The International society for Ecological Psychology

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MEETINGS

1987 ANNUAL MEETING
October 17, Hartford, CT

The program was devoted to the application of the physics and mathematics of dynamics to problems of perceiving and acting. In order, the speakers were Onno Meijer (Amsterdam), Howard Pattee (Binghamton), Peter Kugler (Illinois), the trio of Catherine Browman, Eliot Saltzman, and Louis Goldstein (Haskins Labs), Richard C. Schmidt (Connecticut), and Ennio Mingolla (Boston). *Audio cassette tapes of all talks and the business meeting are available.*

Howard Pattee sent his abstract in time for this Newsletter and is printed below.

The journal news reported in the last newsletter was repeated and discussed. As a result of discussions about the problem of "bootstrapping" an editorial board and a set of papers at the same time, Bob Shaw (ISEP President) appointed a Publications Committee during the week after the meeting. The committee— Anne Pick (chair), Eleanor Gibson, Jim Todd, Carol Fowler, Michael Turvey, and Sverker Runeson—was asked to propose an editorial board to be approved by the ISEP Board of Directors.

Election results will be published in the next Newsletter.

How many meetings should the ISEP hold and when should they be? Those questions received spirited discussion. Some people think we have too many meetings. Others argued that the advantage of more than one meeting a year is to spread them around to a variety of places. If we moved to one meeting per year, it looks like people want it to be more than one day. That, in turn, would dictate late May as the best time because it is so difficult to find meeting times in the fall. At the moment, no changes are in the works. As you will see below, there is a proposal afoot for a spring meeting in 1988. What do you think? Please send me (Bill Mace) your opinions.

The last issue that came up in the short time we had was the title of the Event Conference. Would it be more accurate to call it the International Congress on Ecological Psychology (and maintain the numbering)? The original name was chosen to describe the intersection of ideas inspired by Gunnar Johansson, James Gibson, and Nicolai Bernstein. Are those roots sufficiently recognized even if the name is changed? We always have been careful to make it clear (successfully) that these are not ISEP meetings and that it is important to have a variety of viewpoints represented. Would this policy be harder to convey with a change of name? Or do the precedents speak for themselves? So: What's in the name? Again, tell me what you think.

Trieste

The Trieste meeting was staggering. Its Congress Center (flying the flags of all 22 nations represented by conference participants), in a renovated Trieste Harbor dock, was dazzlingly scenic. The program had grown into a book, and the conference materials were distributed in a striking carrying case radiating "Italian Design." As in Uppsala, an all-Conference dinner was held in an ancient castle (this trend does not mean that subsequent conferences in the U.S. will be catered by White Castle). There were more presentations and more participants than anyone ever imagined in the two years of planning. Fortunately there was also a larger, more professional conference staff than ever before. Walter Gerbino knew what he was doing when he volunteered the Trieste site. Elisa Malutta, who headed the professional staff, won the gratitude and admiration of everyone who had a problem to solve.

We were honored by the presence of a large number of the major figures in Italian experimental psychology. We were saddened by the passing of Metelli earlier in the year, and warmed by the attention given the meetings by Kanizsa.

The numbers (over 250 people attending, nearly all of them participants) dictated a hectic pace—long hours and parallel sessions. The pressure reached a climax Tuesday night when it began to feel like we had been meeting for two weeks. Schedules diversified and people learned to cope so that the pervasive feeling of being overwhelmed subsided for everyone except those with presentations late in the week.

It is impossible to summarize the conference. That was also true of the first three, of course, but even the pretense is inappropriate for this Fourth one. *Stavros Valenti* (Dept. of Psych, Hofstra University, Hempstead, NY 11550 USA) did tape record many sessions and is the person to write if you'd like a tape of some of the talks. He could not be in more than one place at once and therefore has only a selection, but it is probably the largest selection available. The program is also a valuable document and well worth studying if you can borrow a copy from someone who attended. It is far more than a mere refinement of the last tentative list of talks sent out to all ISEP members.

