The International Society for Ecological Psychology

Volume 3 Number 4 NEWSLETTER June 1988

It has been quite a long time since the last newsletter. This reflects nothing except the time taken up to organize the journal. The role of the Newsletter has been to report the news, to keep members equally informed and, hopefully, stimulated. There never has been a formal schedule, but four times a year was the goal. We'll call this issue number 4 and begin Volume 4 with the next one. At the moment producing 4 newsletters and 4 issues of the journal per year is a daunting prospect. It might be possible when routines get established (nothing has been routine in the life of the Society yet), but for the immediate future the timing of each Newsletter will remain a surprise to us all. Thanks for your patience.

ELECTION

Eight people were elected to the Board at the meeting in Hartford last October. Current Board members, then, are:

'87–'89	'86–'88
Claudia Carello	Alan Costall
Carol Fowler	Eleanor Gibson
Walter Gerbino	James J. Jenkins
Claes von Hofsten	Ulric Neisser
William M. Mace	Anne D. Pick
Robert E. Shaw	Edward S. Reed
James Todd	Sverker Runeson
William H. Warren, Jr.	

JOURNAL

By now all of you should have received a brief Call for Papers from our publisher, Lawrence Erlbaum Associates. A longer brochure will follow in the fall. The primary message is that *we are open for business.* Papers may be sent to me (Bill Mace) now—4 copies, APA style, double spacing. The journal is called *Ecological Psychology,* there are four issues a year, and the first will be next January (1989). I am editing the journal with the help of two Associate Editors, Claes von Hofsten and Edward Reed, and twenty-seven Consulting Editors.

1988 ANNUAL MEETING

Remember, Annual Meetings occur on the third Saturday in October. This year that means October 15. Our hosts will be the new Center for the Ecological Study of Perception and Action (CESPA) at the University of Connecticut, Storrs, Connecticut.

For the last several years, Michael Turvey, Claudia Carello, and Bob Shaw have sponsored a regular series of seminars on Mondays called the "Perceiving and Acting Workshop" (PAW). Articles based on presentations at PAW have been collected in technical reports called the *PAW Review,* edited by Claudia Carello. To date, three issues of the *PAW Review* have been produced. Copies can be obtained from Claudia for $2.50 each (Claudia Carello, CESPA, Box U-20, 406 Cross Campus Rd., University of Connecticut, Storrs, CT 06268.)

EVENT CONFERENCE–1989

July 24-28, 1989

Miami University

Oxford, Ohio 45056 USA

After a flurry of phone calls and electronic mail exchanges, and with the excellent cooperation of numerous people, the next Event Conference has descended on Len Mark and Cathy Dent of Miami University (near Cincinnati). It is essential that they begin work on the conference program now. They ask that ISEP members provide them with proposals for conference symposia (or even suggested topics), *no later than August 15, 1988* (earlier would be appreciated). It would be helpful to Len and Cathy if each proposal contained the following information:

(a) A brief (150-200 word) description of the topic (or area) including the types of problems that might be addressed, importance of the topic (or area) to event perception and/or action; (b) nominations of prospective symposium leaders who will take some of the responsibility for organizing the symposium; (c) nominations of 2-3 people who would be appropriate to invite to give plenary talks; and (d) names of at least 5-6 people whose research would contribute to such a symposium. (The organizers anticipate that roughly 10-15 papers will be needed to provide a solid foundation for each symposium.)

The conference will be organized around these symposia, so this call for symposium topics is crucial to the overall plan of the conference. Abstracts of the symposium descriptions will be used in the CALL FOR PAPERS which will be part of the Conference Announcement that will be distributed by September 30.

At this time the organizers anticipate that the program will be able to accommodate 10-12 symposia. The proposed format has plenary addresses for two symposia in the mornings, followed by a 2-3 hour period for viewing relevant poster presentations and finally a discussion period, led by one or more discussants. At this time only the discussion groups will work in parallel. There has been widespread sentiment for avoiding parallel sessions wherever possible. As both Sverker Runeson and Walter Gerbino can testify, avoiding parallel sessions is much easier in theory than in practice. The scheme proposed here will rely heavily on the poster format for presenting data. Participants will have available a variety of audio - visual facilities, including computers, to enhance their presentations.

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ISEP—GERMAN SPEAKING SECTION

On the occasion of the 30th Conference of German Experimental Psychologists (Marburg, March, 1988) the German speaking section of the ISEP was founded.

