matter, and photons of light (or the geometric ideal points and lines) are correspondingly too fine-grained for analyzing light. By the same token cosmic properties of the universe's substances and light present far too coarse a level of analysis for one to expect any immediate ecological significance. One should begin, in Gibson's view, to consider the world as a concrete layout of surfaces and events of changing surface arrangement occurring in a medium which contains multiply reflected light (rather than light idealized as emanating from radiant points). Thus the environment to be considered should be the one where organisms perambulate and light reverberates.

The properties of surfaces which are significant for organisms have always made the highest claim on Gibson's classifying and analyzing capabilities. He typically tries to find basic categories which allow one to exhaustively classify all events or properties at that level of analysis. For example, surfaces may be rigid or nonrigid. There is certainly no ready-made analysis coming from any other field that tells us how to discriminate rigid from nonrigid changes in the light structured by such events and objects. It is not an early problem to be tackled with geometry if one is looking to begin with simple problems. In fact the knowledge that there are nonrigid changes in the world which we perceive as rigid (figures in movies) and rigid changes which are seen as nonrigid (ceiling fans seen at eye level; a hardboiled egg rolling endwise; wire cubes under certain strobe conditions) has discouraged people even from considering the possibility of the kind of correspondence Gibson seeks. Nevertheless he would argue that the rigid-nonrigid distinction is fundamental enough in the world to make us keep in mind the possibility that the above countercases are not really countercases, but are instead special cases which actually structure light in the manner specified even though the distal causes are otherwise. After all, the terrain stretching to the horizon is rigid. It is fundamental for all terrestrial animals in the sense that it provides (or "affords," in Gibson's terminology) support for all the animal's activities. Many other obstacles and conveniences in an organism's environment are arrangements of rigid surfaces and are, in fact, obstacles or conveniences by their very rigidity. Besides support, rigid surfaces can afford collision and thus harm organisms when contact is made too forcefully. Animals for their part are nonrigid and surely distinguishable from rigid objects. Thus the distinction is important enough on the face of it, and the evidence of animals' honoring it reliable enough for us to expect that in some way it is always expressed in the light.

The light and information. The key concept relating the structure of light to the structure of an environment for Gibson is optical information. To say that light contains information means that it is structured and that the structure can uniquely specify environmental properties. Structure is defined not over parameters of radiant light rays such as intensity, but over relations of *change* in intensity. Many significant features of an environment, perhaps all if the theory works, may be specified by virtue of their reflecting light in characteristically different patterns, which give rise to corresponding patterns of light transition or contrast. Such features of surfaces include material composition, which determines a specific texture, pattern of pigmentation, and location and orientation relative to other surfaces. Each property gives rise to differential reflection of light. As long as the patterns in light are defined over contrasts–which are determined by the various types of environmental discontinuity–the patterns will certainly be invariant over changes in amount and direction of illumination. And, whenever a relationship can be defined over ordered adjacencies of contrast, the structure will be preserved across changes in point of view since light travels in straight lines in a medium.

Gibson's basic unit of pattern, which he contrasts with the retinal image in other theories, is the optic array. This refers to the pattern of discontinuities in reflected light reverberating in a medium that converges to a potential point of observation. The optic array is the pattern of ordered adjacent contrasts in the full 360° sphere surrounding such potential points of observation. A sphere of light around a point of observation which contains no contrasts is said to contain no structure, hence no information. This would be an ambient array, but not an ambient *optic* array. Gibson argues that the unusual experiences subjects in *Ganzfeld* experiments have reported arise because the subjects have been confronted with an unchanging, homogeneous array which fails to satisfy the necessary conditions for being a stimulus for an eye, i.e., containing abrupt changes in intensity.

The fundamental structure of an ambient optic array should be defined with respect to a moving point of observation. Gibson points out that this is the natural state of any organism relative to his optical environment, and this approach allows us to avoid possible puzzles which could result from trying to relate static analyses to one another in order to synthesize dynamic relations.

The notion of discontinuity constitutes the core of every level of analysis in ecological optics. For example, some type of discontinuity must exist to distinguish surface from *Ganzfeld* (e.g., Gibson, Purdy, & Lawrence, 1955). A regular change of discontinuity may specify a unique surface texture, and can function as a unit on a new level of analysis. The texture itself may in turn change regularly in density and velocity relative to a moving point of observation, thereby specifying a surface at a slant relative to the moving point. In each case a pattern is formed by uniformities of change based on lower-level changes. This is an example of the very important notion of higher-order information. One of Gibson's most significant

insights, making his program feasible, was to recognize that structure can be defined over ordered units at many levels. Consequently, failures to discover invariant optical structure corresponding to invariant environmental properties are just as likely to be a result of using the wrong relations to describe the stimulus as of selecting the wrong environmental properties to characterize. For instance, a set of variables might give rise to different outcomes when they are used to compute differences but give rise to invariant outcomes when ratios are computed; or, say, ratios of ratios. In Gibson's view the first requirement for detecting invariant properties of events is the existence of the corresponding invariant structure in the light – but the level of definition required to define the relation which is invariant across overall changes in illumination and point of view is a matter for empirical research. In Gibson's analyses, such relations would be possible wherever systematic changes in environmental properties determine systematic changes in the pattern of light contrasts constituting an optic array. Surely it would be hard to deny that some set of such direct relationships exists. One must realize, moreover, that if this possibility is acknowledged–that certain properties of an environment determine the structure of reflected light–this relationship is by no means analogous to a relation between symbols and symbolized: Rather it is a projective relationship.[2] There is no arbitrary code connecting the two, and no information loss analogous to information theoretic accounts of transmitted communications. It then follows that the optical information is available to any organism which possesses the processing structure required to compute the relational invariants specifying persistent environmental properties and the complementary variants specifying changing properties. But it is the optic array available for exploration, not a retinal image, that is the basis for perception.

Precedents. Perception, seen as an organism-environment relation, surely involves both the processing of information and the information to be processed. When we proceed on this basis, we can be primarily interested in processes if we like, but we should also pay heed to the environmental context to provide helpful constraints for limiting the class of devices we consider. In other words environmental considerations can regulate research in an area where regulation is typically accomplished only by models one is already familiar with. There are some illustrious precedents in science for using a "context of constraint" as an essential tool, if not an explanatory concept.

Invariance principles in physics are one example. Laws of nature which were fairly well understood for many years were later shown to form organized systems in their own right. When the invariance laws were made explicit, many physicists realized that they had been implicitly guided by them for some time. Recognizing

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[2] Projective, however, here does not mean there are 1-1 correspondences between points at any level of analysis. One should not forget that an optic array may be textured by all the environment's discontinuities at once, e.g., material composition, pigmentation, pattern of illumination (including highlights and shadows), etc. What is "projected" from environment to light is an overall pattern of corresponding relations which are composed of identical nested invariants of ordered discontinuities.

that unknown laws of nature should satisfy appropriate invariance laws has helped guide the selection of hypotheses physicists formulated and experiments they conducted (Wigner, 1970).