Next Event Conference

The U.S. will be the site of the next conference. At the suggestion of Mike Wade, we have applied for a summer 1989 date at the Asilomar Conference Center in Pacific Grove, California, near Monterey (roughly the San Francisco area). The alternative was for 300 people to appear on Wade's doorstep in Minneapolis (roughly the Lake Wobegon area). Several ISEP members (Cutting, Hagen, Kaiser, and Sedgwick are the ones I know about) attended a NASA conference at Asilomar immediately following Trieste. We should find out soon whether or not they have reserved the days for us. There may be decisions to make about exactly which days are best. If you know of any important considerations that affect the date we choose, let me (Bill Mace) know immediately.

Spring ISEP Meeting—Ohio

Soon after the Annual Meeting at Trinity College, Gary Riccio and Tom Stoffregen volunteered to host a spring 1988 ISEP meeting. It will probably be scheduled for late May. They also will be the program organizers. Therefore all people who want a place on the program should contact Tom or Gary. Riccio's address: Armstrong Aerospace Medical Research Laboratory, Bldg. 248, Wright Field, Ohio 45433 USA. Phone (513) 255-8870.

ABSTRACTS

ATLANTA
May 1987 (Concluded)

Perceivers Are Only As Smart As What They Are Allowed to Perceive

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Recent work has begun to assess infants' perceptual abilities by using more natural, less impoverished perceptual displays and situations. There is a growing consensus that motion enhances the perception of objects, though not under all conditions. There is also evidence that infants can perceive certain types of events and causal relations between objects. These findings are significant in light of developmental theory which claims that infants are incapable of understanding such relations since they do not yet possess the necessary cognitive or representational apparatus. However, the representational position is not being questioned in light of these findings. Rather, when infants perceive successfully, as adults do, it is assumed that they possess the required cognitive skills. When they fail in perceptual tasks, even when viewing rich, dynamic, ecologically valid displays, it is concluded that infants do not yet possess the cognitive or representational skills required.

Being uncomfortable with such non-ecological outcomes and conclusions, we began to examine the types of displays infants were given to perceive, and the experimental context employed in some of the best ecological experiments (e.g. Baillargeon, et al. 1985; Baillargeon, 1986; Gibson, et al., 1978; Gibson, et al., 1979; Kellman, 1984; Kellman & Spelke, 1983; Ruff, 1982). Both positive and negative evidence regarding infants' abilities have emerged from these studies. Infants perceive more when presented with dynamic versus static displays, and are capable of detecting invariant information regarding certain object characteristics and object-motion properties. Infants apparently also possess an object concept at a much younger age than predicted by most developmental theories. However, infants sometimes do worse, perceptually, when given more information, and they cannot perceive 3-dimensional shape when viewing static stimuli. Finally, an infant's perception of objects varies, not necessarily systematically, under different object-motion conditions.

Overall, these studies demonstrate the importance of dynamic perceptual information, but they raise questions concerning how infants' capabilities and failures are explained. These questions revolve around the nature of the event information presented to infants, and analysis of this information (Gibson, 1979). Often infants view objects which hang and move in empty Euclidean space, where there is no surface of support and no apparent reason for the movement. In other cases, where normal event information is presented, this information is ignored when the findings are explained, and cognitive or representational analyses are employed.

It is not sufficient to simply define an event as the arbitrary pairing of objects and motions, or the random application of transformations to structures. Our ingenuity and technology allow us to create an almost unlimited number of structure- transformation relationships, many of which do not normally occur in nature, and some of which may violate an object's transformational or structural integrity. By failing to consider the specific type of event information being presented in experiments, it is impossible to fully assess what infants are capable of perceiving. Unfortunately, there is no overall event framework or conceptualization which can be used to assess the specific event information presented in particular studies. Recognizing this limitation, we have been developing an event framework.

Object-motion relations, which can be depicted by dynamic and static displays, can be lawful or unlawful, predictable or surprising, and natural or artificially created. Thus, two primary tenets underlie the framework being developed: 1) the laws of physics and biology which constrain event relations, and 2) an observer's (albeit adult's) familiarity with event relations. Three major classes of events are distinguished. Natural events depict objects engaging in motions they were meant to perform (lawful events), while Anatural events show lawful, but untypical object-motion relations. In contrast, Unnatural events violate an object's transformational or structural integrity by defying physical or biological laws. These classes of events are further elaborated by distinguishing biological from non-biological objects, and types of transformations (deformations): 1) motions which permanently deform an object's structure, 2) motions which create reversible structural changes, and 3) motions which leave an object's structure unaltered.