This section tries to bring together ecoscientists mainly from Austria, Federal Republic of Germany, German Democratic Republic and Switzerland. The Board of Directors consists of Walter Ehrenstein (Institut für Arbeitsphysiologie Dortmund, FRG), Rainer Guski (Ruhr-Universität Bochum, FRG) and Lieselotte van Leeuwen (University of Nijmegen, NL).

The theme of our first meeting was "Ecological Realism: Perspectives for Experimental Psychology". Papers given were: Epistemological Backgrounds of the Ecological Approach (Christian Munz), Examples and Problems of GIBSON's Perceptual Experiments (Klaus Landwehr), RAUSCHECKER'S Results on the Neurophysiological Representation of Optical Flowfields in the Cat (Walter Ehrenstein), First Hints on Ecological Acoustics (Rainer Guski), Thinking about Ecological Semantics (H. Bock), Perceptual Development of Tool Use in Infants (Lieselotte van Leeuwen) and Ecological Perspectives for Ergonomics (Wolf Heine).

Our next meeting will be held at the 36th Congress of the German Psychological Society at Berlin, October, 1988. We will hold a Workshop on Ecological Psychology. Papers to be given are: Compatibility and Affordances (W. Ehrenstein, W. Heine), Is the Overestimation of Vertical Lines Functional? (R. Guski), About Surfaces (K. Landwehr), Ecological Realism—A Guiding Principle for Functionalistic Psychology (C. Munz), Perception: New Perspectives on the Nature of its Objects, Informational Support, and Natural Realization (S. Runeson) and the Possible Impact of Event Perception on Disposition Research (T. Schulz).

Our own first workshop on Ecological Psychology is proposed for Bochum or Witten (FRG) in April, 1989.

If you would like to contact us, please write to: Prof. Dr. Rainer Guski, Ruhr—Universität Bochum, Psychologisches Institut, Postfach 102148, 4630 Bochum 1, FRG. BITNET: Walter Ehrenstein at UAP004@DDOHRZ11.

MEETING ABSTRACTS

Spring 1988 Meeting
Antioch College
May 20-21

Gary Riccio and Tom Stoffregen organized a superb meeting at Antioch College in Yellow Springs, Ohio. Tom took the first group picture of people attending any ISEP meeting and they produced the first complete program of abstracts for an ISEP meeting. Rather than select some of those abstracts for reprinting here, we decided to send the whole set to all members in this mailing.

HARTFORD, OCT. 17, 1987

Neural Dynamics of Emergent Visual Segmentations

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Segmentations of a visual scene are called "emergent" if regions perceived to be separate do not differ in global statistics of luminance distributions, or if the perceived boundary separating regions is sharp and continuous while the luminance patterns are discontinuous or fuzzy. Considerable terminological debate has centered on the status of such segmentations. (For example, do we "see" an equilateral triangle when looking at three equidistant dots?) A neural network model of emergent segmentations has been developed to account for perceptual data in boundary detection, completion, and sharpening and in grouping of textural regions, through the dynamical interactions of nodes or cells that are functionally identified with cells found in the early visual pathways of primates. The model makes explicit and clarifies the distinction between the recognizable boundaries that form groupings of visual elements, which may or may not be topographically mapped with perceived color or contrast, and those featural qualities of surface appearance commonly described by such terms as hue, saturation, or lightness. This distinction rests in part on the complementary attributes of the dynamics of network interactions underlying the boundary/feature distinction. Interactions in the boundary system are directed inward between two or more orientationally tuned nodes that are sensitive to amount of contrast, but not direction of contrast (light-to-dark vs dark-to-light). A particular arrangement of short range competition and long range cooperation among orientation sensitive nodes ensures a rapid, active choice and sharpening of boundaries, including emergent boundaries. Interactions in the feature system are diffusive, flowing outwards from any single active node, and are sensitive to both amount and direction of contrast. Because the patterns of activity in the boundary system restrict the diffusion of activity in the feature system, the resulting regions of like color or contrast typically match regions formed by boundary segmentations one-to-one. Thus the objects that we regognize in everyday environments generally have surface appearance attributes that are unitary and distinct from appearance attributes of other objects. Many laboratory curiosities, on the other hand, —including illusory contours, Glass patterns, Beck/Julesz textural displays, and neon color spreading—present the observer with a seemingly paradoxical scission between recognizable regions and phenomenal color patterns. The network model clarifies how these phenomena are inevitable consequences of the strategies that the primate early visual system has evolved in order to coherently segment and group complex patterns of optical stimulation in real time and with finite, quantized receptive fields.