Closer to the concerns of evolving, perceiving organisms is the work of Charles Darwin. His best-established scientific competence at the time he was working out the process of evolution was geology. He was, in fact, working actively in geology and more or less founding biogeography during his voyage on the *Beagle.* Michael Ghiselin (1969) assigns a key role to this "context of constraint" in shaping Darwin's evolutionary insights.

Finally, one of the best-loved works in science. D'Arcy Thompson's *On Growth and Form* (1969), showed how homologies across organisms could be established by pointing out that tentatively homologous structures were in fact the result of identical growth processes which had taken place in the framework of differing patterns of stress. Thompson demonstrated that the stress patterns which formed the environments of the morphological structures of interest were far simpler to understand than the relations among the detailed descriptions of features themselves; and in fact such an account proved completely adequate as a type of explanation as well as a tool for discovery.

Much more important, each of these cases is also an example of a very significant unification in its field. Where before there had been a hodge-podge of more or less "understood" local processes, there came to be a comprehensive, unifying system including each of them. Not only was it possible to discover laws of nature to unify observed phenomena, but it was also possible to show that there was another level of higher law which the laws of nature themselves obeyed. This is very much the type of function Gibson's approach could fulfill (see Shaw, McIntyre, & Mace, in press). If there is more unity to be discovered than is currently reflected in perceptual theory, it would seem that a "top-down" strategy such as Gibson's is more likely to discover it than less explicit, more local strategies.

Today investigators are conducting research on processing devices through automata theory. This discipline well recognizes that specifying an appropriate environment is a necessary aspect of defining any computing device. The relationship is integral enough that there is no clear distinction between the processor and its environment.

> The question of where a particular machine ends and its environment begins can be settled only by a convention of definition . . . .When we cannot grasp a system as a whole, we try to find divisions such that we can understand each part separately, and also understand (in that framework) how they interact. When we make such a division for purpose of analysis, each part is treated in turn as the machine of interest and the remainder as its environment. One cannot usefully make such divisions completely arbitrary because an unnatural division of a system into "parts" will not yield to any reasonable analysis [Minsky, 1967, p.19].

With more specifically evolutionary problems in mind, John Holland stresses that

> An adaptive system should seek and exploit environmental regularities–opportunities to depart from enumerative behavior – and should proceed at random only when nothing better is possible. The process of adaptation is essentially that of locating and using such regularities [Holland, 1962, p. 334].

An approach to perception which ignores the structure of environmental stimulation would at best operate under a distinct disadvantage by not searching in every possible place for assistance in making progress toward understanding. At worst it would be condemned to failure.

GIBSON ON "DEPTH"

Contrast between loose organization of "depth cues" and the grammar of ecological optics. Depth perception offers perhaps the best content area in which to highlight the contrast between Gibson and constructive perceptual theorists. Traditionally, the problem of depth, whether in the form of distance of a point from an observer or of solidity of an object, has been a special problem for vision. The Molyneux Premise stated earlier is a representative beginning. Generally it was felt that the flatness of the retinal projection surface was the main obstacle to understanding. How does one extract three dimensions from two? Explanations drew first on the feelings of accommodation and convergence of the eyes as primary cues for the distance of focused objects, but also recognized that depth was perceived at distances too great for the primary cues to be effective, as in pictures. Hence properties of patterns in the light which yielded depth were also considered even if they were regarded as secondary criteria. Pictorial cues were formulated by artists to indicate techniques which could be employed to give a three-dimensional appearance to a two-dimensional canvas. These secondary criteria include superposition, linear perspective, light and shade, aerial perspective, and the seemingly greater length in filled than in empty distance. Motion parallax is often added as a cue which is present in changing three-dimensional patterns, but of course not present in pictures. None of the pictorial cues can be an unambiguous depth index under normal viewing conditions, because we also know that paintings, which use such cues, are really flat (except under very special conditions). In linear perspective, it is known that a single static projection is geometrically ambiguous. Nevertheless these cues ordinarily function well enough for us to judge where the artist has placed all his forms in three-dimensional space. The artist's effects have often been used as evidence of the intermediary enrichment of cognitive-like processes which "add" or "infer" the third dimension.[3]

Interestingly, the pictorial depth cues, formulated as low-level empirical generalizations to assist the practice of art, have received little additional systematic analysis. No one has tried to formulate any higher laws that might be found in them. They seem very much like rules of grammar before the transformationalists,

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[3] Gestalt psychologists were not stymied by the two-dimensional quality of the retina. They argued that the retina was just one stage of processing the total pattern and could not be divorced from the total field forces, which were three-dimensional. Rather than studying patterns intensively to find all the optical information that might exist among them, however, they studied optical patterns from their role in creating the field forces required to correspond to the perception of three-dimensional objects (see Koffka, 1935). Thus Gestalt theorists were satisfied with having a loosely structured list of cues because the field forces forged the bonds of organization (see the earlier Koffka quotation).

rules which might assert an intuitive connection between types of sentences, such as active and passive, but would search for no formal relation. Depth cues are similar to one another to the extent that they indicate depth. Yet researchers who investigate them imply that the cues just happen to be associated with depth in the world and might as well be otherwise independent. According to this view, a well-defined depth situation is one containing combinations of redundant cues. Ecological optics, on the other hand, would not settle for such a casual state of affairs. Like the transformational grammarian, the ecological optician suspects that "cues" which all yield the same type of judgment share the same higher-order depth information—which would, in turn, relate each cue to the others through an explicit rule system. There is no *a priori* reason why such relations should not exist. Indeed, partial successes in Gibsonian research during the last few years give us much promise of discovering them. Thus following the linguistic analogy, we could think of the ecological optics component of Gibsonian work as a search for the grammar of the light to the eye. This is correct as long as we recognize that the relationships relevant to perception picked up by organisms are not arbitrary (as a linguistic grammar could be), but are uniquely determined by the environment structuring the light. The processing implications are clear. The more elegantly structured and interrelated we can show environmental information to be, the more parsimonious could be the processing strategies evolved in such an environment. The more structure uncovered, the less one needs to posit imagination operating on fragmentary data to explain perception.

Gibson asks that we postpone appealing to abstract spaces which cannot be perceived and to mediational processes akin to translating codes until we have considered the possibility that the question is one of perceiving surfaces and their layout or arrangement; then we should consider very seriously the possibility that different arrangements of surfaces, in *any* direction, determine different patterns in an optic array.