This framework serves as both a guide for our own research and for assessing other research. Our own research begins with naturally occurring events and then systematically violates these events. For example, 4, 8 and 12-month-olds were shown events which obeyed or defied the laws of gravity. The 12-month-olds clearly distinguished the lawful from unlawful events, the 8-month-olds were less clear in their distinctions, while 4-month-olds failed the task, though all infants viewed the biological events longer than the non-biological events. These results were expected since infants demonstrated perceptual abilities which "matched" the amount of experience they had with gravity. Thus, infants who differentially utilized gravity (e.g. walkers, crawlers, non- locomotors) differentially perceived displays along this dimension.

Utilizing the framework to assess other research shows that the majority of events employed fall into the Anatural category (e.g. objects moving and floating in mid-air). Infants perceive amazingly well under these conditions, but before offering conclusions, infants should be assessed using natural event sequences. This raises a host of empirical questions, but a more fundamental conceptual issue also emerges. By not recognizing the nature and informativeness of events, some of the best ecological researchers offer some of the least ecological explanations for the abilities demonstrated by infants (e.g. Spelke, in press). The failure to fully analyze the perceptual information presented to infants, while assuming the best of all perceptual worlds, forces researchers to conclude that there is not sufficient perceptual information to account for an infant's abilities. Thus, conclusions requiring cognitive/representational mechanisms must be offered.

Treating events too lightly can produce research plagued by a fundamental ecological flaw. If infants are not given the opportunity to perceive the informative richness of events, they cannot tell us how perceptually "smart" they really are. By not recognizing all the information contained in events, we rob perception of its potential to explain how we can know the world. In so doing, we lose the insights given to us by Gibson, or fail to see how far these insights can advance our understanding.

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The Role of Siblings in Fostering Maternal Attachment in Ducklings

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It is widely recognized that species-typical behavior is an adaptation to a species' typical environment or niche. A more recent discovery signifies that the species-typical rearing environment plays an essential role in the development or production of species-typical behavior. In the area of maternal attachment in ducklings, for example, the ontogenetic experiential input requirement is so highly specific that it can be provided only by the stimulation afforded by conspecific siblings.

For the past twenty-five years I have been studying the developmental basis of mallard and wood ducklings' auditory and visual attachment to their maternal parent. In both species sibling stimulation makes a highly specific (and nonobvious) contribution to the ducklings' perception of the species' maternal call and to the learning of her visual characteristics. I will review first the siblings' role in the auditory perceptual basis of maternal attachment and then turn to their role in the learning of the mother's visual features.

In nature, the hen of each precocial avian species studied to date utters a species-characteristic maternal assembly call to which their highly mobile young are drawn as if to a magnet. (In precocial species, the young are born in a rather advanced state of development.) The young of these species can be hatched in incubators and, never having directly experienced maternal stimulation, will unfailingly approach a speaker emitting their species maternal call over that of another species in a simultaneous auditory choice test. In the mallard duckling, we learned– by surgically devocalizing them as embryos–that prenatal exposure to their own voice (or that of siblings) is essential to the perceptual specificity in the maternal choice test after hatching. Muted mallard ducklings, deprived of hearing their own or sibling vocalizations, would go as readily to the maternal call of a chicken as to the maternal call of their own species. As it turned out, the repetition rate of the chicken maternal call (2.3 notes per second) is close to that of the mallard (3.7 n/s) and, in the absence of hearing embryonic vocalizations, the usually narrow range of the mallard duckling's repetition-rate preferences (3.3-4.2 n/s) widens to include the rate of the chicken maternal call. We were able to show that by giving muted mallard ducklings a choice test with the mallard maternal call at the typical rate (3.7 n/s) versus the same call at 2.3 n/s: The muted ducklings went as readily to the 2.3 mallard call as to the 3.7 mallard call, whereas vocal ducklings went exclusively to the 3.7 call. In experiments to rule in (or out) the necessity of hearing their own or sibling vocalizations, we played pure tones or fragments of sibling vocalizations to muted ducklings and found a degree of experiential specificity for which we were totally unprepared. In order to show the normal preference for the 3.7 maternal call, the muted birds not only had to hear the wide range of repetition rates produced only by emryos, but they had to also hear those notes in the low frequency range (1500-2500 Hz) produced by embryos and with the characteristic frequency modulation produced by embryos. They also had to be exposed to these particular call notes in the embryonic state: Giving them the same experience after hatching was ineffective in rectifying their preference in the test with the 2.3 and 3.7 calls. Thus, the mallard embryo, in relation to the stimulation it provides itself and that provided to it by siblings, is a component of a developmental system that produces the later normally developed phenotype (specific rsponse to the mallard maternal call after hatching). Natural selection thus operated on this entire manifold. Since natural selection does not favor certain developmental pathways over others but operates directly only on outcomes of development, it inevitably is selecting for the whole package, as it were, i.e., the developmental system that led to the manifestation of the adaptive phenotype.