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ARTICLES

A Note on Dynamics and Kinematics

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At the Fourth International Conference on Event Perception and Action in Trieste this past summer, Karl Newell, Bruce Kay, and I had a brief discussion in which we agreed upon a need for clarification of the meaning and use of the term "dynamic". The concern was that there may be some confusion because "dynamic" is used in non-technical parlance to denote activity or change. In this usage, "dynamic" connotes motion. However, the technical use of the term in mechanics contrasts "dynamic" with "kinematic", where the latter term connotes motion and the former is used to signify physical constraints responsible for particular forms of motion. If technical usage is mistaken for the informal expression, then the significance of the contrast between dynamic and kinematic is lost.

Kinematics and dynamics have become a common and important part of theoretical and experimental explorations reported in the psychological literature. Related work in perception and cognition includes studies in naive physics as well as studies on the kinematic specification of dynamics and, more generally, on event perception. Included under this heading is a growing body of work in social psychology on person perception, expectation, gender and age perception, and the perception of emotion. Also included are studies in speech and sign language perception. Related work in action includes studies on control and coordination as well as on skill acquisition and motor memory. Despite its importance to problems in human action and perception, the relation between kinematics and dynamics as descriptive systems has never been very thoroughly explored or explicated in the collective literature. On occasion, brief reference has been made to standard mechanics textbook classifications. However, given the focal importance of this relation to our developing understanding of action and perception, a more careful examination is to be desired. Ideally, the classification should reflect the structure of dynamics. Kinematics and dynamics refer to descriptive systems that are used to describe alternative, but related aspects of events. Dynamical description is not independent of kinematic description.

A standard classification provided in the textbooks and in dictionaries is as follows. (For the following discussion, see Becker, 1954; Halliday & Resnick, 1981; Mach, 1960; Maxwell, 1920; Sommerfeld, 1942;

Tuma, 1974a,b; Merriam-Webster, 1949; Stein, 1981; and Whittaker, 1959.) Classical mechanics is divided into Statics and Dynamics. Statics treats the stability of force structures in which there are no changes in position over time, that is, no motion. Dynamics is used to describe the behavior of force structures in which there is change of position over time. Dynamics is subdivided into kinematics and kinetics where kinematics is used to describe motions without reference to an underlying force structure and kinetics is used to describe the force structure, (e.g. Tuma, 1974b).

This classification is subject to variation. There is general agreement and consistency in the definition and use of "kinematics".[1] However, the use of "kinetics" in the literature of mechanics is inconsistent. "Kinetics" is used sometimes as synonymous with "dynamics" (e.g. Sommerfeld, 1942). Alternatively, Maxwell (1920) reserves "kinetics" for descriptions of collisions between particles where stresses to both particles are included in the description. "Dynamics" is applied by Maxwell to systems in which the action of one particle is replaced by forces acting externally upon the remaining particle. This distinction is consistent with the use of "kinetics" as in the kinetic theory of gases where gases are described as small particles in random motion impacting on one another with consequent changes in velocities or directions of motion.

Most often, kinetics is omitted altogether from the classification of mechanics in favor of reference to only kinematics and dynamics in parallel to geometry and statics. This classification is the more apt. The reason is that classification is based on the measurable dimensions associated with mechanical descriptors and that, in dynamics, only two categories are distinguished. The first category includes descriptors associated only with lengths and times. This is kinematics. The second category includes descriptors associated with mass as well as lengths and times. If this is called dynamics, then reference to kinetics is superfluous.

"Dynamic" and "kinematic" usually are used to refer not only to descriptors, that is, to variables and parameters, but also to equations. Confusingly, variables or parameters identified as belonging to one type appear in the equations categorized according to the other type. Kinematic variables populate dynamic equations and visa versa, dynamic parameters may appear in kinematic equations. Nevertheless, the classification scheme is consistent. Variables and parameters are always combined within the terms of an equation so that the dimensions associated with the terms are of the type appropriate to the equation. However, this observation reveals the mutual dependency of kinematic and dynamic descriptions. This dependency extends beyond the formulation of equations to the definition of individual descriptors which fall into either category.

Kinematic descriptors include change of position as a function of time and the derivatives (to all orders) of this function, that is, velocity , acceleration, jerk, etc. Also included in kinematics are period or frequency, amplitude, lengths and angles used to describe the configuration of a system of rigid bodies including the location of connections that determine joints and lever arms; lengths and angles used to describe the shape of rigid bodies; lengths and angles used to describe positions of the center of mass of rigid bodies; and lengths used to describe height in the gravitational field or displacement from equilibrium of a stretched spring. Implicit in many of these descriptors are references to dynamic properties such as rigidity, gravity, and centers of mass. Torques are implicit in references to lever arms. 'Generalized coordinates' are a means of describing positions that implicitly incorporates rigid body constraints to reduce the complexity of description. A common example is the use of angles at the joints to describe the configuration of an arm assuming the rigid nature of the bones. It is possible, in principle, to write a kinematic description without reference to dynamics. However, in common practice, reference to dynamic properties is made frequently in kinematic descriptions. Further, as shown below, these is a sense in which dynamics is always implicit in kinematics.