One of Gibson's best-known contributions to perception is his demonstration of the role of texture gradients in depth perception. The fundamental terrain of the earth (which is opaque and textured) determines an optic array gradient of texture which proceeds regularly from a coarse to a fine grain as one describes a visual field from bottom to top. More generally, such texture gradients can specify the slant of any surface relative to a point of observation (although one actually needs more than one optic array sample for this specification). Gibson thought at one time that the gradient concept might prove powerful enough to account for most depth phenomena. For example, since the ground is the usual background against which organisms perceive objects, it was possible to account for "distance" of various objects directly by considering the fundamental background texture occluded by an object on the ground. Solid objects are composed of textured surfaces whose gradients relative to some observer would be particular to their shape and slant. Objects separated in depth from one another as well as from the observer could be specified with reference to the texture gradient of that fundamental frame of reference, the ground, by noticing the regular intervening texture transitions. Thus slant information could also provide separation information. Many ambiguous laboratory situations could be accounted for by the absence of such a specifying background; and where there were biases in the absence of stimulation, the biases tended to be in a direction consistent with the assumption that the ground was still taken as background. For example, objects higher in the visual field tend to look farther away than objects lower in the field in the absence of background texture, as would be the case if the ground were background. On the other hand, against a ceiling, the higher object would be closer.[4]

With texture gradient information, Gibson first showed that one could make strides toward unifying previously disparate types of structure in the light (see Gibson, 1950, for a full account). Although the texture gradient concept is not as comprehensive as once thought, it has provided the insight that depth may be treated as surface layout. Gibson did indicate relations which can be included in more general analyses; for the final result, whatever it looks like, promises to be some form of dynamic transformation on texture—which would surely include velocity gradients specific to the projection of surface texture to moving points of observation and yielding slant. Many common observations (some to be reviewed below) make it clear, however, that gradient information is by no means necessary for perceiving a "three-dimensional" layout of surfaces. Gibson himself maintains that what we must initially consider is a cluttered layout in which a primary fact of kinetic arrays is that textures cover and uncover each other as objects are concealed from and revealed to a point of observation. His earlier mistake, he asserts, was the attempt to formulate optical information for an uncluttered, open layout. The question we want to keep in mind, in looking at a certain class of depth effects, is this: Can we find other ways to describe optical information which approach the comprehensiveness once thought to characterize the gradient concept?

LOOKING FOR MINIMAL INFORMATION FOR SEPARATION OF SURFACES "IN DEPTH"

A research strategy described fully in Mace (1971) addressed the specific problem of finding the minimal optical information capable of specifying separation of surfaces in depth. One could then ask if such a situation contained *necessary* information for depth separation wherever it might occur, and perhaps necessary for all cases where depth is perceived. Thus an empirical groundwork would be laid for a unified theory of surface layout.

The first task was finding a case which the researcher could intuitively judge to contain the least possible known depth information that was, at the same time, analyzable. Such a demonstration would be a focal point for the experimental

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[4] Explaining the depth cue of height on the picture plane by appealing to an assumptive ground plane might appear to be a constructivist ploy. Gibson would probably not be too disturbed, however, for the kind of experience-based "memory" assumed to be operating here is the entire visual system, developed through the "experience" of evolution to locate objects with reference to the ever-present ground plane. Gibson certainly would deny that the effectiveness of a cue such as height in the picture plane could be accounted for by particular experiences which observers stored on various occasions, then subsequently recalled from their storage-bin memory for reference in an impoverished experimental situation.

analysis required to isolate the effective information in the display. This information might be found to apply to all more complicated situations (if the proper simplification had been performed), and hence could be said to be the minimal necessary and sufficient information for separation in depth.

Principles of simplification and kinetic variable. The first task was to classify the concrete cases that might share the same minimal information for separation but could still be distinguished from one another. Ideally we might find an ordering of cases in which a hierarchy of "depth" qualities is reflected in a concomitant hierarchy of optic array information. One such hierarchy that guided these investigation begins with the *Ganzfeld,* the case of an unstructured ambient array which specifies nothing, or, "no-surface." There is no support for the concrete experience of a surface anywhere (Gibson & Waddell, 1952). With a complication of the *Ganzfeld* providing structure through some contrast, a surface at a distance from the observer can be specified (Gibson & Dibble, 1952). Here one can distinguish two subcases which are always possible in a discussion of separation. First, the perception could be reliable only for the fact of separation itself and not its amount. If could be that additional information is required to allow amount to be perceived reliably, in which case one might call the first case indeterminate and the second determinate distance.

Further possibilities arise if one considers two surfaces, each in the same direction from the point of observation, but at different distances from it. At least two properties might be determinate or indeterminate—the order of the surfaces as well as the amount of their separation already mentioned. Order refers to front-back relations relative to the point of observation. If one knows which is in front and which is behind, order is determinate. If one knows only that there are two surfaces and that they are at different distances, then order is indeterminate. These possible specifications may be applied to cases of opaque surfaces, where the depth relations would be at an occluding edge, and transparent surfaces, where one surface is seen through another. If there are surfaces in a particular environment, and information for them is contained in the optic array, then the surface layout could be said to be completely specified in the light.

Now it becomes clearer what one might be looking for if one declared an interest in finding the minimal information for separation of at least two surfaces in depth. The weakest display to qualify would be one where separation in depth alone was reliably presented in the light without specification of order or degree of separation.

Since it is well known that no pictorial cues for depth, including perspective, can specify a layout unambigously, only displays that offer more than one sample of an optic array will be considered, i.e., kinetic arrays. (Binocular arrays offer more than one sample, but not all organisms have focusable conjugate binocular systems, whereas all organisms do obtain different samples of the array by actively exploring their environments.)

Now I shall describe the hunt for a minimal case and then some of the initial experimental analyses of the demonstration selected. Like so many enterprises in perception, it can begin with Helmholtz.

> In walking along, the objects that are at rest by the wayside stay behind us; that is, they appear to glide past us in our field of view in the opposite direction to that in which we are advancing. More distant objects do the same way, only more slowly, while very remote bodies like the stars maintain their permanent positions in the field of view, provided the direction of the head and body keep in the same directions. Evidently, under these circumstances, the apparent angular velocities of objects in the field of view will be inversely proportional to their real distances away; and, consequently, safe conclusions can be drawn as to the real distance of the body from its apparent angular velocity [Helmholtz, 1962, p. 295].

Thus Helmholtz described the effectiveness of the "depth cue," motion parallax. It is a situation in which the geometry is plain and constitutes a case of potential information in the optic array. Is it detected as such? If organisms determined depth in this manner, they would be computing the differential angular velocities and comparing them for all points (geometrical points?) in their visual field (unless one were to add a filtering device which could select only "strategic" points). This would obviously be a processing hypothesis based on the assumed variable of stimulation. I shall return to the motion parallax cue later.