We soon discovered that the extreme developmental specificity in the acoustic background to the mallard duckling's response to the maternal call of its species is shared by the wood duckling. In the wood duckling the developmental background is more frankly social because, in contrast to the mallards which can profit either by hearing their own vocalizations or those of sibs, the wood duckling must hear siblings (or the recordings of siblings) in order for it to be specifically responsive to the wood duck maternal call; self-stimulation does not work in wood ducklings. The critical acoustic feature of the wood duck maternal call is the descending frequency modulation that is present in virtually every note of the wood duck maternal call. The maternal call notes of the wood duck are shaped like an inverted u, with the second tail of the note descending about 25- 50 Hz below the first tail. As we learned from experiments with synthetic wood duck maternal calls, the descent of the second tail must exceed the initial one if the wood ducklings are to favor it in a choice test. What seems remarkable is that this minute sort of preference is abstracted from the duckling hearing a sibling distress call that descends over a range of several thousand cycles (from around 6000-3000 Hz), and they apply this abstraction to the tail of a naturally occurring maternal note in the region of 1300-1100 Hz. Wood ducklings do not produce calls in that region. But to get to the specificity of the background developmental experience, the wood duckling must hear a sibling's distress call played in the forward direction (i.e., as a descending call note) in order to manifest the normal preference for the descending notes of the maternal call. If the wood duckling is exposed to a recording of a sibling distress call in the reversed direction (ascending modulation), it shows neither the normal descending preference nor a preference for an ascending maternal call note. Our experiments with wood duck embryos suggest that they already have a preference for descending notes in the region of 1300-1100 Hz and that the experience of hearing a sibling's distress call is necessary to maintain that already developed preference. That is, in the absence of normal auditory contact with siblings the normal descending preference is absent.

In the visual sphere, our experiments thus far have involved only the mallard duckling and its learning of the visual characteristics of a mallard hen over the hens of other species. We (Timothy Johnston, Robert Lickliter, and I) have found that for the learning of certain visual discriminations (e.g., mallard hen over pintail hen), the ducklings can be housed nonsocially in visual isolation. But for the learning of other, more difficult discriminations (e.g., mallard duck over redhead duck), they must be reared not only in visual but social (interactive) contact with siblings. Further, this necessarily interactive contact must be with members of their own species; rearing with other avian or even other duck species is ineffective. Because we are using silent stuffed hens for this work, the learned preference demonstrated in a two-choice test must be based strictly on visual cues. That there is something species-specific supplied by the social interaction, and not merely the visual appearance of siblings, is indicated by the finding that dark mallard ducklings or yellow Peking duckling (domesticated mallards) work equally well as sib partners to consolidate the learning of parental visual characteristics. I use the term consolidate because it is the social interaction with siblings that occurs after the learning or imprinting trial with the hen that is effective. Rearing with siblings before the maternal imprinting trial neither helps nor interferes with the visual learning of the mother's characteristics. It is some as yet unidentified feature of the necessarily social interaction with siblings that is responsible for the consolidation of the learning, because otherwise imprinted ducklings that are kept in a transparent container in a social group (and thus cannot interact with their siblings) do not exhibit the learning of the hen's characteristics. In addition, there appears to be an optimal brood size, in the two ducklings, one on one, is not sufficient and group sizes larger than eight (twelve to sixteen) are not as effective for long-term retention as are groups of eight.