Dynamic descriptors include mass or inertial coefficients, damping coefficients, stiffness coefficients, external forcing coefficients, and coefficients of elasticity. The explicit relevance of kinematics to dynamic descriptors is apparent in the associated dimensions. Dynamics includes not only the mass dimension, but also the (kinematic) dimensions of length and time. For instance, force is a dynamic descriptor since, dimensionally, it is mass-length per time squared. The dimensional composition of force reflects the form of the dynamic scaling relation that describes the scaling between kinematic properties and dynamic properties. The relation is Newton's second law, $F=ma$. Inertial force is scaled by a mass coefficient on acceleration. The preponderance among dynamic descriptors of coefficients reflects the role in dynamic descriptions of kinematic variables.

The descriptive apparatus in dynamics consists of equations of both differential and non-differential type. The motion of a system is said to be described by an equation that represents position as some function of time. This equation constitutes the solution to a differential equation which, confusingly, is called the 'equation of motion' . The differential equation is referred to as describing the dynamics while its solution corresponds to the kinematics. The reason is that, using a Newtonian as opposed to an energy formulation, each term of the differential equation has the dimensions associated with force while the terms of the solution equation are associated with the kinematic dimensions.[2]

When the components in each term of the differential equation are considered, one finds a kinematic variable as well as dynamic coefficients. In fact, many of the kinematic variables appearing in the dynamic differential equation never appear in the kinematic solution equation. These variables appear in the differential equation by definition because these variables are derivatives representing velocity and acceleration and derivatives contain the differentials of differential equations. An equation made up solely of dynamic descriptors is rarely, if ever, encountered. The force structure in dynamics is not described in a way that is independent of the kinematics. Furthermore, either dynamic or kinematic descriptors could appear as parameters in either the differential equation or the solution equation. The reason is that there are equivalence relations between kinematic descriptors conventionally appearing as coefficients in the solution equation and dynamic descriptors appearing as coefficients in the differential equation. In the case of a linear oscillator, for instance, frequency is equal to the square root of a ratio between stiffness and mass.

Approached geometrically rather than analytically, dynamics and kinematics are, locally, dual descriptions of a single qualitatively distinguished structure. Geometrically, the kinematics is represented by trajectory curves in phase (or state) space. The dynamics is represented by a vector field on phase space in which each vector is tangent to a trajectory curve and of a magnitude representing the phase velocity. Thus, qualitatively, the layout of trajectories and of the vector field is the same. The shapes in the kinematic and dynamic portraits are identical.

Strictly, the difference between kinematics and dynamics is in scope. But how they are distinguished can vary. If we confine attention to a single trajectory and notice that the differential equation describes an instantaneous, 'differential' relationship while the solution equation contains a function specifying the form of the entire trajectory, then dynamics would provide a local description while the kinematics provides a more global description (Padulo & Arbib, 1974). [Padulo and Arbib note that if derivatives of all orders (to infinity) are available at an instant, then for many functions the entire trajectory is determined.] However, when the entire phase space is considered, the differential equation determines all the acceptable solutions which are dense in a region of phase space while a solution equation corresponds to only a single curve in that region. Viewed in this way, the dynamics would correspond to a global description while the kinematics are local. Either way, locally, the equations provide dual descriptions of the form of a trajectory. Globally, kinematic trajectories and dynamic vector fields are merely alternative representations of all the behaviors that could possibly be exhibited by a system.[3]

Kinematic and dynamic descriptions provide a means of representing alternative aspects of unitary events. Kinematic and dynamic descriptions are related intimately because they are used to describe different aspects of a single entity. Dynamical description includes an extra dimension, mass, and hence, is superordinate to kinematic description. At the risk of sounding redundant, dynamical descriptions are used to describe the dynamics of an event. The dynamics in a particular instance are not identical to a particular differential equation or to a particular geometrical representation, that is, to a vector field on a phase space with particular coordinates. This is apparent because the same dynamics can be represented by different differential equations or by vector fields in different coordinates. (See e.g. Hirsch & Smale (1974), pp. 1-8; and Marmo, Saletan, Simoni & Vitale (1985).) Some representations are less complex than others allowing important and useful properties of a dynamical system to be apprehended more readily. The investigation of a dynamical system often involves transformations from one representation to another in search of properties that remain invariant over alternative representations. Such invariants reveal essential properties of a dynamical system and are used as a means for recognizing a specific system. Progress in identifying dynamical systems has required care in distinguishing descriptive apparatus from that which is to be described. (See Bingham (1987) and (in press) for further discussion of these matters specifically within the context of problems in either perception or action.)