Two dimensions of change and projective transformations. One of the best-known monocular kinetic depth phenomena is Wallach's kinetic depth effect (Wallach & O'Connell, 1953). This is actually concerned with the perception of rigidly rotating objects rather than separation of surfaces in depth, but it also represents one of the earliest attempts to specify minimally effective information for depth and it does so in a way that would be equally appropriate to Helmholtz's case in the woods. Wallach showed that the shadows of rotating geometric objects (solids, wire figures, and straight rods), when observed on a back-projection screen, provided sufficient optical information for the correct identification of the rotating three-dimensional object. Shadows projected from the same objects when static were not sufficient to specify correctly their three-dimensional shape. Thus the changes in form of the two-dimensional shadow were perceived as projections of rigid rotations of an object rather than elastic changes of form. For Wallach the essential condition for this effect is a change in at least two dimensions. He states that "shadows whose only deformation consists in an expansion and contraction in one dimension will look flat," while "shadows which display contour lines that change their direction and their length will appear as turning solid forms [Wallach & O'Connell, 1953, p. 209]."

In 1934, Metzger (1953) demonstrated that two-dimensional shadows back-projected on a screen from a series of vertical pegs revolving on a turntable provided sufficient information for the perception of their dynamic three-dimensional configuration. Not only were shadows of the top and bottom of the pegs masked from view, but the light source was far enough away from the pegs to approximate an isometric or so-called "parallel" projection. Therefore the shadows of the individual pegs were not seen to undergo perspective changes in either length or width as the pegs revolved from a near to a far position relative to the light source. Since no changes in either size or shape of the shadows were perceived, these cannot be the two variables of optical information responsible for the perceived three-dimensionality. Wallach recognized this problem but dismissed Metzger's case because his interest was in the kinetic depth effect, which he regarded as stronger and more stable than Metzger's phenomenon (Wallach & O'Connell, 1953). White and Meuser (1960) investigated the Metzger rotating bars in some detail; they concluded that it was quite an effective depth situation and used it as evidence that simultaneous changes in two dimensions were not necessary for the perception of depth.

There is, however, still a source of projective information that must be considered as a possible explanation for Wallach's and Metzger's kinetic depth phenomena, namely, harmonic motion. Motion projected from a rotating object onto a plane surface (e.g., shadowgraph of rotating pegs) is called harmonic; i.e., given any projected point on the plane of projection, it periodically moves back and forth on a linear path, accelerating when moving inward from the end-points of its path and decelerating when moving outward from the midpoint of its path. Oscilloscope watchers have known for some years that certain combinations of wave patterns appear to specify rigid surfaces rotating in depth (see, for example, Fisichelli, 1946). These are called Lissajous patterns. All the kinetic depth phenomena discussed so far involve projected harmonic motion. But such motion is not a necessary condition for specifying the separation of surfaces in depth. This becomes clear when we consider still other situations in which kinetic depth effects are obtained, although projective information has been systematically excluded.

Gibson, Gibson, Smith, & Flock (1959) sought to test rigorously the long-accepted idea that *motion parallax* was sufficient information for perceiving depth. Their stimulus display consisted of moving shadows presented to a subject on a back-projection screen. The projected shadows were created by shining a point source of light through two transparent surfaces irregularly covered with talcum powder. These powdered surfaces were rigidly yoked on a common carriage. One surface was directly in front of the other along the observer's line of regard. When the display was static, the projection gave the impression of being a single surface of scattered dark spots. When the carriage was moved on a line perpendicular to the line of regard, however, the shadows projected from each powdered surface moved across the screen at different rates according to the geometry of motion parallax. When the texture motion disparities were large enough to be perceived easily, observers did not judge the order of depth consistently. Sometimes the faster-moving surface was seen as in front and sometimes the slower surface was in front. Thus, an example of an indeterminate order of depth effect was found. If the depth information used had been motion parallax, the faster-moving texture would have been seen in front at all times. Helmholtz was correct about his geometry and thus about the available information, but apparently not about the detected information. Therefore Gibson *et al.* concluded that *Ss* were not informed of the depth through motion parallax.

Gibson suggests that the optical information necessary to specify separation might be what he calls *topological breakage.* This concept refers to the kinetic margin separating two subsets of points which differ in texture elements sharing velocity vectors which are related in some principled way. The principle explaining the perceived coherence of texture elements into a single subset (i.e., an optical whole) was called "the law of common fate" by Gestalt psychologists. Simple examples would be flocks of flying birds, or platoons of marching soldiers. When texture elements sharing common fate are packed with sufficient density and aligned in the proper way, the optical wholes are seen as optical surfaces. Thus, a single optical surface is defined by texture elements having a common kinetic fate (i.e., proportional velocity vectors), while topological breakage, as a higher-order concept, is defined by subsets of texture elements having different kinetic fates. Gibson has also defined topological breakage as a disruption of adjacent order of texture units of one subset by another.

This is an extremely simple but powerful principle, since the information for topological coherence applies to all known cases in which kinetic depth phenomena are perceived. In this concept we seem to have an implicit principle of sufficient generality and logical necessity to account for separation of surfaces not only in three dimensions but in two dimensions as well. For instance, distinct coplanar textured surfaces which move in relatively contrary directions (i.e., that have different velocity vectors) appear to be separated by a margin of topological breakage (e.g., a crack of zero width). Notice that the Gibson *et al.* effect is an instance of transparent depth. One surface is perceived *through* another.

It would appear also that we could reach back and include the Metzger rotating pegs as an instance of the same type of transparent depth. For instance, the shadows from the pegs could just as well be projected from vertical semitransparent stripes painted on a piece of transparent plastic bent around the turntable to form a solid ring. This in turn suggests that harmonic motion might not only be unnecessary for depth in general, but for obtaining Metzger's effect as well.

The Gibson *et al.* experiment provides some very important clues to what might *not* be minimal and/or general conditions for the perception of depth. However, their experiment does not provide enough information to decide what other cases might be counted as instances of topological breakage. That is, we do not know how much or how little a pattern must be transformed for its elements to be considered as changing their adjacent order. Nor is it clear that the crucial information for separation in depth in their case could not be subsumed as a special case of occlusion information, already the most general kind of surface separation information in the literature.

Accretion/deletion of texture as information for occlusion. Whenever an opaque surface moves across another surface in depth it progressively subtracts the optical texture of the rear surface from the optic array. In the same manner, a surface which is uncovered after being behind an opaque surface is specified by the progressive addition of its optical texture to the optic array. Kaplan (1969) has demonstrated the effectiveness of this general type of information, the accretion or deletion of texture at a visual margin, for specifying separation in depth and direction of depth (that is, determinate depth) at an edge.

We know from work done in Michotte's laboratory (Michotte, Thinès, & Crabbé, 1964) that it is unnecessary to have textures on both sides of a margin to perceive determinate depth at an edge. A dark disk whose leading contour suddenly stops at a linear deletion boundary while the trailing contour continues to move is seen to disappear through a slot. Thus an occluding surface is specified in the transformation of the occluded surface alone. Having random texture on both sides of his optical margin gives Kaplan a more dramatic effect, however, for there is no discernible margin of any kind when there is no motion. This buttresses Kaplan's argument for the crucial role of kinetic information.