In conclusion, both in the auditory and visual modalities, young ducklings require the highly specific ingredients afforded by siblings of their own species if they are to manifest their usual or species-typical auditory and visual competencies in becoming attached to a hen of their own species. In the evolution of species-specific perception and learning, natural selection has involved a selection for the entire developmental manifold, including both the normally occurring extrinsic as well as the intrinsic features of ontogeny.

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Phenogenesis—An Integration of Psychological Evolution and Developmental Theory

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Ecological psychology recognizes that animals have evolved to cope psychologically with the demands of particular environments, and analyzes their psychological abilities in relation to those environments. It is therefore an evolutionary as well as an ecological discipline; indeed, one might anticipate the emergence of an evolutionary psychology that combines the insights of ecological psychology and evolutionary theory to yield an account of the evolution of psychological capabilities. However, any such account must go beyond the almost exclusively genetic focus of contemporary evolutionary theory and consider in greater detail the nature of change in the phenotype over evolutionary time. Almost 40 years ago, Dobzhansky defined evolution as "a change in the genetic composition of populations," and most authors since Dobzhansky have modified his definition only slightly. Genetic change remains at the center of evolutionary concerns.

The genetic emphasis of modern evolutionary theory stems from the "modern synthesis" of Darwinian natural selection and Mendelian genetics during the 1930s and 1940s. The insights generated by the neo-Darwinian theory that emerged from that synthesis have been profound, but is has gradually become clear that the account it provides of genetic change in populations do not translate easily into accounts of phenotypic change. If we are to explain, for example, how the visual control of locomotion has evolved in various species, we cannot appeal simply to changes in the relative frequencies of alleles at one or more genetic loci. Something must be said about change in the phenotype whose characteristics include that perceptual ability.

The relation between genotype and phenotype is established by the mechanisms of development, and an important recent effort in evolutionary biology is to clarify the role that development plays in evolutionary change. The likely importance of such a role was recognized by Darwin, but development received little attention in the formulation of neo- Darwinian theory. In recent years, the issue has resurfaced, and the importance of developmental considerations for explaining evolutionary change is more widely appreciated. For example, it has frequently been proposed that relatively minor changes in regulatory genes may have far- reaching effects on the phenotype because of their control over large portions of a "genetic program" that itself controls development. It has also been pointed out that development may constrain as well as promote evolutionary change in the phenotype. For example, the nature of morphogenesis limits the range of anatomical forms that can exist in a species, and thus the variation in anatomy on which natural selection can act.

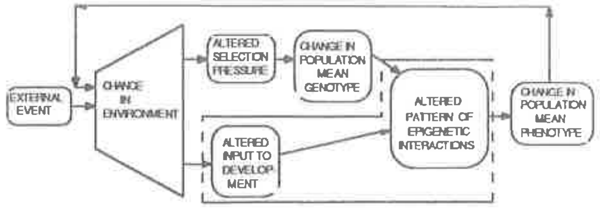

Figure 1. The process of phenogenesis includes both the genetic change that is the focus of neo-Darwinian evolutionary theory and extra-genetic change (within dashed lines) produced by the direct action of the environment on the developing phenotype. The consequences of both kinds of change are integrated by the set of epigenetic interaction (including genetic, environmental, and other components) that give rise to the phenotype.

Such arguments have encouraged the inclusion of the notion of a developmental program between genotype and phenotype; the details of execution of this program provide a mapping between change in the genotype (resulting from natural selection, mutation, and other evolutionary processes) and change in the phenotype, and help to explain why genetic change does not map simply on to phenotypic change. Although the inclusion of developmental considerations such as these is an important step towards unifying genetic and phenotypic levels of evolutionary explanation, the notion of a developmental or genetic program raises serious conceptual problems. Programmatic views of development, in which the developmental process is seen as being controlled by information encoded in the genes, have long been popular in both biology and psychology, but they fail to provide an adequate account of development. Development is an interactive process, involving elements (such as genes) that act from within the organism, but also external influences, provided by the environment in which development occurs. From this interactionist perspective, genes play an important role in the development of the phenotype, but one that is not properly viewed as programmatic.