Footnotes

[1] Note that kinematics is used also, as in applied kinematics, to describe "the theory of mechanical contrivance for converting one kind of motion into another" (Stein, 1981). See also Patton, (1979) and Prentis, (1980). According to J.L Heilbron (1981),

Simon Stevin (1548-1620) distinguished kinematics from statics to allow unencumbered use of the method of virtual displacements in statics. The Newtonian method of force balance is used, in both static and dynamic situations, in the derivation of equations which describe force structures. Based on this observation, dynamics might be described as a relation between statics and kinematics. This is the approach used by Brady, Hollerbach, Johnson, Lozano-Perez & Mason (1982) in their book on robot motion. However, this is at odds with common usage in which statics applies only to problems involving no motion, that is, to static situations. Following common usage, geometry (as involving only lengths) is to statics as kinematics is to dynamics.

[2] A rule of dimensional homogeneity in mechanics requires that all of the summed terms in an equation have the same dimensions.

[3] This is assuming, at least, a C1 smooth system so that the vector field is defined at each point along all the trajectories. Without this condition, the relation must be revised to handle corners and other discontinuities in a piecewise manner.

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Unpacking the Ames Room: What the Transactionalist's Brain Doesn't Tell the Ecological Psychologist's Ear

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New York University

This paper examines the strong claims of Gehringer & Engel's (1986) study of an Ames Distorted Room Illusion (DRI). They found decreasing size-illusion-magnitude with increasing opportunities for binocular viewing, head movement, and changes in viewing position. But since the size-illusion did not disappear completely, they interpreted their results as weighing against Gibson's view of information inadequacy as the basis for Ames Room phenomena. The present paper counterclaims that aspects of their procedures precluded a strong test of Gibson's position on Ames Room phenomena, and that their claims were unwarranted. Further, observations of the full-size Ames Room at IBM's "Seeing the Light" museum exhibit, and explorations of explanations provided, suggest that Transactionalist accounts of the phenomena are inadequate. The Ames Room appears rectangular or distorted depending on viewing conditions. Contrary to Transactionalist accounts, both phenomena must be primarily "information driven" rather than "assumption driven."

Gehringer & Engel (1986) attempted to address Transactionalist vs. Gibsonian accounts of nonveridical perception by altering the usual restricted conditions of viewing an Ames Distorted Room. Subjects made size judgments of discs within small model Ames Rooms while looking through the usual aperture, or with progressively more information provided by binocular viewing, head movement, and multiple viewing positions. Illusion magnitude decreased substantially with binocular viewing and increased movement, but did not disappear altogether. Thus while freer access to normal visual information about room shape decreased error, it did not entirely eliminate error. The authors concluded that the Ecological account of Ames Room phenomena (Gibson, 1966; 1979) fails to account for their results because despite allowing viewers free access to more information about the room and its contents, some illusion persisted. Gibson had once claimed that if binocular viewing and multiple views were allowed, the actual spatial layout would be picked up.

First, one may question the DRI used by the authors to index perceptual change in the Ames Room. The primary phenomenon is the rectangular (or cubical) appearance of the room in spite of its non-rectangular distal shape. Transactionalist theory holds that the first phenomenon of interest is the perceiver's selection of a rectangular distal situation in the face of ambiguous proximal information, which could specify sloping floors and ceilings, unequal size and shape windows, slanted walls, or a rectangular room with normal details. The Transactionalists hold that this initial selection, driven by assumptions derived from past experiences with normally carpentered environments, then produces secondary phenomena, such as mistaken perceived sizes and shapes of the room's contents. Perhaps the primary apparent shape/slant phenomena would have more directly indexed perceptual changes as a consequence of further visual access. It is well known that in size-judging situations, observers may selectively adopt "perspective" or "objective" strategies, variously ignoring or attending to contextual environmental distance information, with resulting proximal size matches, distal size matches or compromise matches (e.g., Carlson, 1977). Thus a size-judging task may involve substantial variability in subjects' utilization of the very spatial information they were supposed to be picking up as more access to envionmental information was permitted. Gibson's own work on size judgments out of doors in information-rich natural environments revealed less than perfectly accurate judgments. It is unlikely that he would have predicted perfect performances in information-poor Ames Rooms since his own data yielded size overestimations on the order of 6.8% (Gibson, 1950, p.186). Yet Gehringer and Engel stated: "However, Gibson's claim was not simply that illusion will be modified in a veridical direction as one adds further information, but rather that if access is allowed, the actual spatial layout will be picked up. This latter concept leaves no room for *any* (italics mine) perceived rectangularity (sic) in viewing a trapezoidal three-dimensional configuration" (Gehringer & Engel, 1986, p.185).