How could the Gibson *et al.* (1959), results be related to Kaplan's work? He achieved determinately ordered depth judgments, they did not. Fig. 1 suggests a possibility. A horizontally moving version of Fig. 1a is roughly what Kaplan presented, while Fig. 1b is like the Gibson *et al.* case. In the first case, one figure wipes out certain contours of the other; in the second, the figures wipe out one another's contours because there is no brightness difference between the intersecting and nonintersecting portions of the figures. This would almost guarantee indeterminate depth. One could also point out that there was no overall margin defined in the Gibson *et al.* pattern, only an array of mini-margins where each texture clump interacted with others. This lack of an overall margin could have contributed to the indeteminateness of the depth.

If some variation of Kaplan's accretion/deletion transformation were shown to be effective in this indeterminate depth case, it would be a very important finding, because it has already been shown to cover more cases than one might have thought at first glance. For instance, when an object approaches from a distance or we approach it, we see less and less background behind it. We also see less and less of the rear textures of the object itself. When the distance between ourselves and an object increases, background texture is accreted in the same manner that it was deleted in approach. Hence accretion/deletion can also specify approach and recession. Another instance is simply the turning of one's head to look around at the world. The direction we turn can be specified by the addition of texture and the direction we are moving away from by the subtraction of texture at the edges of our eyesockets and nose (Gibson, 1968). This could also be information for distinguishing ourselves from the world (Gibson, 1950).

To stretch his hypothesis further, Kaplan proposed a more general version, which stated that the information specifying depth at an edge is due to the *disparity of rates* of accretion or deletion at an optical margin. The texture with more accretion/deletion is behind. Then the previously described cases form a particular instance of this more general formula. That is, the preserved (occluding) texture has a rate of accretion/deletion equal to zero at the common margin.

a

b

Fig. 1. Two types of occlusion. a. One figure wiping out contours of the other. b. Figures mutually obscuring contours.

Kaplan tested the more general hypothesis by constructing displays with four different rate disparities, ranging from zero to a case where one texture was accreting or deleting at three times the rate of the other. Where accretion (or deletion) occurred simultaneously on both sides of the margin, there was no longer any information for an occluding edge, but rather a stationary margin from which (or into which) the textures were rolling, i.e., a crack.[5] Kaplan had other conditions which had no ready ecological interpretation. Ghostly margins seemed to be specified. Nevertheless there was a strong tendency for *Ss* to report the slower-changing texture as being in front for all conditions with the appropriate disparity. Thus there is a seductive unifying power in a notion as simple as the wiping away and adding of texture.

Limitations of Kaplan's formulations. Powerful as Kaplan's hypotheses appear, however, they work best *post hoc.* The most fundamental difficulty arises when we consider his second, more general hypothesis. To define a *rate* of accretion or deletion in order to calculate a disparity of rates requires that we be able to count texture elements affected per unit time. Kaplan used randomly distributed texture elements on either side of the margin so that any counting method would work, since each side had approximately the same configurations. But imagine that the texture units on one side of the margin are several times longer than on the other side and that the motion at the margin is across this length. Assume also that the distance between elements is identical for both textures, and that the speeds are the same. If the rate of accretion/deletion is defined over the *number* of elements added or subtracted per unit of time then the smaller texture should be seen as behind. But it is easy to imagine a "texture element" whose progressive addition or subtraction is readily apparent; enough so that it would give a Michotte-like "rabbit-hole" effect if seen in isolation. Would we still wish to maintain that accretion/deletion had to be defined over the *number* of whole units which come or go? If not, what would be a unit on this larger texture surface? The rate of accretion/deletion at a margin can be defined only when a general rule relating density of texture, size of texture units, and number of texture units can be defined rigorously.

Thus we should be alert to the possibilities of disambiguating the Gibson *et al.* optical information, for accretion/deletion is not so patent a candidate for expressing the generality of depth information as we might first think. It would be very satisfying if an adequate explanation of the Gibson *et al.* effect might be extended to Kaplan's cases and restore the trend toward unity that was lost when it was only beginning to form.

Relative motion alone. Another result in the Gibson *et al.* paper should receive more attention than it has: The investigators included a condition in which there was just a single opaque spot on each of the parallel transparent surfaces. One spot

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[5] It should not be assumed that simultaneous accretion or simultaneous deletion at a margin is necessary information for specifying a crack, since a contrary parallel rectilinear translation of texture is also sufficient to determine a margin without depth (i.e., a crack). Such a transformation can be termed a shearing of adjacent texture at a margin whose contour is defined by the direction of contrary translations.

was placed higher than the other, so that there was *no* occlusion information. Nevertheless subjects reported separation in depth when they saw the differential motion. Here, then, is an example of a depth effect carried by something other than the well-known variables that have been discussed. The spots are merely moving relative to one another. They are near each other but they do not intersect. This is an additional reason to suspect that the major effect in Gibson *et al.* does not depend upon occlusion. That possibility will be examined shortly.

Figural conditions for separation in depth. Up to this point the primary emphasis has been on the transformations shown to be sufficient to produce the perception of depth. However, as the difficulties of Kaplan's hypotheses begin to make clear, a truly general formulation of the optical information which specifies depth must take into consideration spatial structure as well as kinetic structure. An acceptable generality (even of a subset of depth cases) must rigorously relate figural and motion properties, just as Kaplan must relate size, number, and density of texture elements to generalize the applicability of the notion of rate of accretion or deletion of texture. Even when not discussing it explicitly, all the aforementioned studies require some specification of the spatial organization to which a transformation of interest will apply to produce depth. For example, some of the displays in Green (1958) showed that certain small, scattered groups of points were insufficient to carry the depth information of a projective transformation.

Some striking instances of the role of figural properties in depth separation were collected by Wallach, Weisz, & Adams (1956). For example, a solid figure-eight shape with no visible discontinuity within its margins appeared to separate into two circles when rotated on a turntable. The circles seemed to revolve independently of one another, one sliding *over* the other. Less symmetric figures such as arrows did not show this effect.

More generally, two types of factors must be considered in any instance of perceptual organization: those which produce coherence and those which produce separation; or information for continuity and discontinuity. For the moment we are most interested in separability or discontinuity in depth. Spatial and kinetic structure may, of course, contribute to either. Hence the necessity of relating the two to define the perceptual information in any event. Johansson (e.g., 1958) has been particularly explicit about specifying both synthesizing and dissociating types of perceptual information.