If the interactionist view is correct, then changes in the phenotype over evolutionary time cannot be accounted for solely by postulating genetic change in the populations. Whatever genotypes exist by virtue of some history of natural selection, they develop phenotypes as the result of interactions that occur between the developing organism and its environment. Phenotypic change is brought about both by changes in the distribution of genotypes in a population and by changes in the distribution of environments in which the members of the population develop. The developmental challenge to evolutionary biology is to integrate the effects of natural selection among genotypes and the effects of changed environments on phenotypes so as to provide a theory of how phenotypes change over evolutionary time—a theory of phenogenesis, of which evolution (the process of genetic change defined earlier) is a component (see Figure 1).

The recognition that phenogenesis includes both change due to alterations in genotype and change due to the effects of an altered environment is likely to be particularly important for theories of behavioral and psychological evolution. Behavioral and psychological functioning is known to be profoundly influenced by environmental contributions to development, especially experience and opportunities for learning. Such influences are so important and so widespread that any theory of the mechanisms by which psychological capabilities appear and are modified over evolutionary time must consider the role played by direct modification of the phenotype by changed circumstances of development, as well as the selective effects of differential reproduction of various genotypes in the population. It may be objected that such direct (or extra-genetic) environmental modifications are not of any evolutionary significance unless they become "genetically assimilated"; only then could they be said to evolve as their genetic basis is modified by natural selection. This argument implies that it is legitimate to distinguish two kinds of character in the phenotype: those that result from the direct action of the environment on the developing phenotype (acquired characters) and those that are specified by the genotype (inherited characters). But his argument is precisely that against which the interactionist view of development is directed. If it is conceptually unacceptable to distinguish acquired from inherited characters in development, as the interactionist view asserts, then it is equally unacceptable to make that distinction in evolution. The concept of phenogenesis, a process in which both genetic and extra-genetic changes contribute to the modification of the phenotype, transcends the distinction between inherited and acquired characters. The challenge, both theoretical and empirical, is to integrate evolutionary (selective) and developmental influences in a way that respects the developmental integrity of the phenotype.

Taxonomies of Organisms and Environments: An Ecological Perspective

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The taxonomic systems used by ecological scientists to classify organisms or environments should be ecological; that is, they should reflect the interrelations between living organisms and their surroundings More specifically, truly ecological taxonomies base their classification on ***functional specification*** of organisms' potential and actual functioning and the ability of various environments to support significant biotic activities.

Many systems of classifying organisms, including systems used routinely in psychology and ecology are not ecological in this sense. These non-ecological taxonomies classify organisms without reference to their relationship to an environment, or classify environments without reference to the organisms inhabiting them. Prominent examples include the widely used taxonomies based on genetic or phylogenetic relationships. While non-ecological taxonomies can help achieve a variety of goals (such as the creation of a consistent nomenclature for organisms), their usefulness in ecological sciences is limited to the extent to which they fail to reflect the functional similarity of organisms and the affordances of environments.

Although functional similarity usually covaries with genetic similarity, it is also often the case that such taxonomies poorly reflect organisms' functional relations with their environments. Perhaps the most striking cases are metamorphic species, but even in most non-metamorphic species important functional diversity may exist due to development, genotypic variation and phenotypic variation induced by environmental influences such as disease or learning.

A number of taxonomies may be referred to as "semi-ecological". In such taxonomies, organisms are classified with reference to some environmental factor(s) *or* the environments are partitioned with reference to some organism(s), but no mutually referential pair of taxonomies is used. Instead, criteria for classifying either environments or organisms are selected arbitrarily or, at least, without reference to an biotic or environmental component. Several common concepts in ecology fall into this group, including (geographical) *range*, *habitat*, and *community*.

Truly ecological taxonomic units clearly reflect the activities and functions an organisms may exhibit in a given environment and, concomitantly, what environmental characteristics support these potential activities. We can refer to the groups of environmental characteristics supporting the functions of an organism as an "ecological niche" or "econiche", for short. Thus, an organism's econiche consists of all of the conditions involved in supporting the vital activities of that organism. From this perspective, an econiche necessarily implies a functionally specific type of organism, and a class of functionally similar organisms implies a special econiche.