The average magnitude-of-illusion value reported by Gehringer & Engel in their "unrestricted viewing" condition was a PSE of 32.6mm for a 30mm disc–8.7% illusion and an average error of 2.6mm, making something of a mountain out of a millimeter. It must also be noted that Gibson made no specific claims regarding size judgments; he wrote only of the apparent shape of the distorted room: "The fact is, when an observer uses two eyes and certainly when one looks from various points of view the abnormal room and the abnormal window are perceived for what they are, and the anomalies cease" (Gibson, 1979, p.168).

In their second experiment, Gehringer & Engel instructed subjects "...to move back and forth along the entire front of the room before making their judgments." They were also told "...that they were free to make any other movements they felt necessary for an accurate judgment" (p.185). However, this instruction assumes subjects have good strategies for picking up information about relative size in distorted and undistorted rooms. The authors present no evidence this is so. Moving back- and-forth in front of the rooms may provide motion gradient information about the slant of the rear wall and window shapes. But unless the floor or ceiling were visibly textured (which they weren't), the resulting situation for discovering the relative distances of the discs might be far less then the "...optimum conditions of observation" invoked by the authors (p.185). Motion parallax might have been minimal since there were no objects between subjects and target discs on the rear wall (e.g., see Eriksson, 1974). The only basis for motion parallax/perspective might have been the open wall closest to subjects, but one cannot determine the clarity of this stimulus information from the report. Nor does the report mention whether subjects went around to the ends of the room, which might have provided better information about room shape and floor-to-wall distances, whether subjects moved toward and away from target discs, or in other ways optimized strategies for picking up relative distance information presumably required or veridical size judgments. The size-judging task may also have induced a "frontal view" strategy precluding obtaining the information for precise size judgments. Indeed, the amount of time permitted subjects to explore the arrays was likely inadequate. The Transactionalists themselves have demonstrated that learning the actual shape of a distorted room is a complex and extended process (e.g., see Kilpatrick, 1954).

Further, one may question whether Gibson's position was as absolute as Gehringer & Engel indicate. Gibson has written: "The perceiver who has observed the world from many points of view, as we say, is literally one who has traveled about and used his eyes. That is, he has looked at the furniture of the world from many station points. The more he has done so, *the more likely it is* (italics mine) that he has isolated the invariant properties of things—the permanent residue of the changing perspectives" (Gibson, 1960, p.220) This quote suggests a far more labile position regarding pickup of layout information than Gehringer & Engel permit, and is not at all inconsistent with their data. Considering the rather unique configurations of distorted rooms, it appears unwarranted to suggest that Gibson would predict rapid and complete discovery of layout information sufficient for perfect size judgments given the situation presented subjects.

When Ecological theory asserts that ambient structured light unambiguously specifies a room's shape, etc., this properly refers to an environment which has not been purposely umpoverished to eliminate most information specifying shape/ slant, size/ distance. Ames Rooms are typically constructed to do just that; lighting is low-intensity and uniform so that shadows are minimized, and surfaces are smooth and painted flat to eliminate microtexture information (e.g., see Gehringer & Engel, 1986, p.182). Gehringer & Engel's rooms were suboptimal structures for veridical perception. Thus, even if their subjects utilized optimal information pickup strategies (which is unlikely), the environments precluded optimal size judgments, making requirements for complete precision of size judgments unwarranted. The dual issues of the quality of information available and the way tasks may affect information pickup has been elaborated by Cutting (1986, pp.250- 252), and preclude the strong claims of Gehringer & Engel's paper.

Two recent visits to IBM's "Seeing the Light" illusion exhibit at the N.Y. Hall of Science permitted the present author access to a full-size and highly effective Ames Room, and informal interviews with several visitors exploring the room. This room is about 5m long, contains sharply inclined floor and ceiling, and an ellipsed clock on the rear wall containing the usual trapezoidal windows. One end of the room contains a door permitting visitors entry, and they often enter the room and explore it after looking through the aperture. The room is extremely well-constructed, and looks rectangular from the aperture—even to at least one Ecological psychologist.