From speculation to research. We cannot proceed, however, to proposals for the proper segmentation of the optic array into organizing and separating-in-depth factors without certain data. Of all the cases reviewed to this point, the Gibson *et al.* situation stands out as particularly promising. Here a random texture that looks like a single "cloud," when transformed by a mere interpenetrating motion of two subsets of texture, separates perceptually into two noncoplanar surfaces which are not consistently ordered in depth. It appears simple in terms of the information involved as well as with respect to its perceptual consequence from the standpoint of the ecological optics hierarchy presented above; that is, it is an indeterminately ordered depth phenomenon.

What factors are necessary to obtain the perceived separation? Is occlusion required? A random texture was used. Does this contain some necessary spatial structure? The result of the two-spot condition suggests the possibility that neither occlusion nor any spatial structure beyond proximity is needed. But we should be extremely wary of generalizations from two spots to a set of spots or a *texture.* What are the kinetic structural conditions on the separability? Is one pattern of relative motion as good as another as long as one set of spots moves closer to another set? The general question to be posed, then, is: What spatial-kinetic patterns yield separability in depth of the sort obtained by Gibson, *et al.* (1959)?

Pilot demonstrations. The availability of a laboratory computer with an associated cathode ray tube made it possible to artificially structure a large number of patterns of theoretical interest.

The first pattern constructed presented the Gibson *et al.* situation with modifications suggested by the last section. Thus regular, rather than random, patterns were used and there was no occlusion when the two patterns intersected. The patterns are shown in Fig. 2. Each is composed of a lattice of dots all moving with exactly the same motion. Is that sufficient information to specify a surface? When the two patterns intersect without occlusion, would the surface characteristics (if there were any) disappear or maintain their integrity? The intersecting patterns could be considered as somewhat similar to groups of soldiers marching through each other. Then the effect would not be one of depth, for the points would be coplanar.

The actual result was striking and unambiguous. One pattern did indeed appear to be in front of the other. The judgments shifted, thereby indicating that the separation in depth was an indeterminate one, but the basic separation was undeniable. It was extremely difficult to see the points as coplanar. All persons who saw this pattern immediately described it without prompting as one thing moving in front of another and were surprised that there should be any problem, because the phenomenon was so apparent.

Clearly neither occlusion nor a random array is necessary to obtain separation in depth. What is necessary? Can variables be isolated which manipulate the perceived separation? Or will all patterns with *any* detectable motion disparities separate? The investigations to be described explored these possibilities.

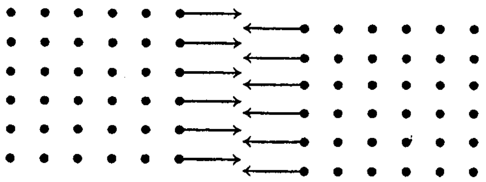


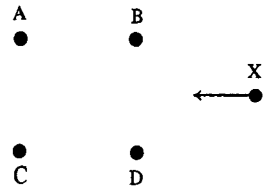
Fig. 2. Schematic representation of pattern used in pilot demonstration. Pattern was small enough relative to the CRT screen that all four margins of each were visible.

EXPERIMENTS

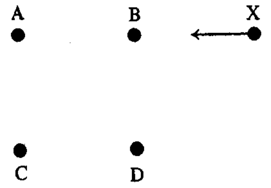
Four ways of modifying the demonstration pattern were selected for experimental analysis.

1. Optical margins or contour. One of the most important types of discontinuity in an optic array is determined by occluding edges in the world (Kaplan, 1969). Kaplan's patterns contained these edge-specifying margins, as did the primary demonstration pattern of Fig. 3. The former was generated by the accretion or deletion of texture, whereas margins in transparency situations result from brightness differences that supposedly could not be described as additions or subtractions of texture. The brightness differences of Fig. 3 are created by changes in texture density. The random textures of Gibson, *et al.* (1959) did not have margins. Hence we ask the question: what is the role of optical margins in the separability of *regular* patterns?

2. Phase. The language of periodic phenomena can be applied conveniently to both the spatial and the kinetic structure of patterns. The lattice patterns used, in these symmetry terms, could be described as the result of two independent translations (Coxeter, 1969). Two types of interaction between identical patterns were of interest. In the first, sections of which are shown in Fig. 3, the rows of one pattern moved directly between the rows of the other pattern. A pattern of this type was called spatially "out-of-phase." In the second type of pattern, naturally called "in-phase," the rows were collinear so that the patterns periodically intersected perfectly.



a. Out of phase.



b. In phase.

Fig. 3. Two degrees of "disruption of adjacent order." In a, only one adjacency is disrupted; in b, four adjacencies are disrupted.

The terms referring to phase reflect the frame of reference leading to the use of this variable in the design, but it is certainly not the only frame of reference that would make these patterns interesting. Gibson *et al.* (1959) discussed their separation effect in terms of the disruption of adjacent order of texture elements. If the latter is a proper description of separability information, would it not be plausible to assume that disrupting more adjacent orders would increase the separability effect? Figure 3 shows how "in-phase" and "out-of-phase" patterns disrupt adjacent collinear orders. A point moving "in phase" disrupts only the AB adjacency, while a point moving "out of phase" disrupts BD, AD, BC, and finally AC.

3. Type of motion. Three combinations of motions of the two patterns were selected to compare their depth-inducing capabilities with the other factors on depth-inducing capabilities. In the demonstration pattern, the two patterns moved across the display from opposite directions. Could there be a noticeably different effect if one of the patterns remained static, perhaps as a more stable frame of reference? Patterns of each type were used. The third type of motion was an oscillation of one lattice within another instead of a translation through it.

4. Direction of motion. It was observed in some demonstration patterns that the direction of one pattern's motion across another might make a difference for perceived separation. A lattice moving across an identical static lattice did not necessarily give symmetric perceptions. If the motions were diagonal, separation appeared to be better than with either vertical or horizontal motion.

Shaw (see Shaw *et al.*, in press) has explicitly incorporated the separating-in-depth capacity of diagonally moving rectangular lattices in a symmetry group analysis of these pattern effects. Briefly, he argues that diagonal motion can be viewed as a pattern discontinuity like an optical margin or an "out-of-phase" intersection. The essential assumption is that the spatial arrays are geometric lattices and thus by definition can be thought of as being generated by two independent translations whose basic units of translation are both discrete and proportional in size. Given the directions of these two translations, it can be shown mathematically that in general no motion in the third direction could be generated whose basic unit of translation is either the same size as or even a rational proportion of the bases of the other two—with the exception of a few isolated cases. The same vector basis could be preserved, however, if the display were resolved as a case of transparency, containing patterns at two distances from the observer instead of one. This appears to be a broadly applicable analysis which might be a strong candidate for being a necessary abstract condition that optical structure must satisfy in all cases of perceived depth.

This is a very important empirical juncture. The forms of discontinuity represented could all function as separability factors, but we have no idea how they compare or interact since none of them seems to have ever been manipulated before. The results of these comparisons should tell us a great deal about the direction a comprehensive theory of separability should take; that is, we shall know which instances we must try to include under a common characterization. Of course, the logical necessity for common factors to hold across depth situations cannot be concluded from this or any other set of experiments, since universal claims are only related to a summary of empirical findings. But insofar as common optical conditions can be found to hold across different depth phenomena, to that extent our confidence in the unified depth hypothesis will be strengthened.

Results

To distinguish the effects of each pattern, adults were asked to rate the quality of the separation in depth of each one on a 7-point scale after they had first spontaneously described the whole set of patterns.

Since some depth was reported in most patterns, it was evident that separability information was available. The effects obtained were, like the demonstration pattern, of transparent, indeterminate depth separation.

The most effective separability factor was diagonal motion of the rectangular lattices through one another–a finding consistent with impressions based on the pilot displays. This is the only effect that holds up under repeated experimentation with equal strength, and is the result that has received the most attention in the formulation of new theories (Shaw *et al.*, in press). What was not expected was the striking difference between horizontal and vertical patterns. Horizontal displays were judged better separated in depth than their vertical counterparts. This has been replicated enough to suggest further attention, but since it seems to require somewhat special conditions and since it is not at all clear where to fit it in theoretically it has not received as much attention as the diagonal effect.[6]

Out-of-phase patterns reliably increased the separability achieved by any particular in-phase pattern, but this gain was negligible in the presence of contour or diagonal motion. Type of motion used had no systematic bearing on separability. Contour played a more complicated role in separation, a role which has not been replicated since and, consequently, will be regarded as an ineffective factor.

Despite the fact that depth was reported at least once in all displays, the lattice with no margin which was moving vertically over another lattice in phase was rated by 6 of the 11 observers in the experiment as having *no depth separation at all* (a rating of 0 on the 0–6 scale). Thus mere relative motion of two subsets of dots, micro-accretion/deletion of texture elements, and the disruption of adjacent orders of elements *per se,* are each insufficient to produce depth. To say, then, that the elements of a set are adjacent is not enough to tell what transformations will provide the discontinuity necessary for separation in depth. A truly general definition of depth information must describe more structure in an array to establish clearly the nature of the continuity relative to which some depth-producing discontinuity can be specified.

Later experiments indicated that the effective "diagonality" must be defined in terms of the pattern's internal structure and not with respect to either an environmental or an observer's frame of reference. This was pointedly demonstrated in the finding that interpenetrating *random* (not lattice) patterns looked well separated regardless of the direction of their interaction. Shaw's analysis shows how certain symmetry considerations might handle these results. The analysis is stated in terms of the regular lattice case; but, since it depends on an interaction between the spatial and the kinetic structures of the display, it could well be generalizable to the finding with random patterns.

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[6] All lattices used in these experiments were rectangular, i.e., the vertical and horizontal spacings between points were not equal. A full replication of this 36-pattern study using square lattices (the "second experiment" mentioned above has used rectangular ones) showed that only the diagonal interaction significantly accounted for perceived differences in quality of separation. However, the horizontal patterns were still stronger than the vertical ones and this difference almost reached statistical significance. Hence the horizontal-vertical difference is probably not attributable solely to the rectangularity of the lattices in the experiment being described. The same experiment with the square arrays also failed to replicate the phase effect. There was only a small trend in favor of the out-of-phase patterns. Therefore diagonal interaction must be considered the only reliable effective factor enhancing separation here.

SUMMARY AND CONCLUSIONS

Assume it known that animals see surfaces and their layouts, including what have historically been called their depth relations. When proposing how this might be accomplished, from Gibson's point of view, we first try to ascertain the richness of available information; then, if we want to think about the types of processing which might be computing functions of this available information, we seek processes whose structure is compatible with the information putatively used. Theorists lik~~e those quoted initia~~lly have imagined processes which must produce the known riches of perceptual accomplishment from rather austere stimulation. That is, these theorists need enriching operations. The type of machinery that lends itself to such enriching operations seems to be that which stores what it can in memories, then operates on the stored material with various inferential procedures. This is one type of physical world–stimulus-processor compatibility. However, if the stimulation is thought to be rich and informative, then the structural complexity (which could still be great) of the processes that put one in touch with one's environment might be thought to derive from the necessity for structural compatibility with the available stimulus relations themselves – in the same way that a language processor must be compatible with the structure of the language being processed. The function of having wondrous perceptual processes would then be to find environmental structure rather than to make it up or to assume it on the basis of only suggestive evidence.

Gibson has argued in many places that viewing the function of perceptual processes as one of detecting available information removes the need for appealing to memory-store-plus-inference models of perception. He stresses that memories as such are not required but rather appropriately structured detection devices. Gibson by no means denies the existence of memory functions if we mean by a memory function that an organism's past is very much a part of his present. But Gibson does deny that memories of the sort that might be stored in the bins of a general-purpose storage area play a role in the perception of the significant properties of an organism's environment. In Gibson's detection of pattern invariants over time, the perception of surface layout and events is treated as one might treat the perception of a melody. Recognizing that there is a structure to be detected, in this case the melody, one does not then propose that the individual notes must be remembered in order to compare them with one another and then decide what the melody is on the basis of inferential processes. One must say, however, that the processor can detect structure over time and that this structure is in some sense compatible with the structure of the melody (but this does not commit one to a storage-and-retrieval model of melody detection).

As his alternative conceptualization of processing, Gibson has used the idea of resonance to portray the general type of activity he has in mind. Shaw and McIntyre's chapter in this volume and the recent writing of Karl Pribram on holographic models of memory represent the major progressive elaborations of the resonance model that exist in psychology to date. The resonance concept is one way of capturing what I previously called the structural compatibility between stimulus information and its processors.

The particular research this chapter described was addressed to the problem of specifying the available and used information for separation in depth. It was based on the assumption that certain yet-to-be-discovered abstract conditions are the same across all cases of depth separation. This assumption reflects faith in the unity of the world and of the organisms which have adapted to it. I would argue that for the time being this is heuristically the most fruitful assumption, even though it may be a difficult one to accept. The alternative assumption, that organisms have banks of very specialized detectors for specialized situations, such as one device (or even program) for computing depth from motion parallax, another for superposition, another for perspective, and so on, simply does not seem likely to motivate systematic research.

A selective review of the literature showed that there have been few attempts even to propose a type of depth information available in all instances of perceived separation of surfaces. Of the conditions offered, none was necessary. A demonstration case which did yield depth but apparently involved only the stimulus variable of disruption of adjacent orders of elements (also called topological breakage) in moving lattices was described. However, the insufficiency of topological breakage as an adequate description of separability information was demonstrated in a set of experiments which showed that the disruptions of adjacent order were effective only in certain pattern configurations.