Visitors questioned about the room's appearance from the aperture concurred that the room looks rectangular, the windows normal in shape, the floor level, the clock circular, etc. But when questioned about the room's appearance from the doorway, the same visitors said it looked distorted, "weird", the floor steeply inclined, etc. When they entered the room, they did not trip, but mounted the ramped floor and walked about without mishap. The museum's posters explaining the exhibit proclaim: "Your brain is so convinced (from past experience) that the room is a box... it refuses to see how far away the (rear) wall really is...the experience clue is so strong that the brain admits even the clearly ridiculous perception that people grow longer (when walking across the room)..." And, in large bold print, visitors are told: "All visual information is ambiguous."

If we unpack these assertions carefully, we find them fragile indeed. If the distorted room looks normal from the aperture because of assumptions derived from past experience with normal (western carpentered) rooms, and if all visual information is indeed ambiguous, why do the room and its details look extremely distorted from the doorway? It is possible to produce a distorted proximal layout from the doorway position with a rectangular distal room arranged to project a distorted appearance. Certainly the selection of the rectanguar distal layout makes more Transactional sense than selecting the distorted layout; but that is what observers apparently do. Surely we make the same assumptions from the doorway that we do from the aperture, which would hardly lead to selection of a distal situation as bizarre as that provided. Further, after walking about inside the room, the room still looks rectangular, the clock circular, etc., to me and to other visitors questioned, in spite of substantial behavioral transactions with its nonrectangular/noncircular features. This set of phenomenal facts is about as damaging to Transactional accounts as the room's distorted appearance from the doorway. Only if one admits substantial information-driven perceptual consequences do Ames Room phenomena become explicable. As noted previously, rectangular rooms can be made to appear distorted, just as distorted rooms can be made to appear rectangular (Welford, 1970, p.17). Ames Rooms appear rectangular from the aperture because the information available has been biased for that appearance; they appear variously distorted from other positions depending on the quality and amount of information available which specifies their actual shape.

Transactional accounts of Ames Room phenomena give us little predictive power concerning changed appearances. They maintain that assumptions determining perceptual selection are weighted averages of previous experiences, each experience entering into the average weighted differently in terms of its importance to the experiencing person (Ittelson & Cantril, 1954, p.12). Such a view seems to imply that substantial experience inside distorted rooms should markedly alter experiences of observers looking through the aperture. My experiences at the museum exhibit fail to confirm this. One cannot prove there is no change, but at best there is little change. But even a quick static look through the doorway reveals a great deal about the actual layout of the room, and the shapes of its contents. Why doesn't the museum observer's "...brain refuse to see..." how distorted the room really is? The failure of Transactionalist accounts to illuminate these issues seems far clearer than any predictive failures of Ecological theory regarding the Ames Room.

From an Ecological point of view, one can correctly predict that pickup of invariant structure may be greatly aided by binocular viewing, head movement, and larger changes in viewing position. Optimizing environmental information in a natural environment (e.g., by increasing room illumination, permitting shadows and visible surface microtexture to be seen), or by artificially enhancing this information by surface-structure manipulation follows from Ecological theory, but not from Transactional theory. Kilpatrick's (1954) study of perceptual learning in distorted rooms left observers motionless while they (or others) explored the room's distance relationships with a stick. Whether perception becomes quickly and completely veridical is a matter of the optimality of environmental information on the one hand, and optimal observer strategies on the other. Single views of arrays can indeed be ambiguous, as agreed upon by both Transactionalists and Ecological theorists. Multiple views and transformations usually serve to provide unambiguous percepts; but no theory predicts perfect accuracy! As stated by Transactionalists themselves: "Perceiving never takes place by itself...it can only be studied as part of the situation in which it operates" (Ittelson & Cantril, 1954, p.2) The situations provided by Gehringer & Engel's study yielded large improvements in accuracy of size-judgments as observers were given access to more information. But the situations and strategies only permitted accuracy over 90%. The defense rests.

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A Critique of the Inferential Paradigm in Perception

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(This is an extended abstract of the longer paper cited at the end—Ed.)

The prevailing cognitive paradigm of perception is inferential (or computational) in which perception is conceived of as a product of prior, and usually unconscious, intellectual processes. This paradigm is examined and shown to be inadequate, and an alternative paradigm is proposed.