The difficult problem at this point in the program is identical to that encountered in Kaplan's research on depth at occluding edges. It might be stated as the problem of finding a description of the optical events which generalizes the effects of transformations of texture (such as accretion/deletion) across arbitrary textures. It might be that a solution to the current problem would indeed be applicable to other cases of depth separation and hence be a candidate for the necessary and minimal depth information being sought. If these displays were easier to work with than other classes of patterns conceivably bearing the same information, then they would have served their purpose admirably. One might say that they would have proven ecologically relevant after all.

We are not lost in the woods without a compass. The symmetry approach described in Shaw *et al.* (in press) has the necessary abstract properties that we know a proper analytic scheme needs (e.g., naturally characterizing invariants across transformations and being able to deal with spatial and kinetic structures in a unitary way). In fact, symmetry principles have been proposed there as formal underpinnings for Gibson's work at the level of the basic epistemic relation between organism and environment. The same principles have also been applied to the research described in this paper and other key problems in event perception. In each case the analyses are sketches waiting for much more detail. But the symmetry framework has been fruitful enough to justify concerted efforts to develop it much further.

In general the psychologist must know what the effective stimulus is in a given situation. Contrary to past theory and practice, this is not merely a routine task of manipulating a few isolated stimulus "dimensions" to see which one the organism responds to. It is a task of hypothesizing and verifying stimulus structure which, like the task of writing grammars, demands as much cleverness in imagining possibilities and ways to test them as any processing problem. The particular research efforts described in this paper were meant to dramatize this point. They seem to be as interesting and as fruitful as any strictly process-oriented research being done. A great many problems, new and old, remain, but many seem solvable if we do the concerted theoretical and empirical work required.

REFERENCES

Boring, E. G. *Sensation and perception in the history of psychology.* New York: Appleton-Century-Crofts, 1942.

Boring, E. G. The perception of objects, *American Journal of Physics,* 1946, **14,** 99-107. Reprinted in H. Leibowitz, *Visual Perception.* New York: Macmillan, 1965.

Bruner, J. *On perceptual readiness. Psychological Review,* 1957, **64,** 123-152.

Coxeter, H. S. M. Introduction to geometry (2nd ed.) New York: Wiley, 1969.

Creel, W., Boomsliter, P., & Powers, S. Sensations of tone as perceptual forms. *Psychological Review,* 1970, **77,** 534-545.

Fisichelli, V. R. Effect on rotational axis and dimensional variations on the reversals of apparent movement in Lissajous figures. *American Journal of Psychology,* 1946, **59,** 669–675.

Ghiselin, M. T. *The triumph of the Darwinian method.* Berkeley: University of California Press, 1969.

Gibson, E. J., Gibson, J. J., Smith, O. W., & Flock, H. R. Motion parallax as a determinant of perceived depth. *Journal of Experimental Psychology,* 1959, **58,** 40-51.

Gibson, J. J. *The perception of the visual world.* Boston: Houghton Mifflin, 1966.

Gibson, J. J. The concept of the stimulus in psychology. *American Psychologist,* 1960, **15,** 694-703.

Gibson, J. J. *The senses considered as perceptual systems.* Boston: Houghton Mifflin, 1966.

Gibson, J. J. An outline of experiments on the direct perception of surface layout. Unpublished manuscript, Cornell University, 1968.

Gibson, J. J., & Dibble, F. Exploratory experiments on the stimulus conditions for the perception of a visual surface. *Journal of Experimental Psychology,* 1952, **43,** 414-419.

Gibson, J. J., Purdy, J., & Lawrence, L. A method of controlling stimulation for the study of space perception: The optical tunnel. *Journal of Experimental Psychology,* 1955, **50,** 1-14.

Gibson, J. J., & Waddell, D. Homogeneous retinal stimulation and visual perception. *American Journal of Psychology,* 1952, **65,** 263-270.

Green, B. F., Jr. Some conditions for the occurrence of the kinetic depth effect. *American Psychologist,* 1958, **13,** 406 (Abstract).

Gregory, R. L. Seeing as thinking. An active theory of perception. *London Times Literary Supplement,* June 23, 1972, 707-708.

Helmholtz, H. *Physiological optics.* (Edited by J. P. C. Southall) Vol. 3. New York: Dover, 1962.

Holland, J. Information processing in adaptive systems. *Excerpts Medica International Congress Series* No. 49. Information processing in the nervous systems. Proceedings of the 22nd International Union of Physiological sciences, 1962, 3,

Johansson, G. Rigidity, stability, and motion in perceptual space. *Acta Psychologica,* 1958, 13, 359-370.

Johansson, G. Perception of motion and changing form. *Scandinavian Journal of Psychology,* 1964, 5, 181-208.

Kaplan, G. Kinetic disruption of optical texture: The perception of depth at an edge. *Perception & Psychophysics,* 1969, 6, 193-198.

Koffka, K. *Principles of Gestalt psychology.* New York: Harcourt, Brace, 1935.

Kolers, P. A., & Pomerantz, J. R. Figural change in apparent motion. *Journal of Experimental Psychology*, 1971, 87, 99-108.

Leibowitz, H. *Visual perception.* New York: Macmillan, 1965.

Mace, W. An investigation of spatial and kinetic information for separation in depth using computer generated dot patterns. Unpublished doctoral dissertation, University of Minnesota, 1971.

Metzger, W. *Gesetze des Sehens.* Frankfort: Waldemar Kramer, 1953.

Michotte, A., Thinès, G., & Crabbé, G. Les compléments amodaux des structures perceptives. *Studia Psychologica.* Louvain: Publications Universitaires de Louvain, 1964.

Minsky, M. *Computation: Finite and infinite machines.* Englewood Cliffs, N. J.: Prentice-Hall, 1967.

Neisser, U. *Cognitive psychology,* New York: Appleton-Century-Crofts, 1967.

Pastore, N. *Selective history of theories of visual perception: 1650-1950.* London: Oxford University Press, 1971.

Shaw, R. E., McIntyre, M., & Mace, W. The role of symmetry in event perception. In R. B. MacLeod and H. Pick (Eds.) *Studies in perception: Essays in honor of J. J. Gibson.* Ithaca, New York: Cornell University Press, in press.

Thompson, D. W. *On growth and form.* Cambridge: Cambridge University Press, 1968.

Wallach, H., & O'Connell, D. The kinetic depth effect. *Journal of Experimental Psychology,* 1953, 45, 205-217.

Wallach, H. Weisz, A., & Adams, P. Circles and derived figures in rotation. *American Journal of Psychology,* 1956, 69, 48-59.

White, B. W., & Meuser, G. Accuracy in reconstructing the arrangement of elements generating kinetic depth displays. *Journal of Experimental Psychology,* 1960, 60, 1-11.

Wigner, E. *Symmetries and reflections.* Cambridge: M.I.T. Press, 1970.