The difficulties of the inferential paradigm may be summarized as follows. 1) The postulation of unconscious inferences does not add any explanatory force. Duplicating the conscious mental realm into the unconscious one is not an explanation. It only provokes a need to postulate the existence of a homunculus and, in addition, an endless inferential regression is created. 2) Perception is explained by the reasoning processes typical of thinking. According to the evidence of evolution, these thinking processes evolved only later. 3) The feeling dimension as well as the difference between conscious and unconscious mental states are not explained. 4) There is a confusion between a rule-following behavior and a rule-described behavior. Consequently, there is a confusion between abstract mathematical relations and actual psychological processes. 5) The empirical evidence for the existence of reasoning processes, different reaction-times, is questionable. 6) The reasons for ascribing reasoning processes to the perceptual system are valid for inanimate systems as well—this casts doubt on the ascription in the first place.

In the face of the stated difficulties, perception could be considered to be devoid of reasoning processes such as inference, interpretation,and judgment, but relying nonetheless on computation. This widespread view may avoid some, but not all, of the problems of the inferential paradigm. The basic disagreement involves not which types of thinking processes should be postulated to explain perception, but whether such processes are required in the first place. Answering negatively to the latter question reduces the attraction of the solution suggested above.

The move from inferences to computations has additional significance. Inferences, interpretations, and judgments are intellectual activities related to cognitive content, whereas computation may be purely formal. If computations are purely formal, then part of my previous criticism is no longer relevant. However, a new difficulty emerges: the inability to explain a very important feature of psychological states: meaningfulness (or sematic content). The proposed computations involve the manipulation of meaningless disembodied signs. The meaningless signs cannot be used to explain perception and cognition in general because cognition is meaningful. Meaning is not a formal feature. It belongs to the content of the mental state. The replacement of inferences with computations rather than improving the inferential paradigm, considerably weakened it.

I do not suggest discarding the empirical findings of the inferential paradigm. I suggest placing (most of) them in a different conceptual framework. The reasoning processes postulated by the inferential paradigm should be viewed as one way of describing the organism's behavior, not as actual internal processes inside its head.

In the last section of the paper I briefly propose what may be termed a "schema paradigm." A perceptual schema is the way a perceptual experience is organized. The organization is shaped by both the agent's innate dispositions and acquired personal characteristics. In the inferential paradigm, reasoning processes precede and produce a finished perceptual product separate from the processes themselves. Therefore, in the inferential paradigm the reasoning processes function as preparatory and mediating elements in preception. In the cognitive paradigm a perceptual schema functions entirely differently, it is not separate from the actual preceiving. The schema is constantly participating in the in the ongoing state of perceiving because is is the way the perceptual system is "tuned."

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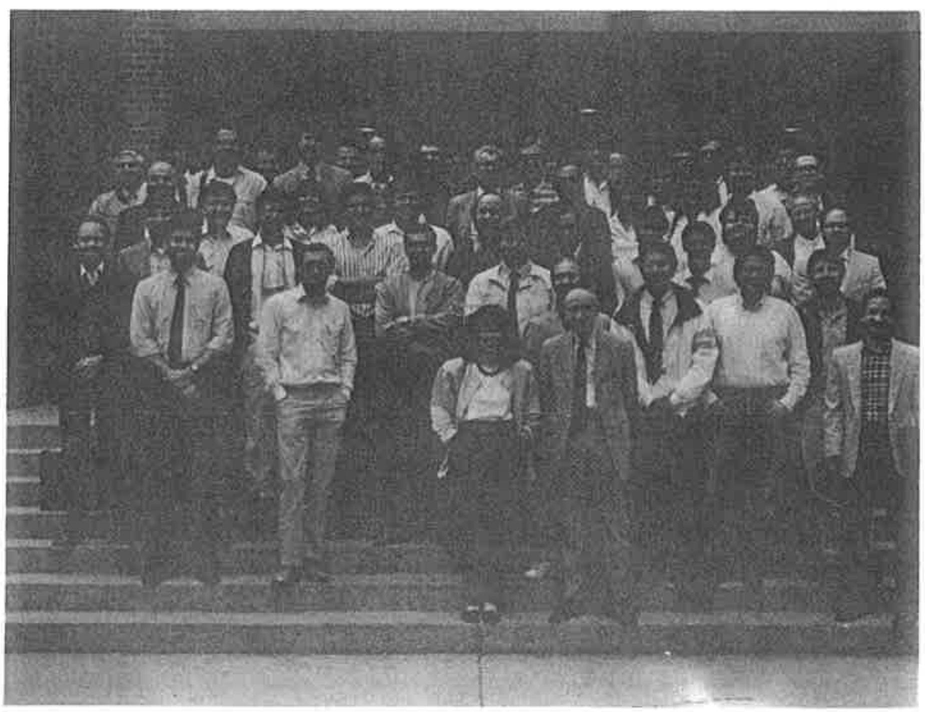
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