Thus, the reciprocal of an econiche can seldom be defined along traditional taxonomic lines (e.g., species, genus); instead the true reciprocal of an econiche is a *functionally specified taxonomic unit* (FSTU) (Alley, 1985). Since the environmental support for the organismic activities of a FSTU is the econiche of that FSTU, it can be said that an econiche consists of the affordances for a FSTU (Alley, 1985). Although ecologists have occasionally recognized advantages of purely functional taxonomy (e.g., in delimiting "ecological equivalents"), both they and comparative psychologists have relied too heavily on traditional taxonomy.

This perspective implies a number of things about affordances and econiches. For instance, we cannot sensibly speak of "empty econiches" any more than we can speak of affordances without reference to a FSTU. Second, affordances need not have an informational aspect; for example, a specific food item may actually support the sustenance of a FSTU without that FSTU being able to detect that affordance. Third, there are both potential and actual affordances. Fourth, the grain-of-analysis applied to descriptions of the functional characteristics of organisms can and should vary so as to define FSTUs varying in inclusiveness. Specifically, the grain of analysis used to define econiches and FSTUs can vary along three dimensions: (1) degree of functional similarity; (2) span of ontogeny; and (2) phylogeny.

The relative importance of psychological factors in determining organism-environment relationships will vary across FSTUs and econiches. Factors producing this variation include variance in: the amount and type of information available in the environment, the FSTU's capacity for action, the distribution of vital resources, and environmental stability and predictability.

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The Self: An Ecological Approach

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An ecologically based account of the self must begin with the kinds of information that are relevant and with what they specify. In this vein we can distinguish five kinds of self:

-*the ecological self* that is specified by optical flow fields and many other kinds of visual and non-visual information, some of which specifies one's own actions: I am the person *here*, doing *this*, at this particular point of observation in this particular environment. This is the self that J.J. Gibson had in mind in insisting that all perception is co-perception of self and environment; it is related to James' "bodily self" but more richly specified.

-*the interpersonal self*, which appears from earliest infancy just as the ecological self does and is specified by time-locked, species- specific signals of emotional rapport and communication. In this process, which Colwyn Trevarthen calls "primary intersubjectivity," we are aware of ourselves as targets *of* and participants *in* emotional interactions: I am the person engaged in this particular personal interchange, here and now.

-*the extended self*, based on our personal memories of our own anticipations of the future: I am a person who who did those things, had those experiences, has these plans. When a close friend dies and we say "It is as if I had lost a piece of myself," the lost piece is from the extended self.

-*the covert self*, which appears when the young child first notices that some of his experiences are not directly shared by other people: I am the person who feels this unique and particular pain. These experiences—sensations, thoughts, dreams, etc.—are valid and important, but we must not make the Cartesian mistake of treating them as the only and ultimate criterion of self.

-*the conceptual self*, based on our general concepts of human nature as applied to ourselves. Like most other natural categories, concepts of the self draw their meaning from the theories in which they are embedded—in this case, on socially established theories that may be widely shared but are not necessarily explicit. Theories of the self differ widely from one culture to another, and even within a given culture. We may believe that there is an inner self which will survive bodily death, for example, or a morally responsible self that must be held accountable for one's actions, or an unconscious self that controls us despite our best intentions to the contrary.

These aspects of the self are not usually experienced as separate because perceptual information holds them together: I can see that it is I, here, who am engaging in this interaction or thinking these thoughts. (They can, however, become partly separate in some forms of pathology.) All are of fundamental importance: the sum total of the self includes *percipio, amo*, and *remniscor* as well as *cogito.*

Hartford
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On Relating Dynamics and Symbols

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Formal dynamics is abstracted from the concepts of forces and motions that are perceived as continuous and rate-dependent. Formal symbol systems are perceived as discrete and rate- independent. The relations between dynamical forces and motions are generally regarded as naturally lawful, whereas the relations between discrete symbols are largely chosen by rules. We think of natural laws as operating inherently, at all times and places, and with no possibility of error. In contrast, rules operate by specific mechanisms, only locally, and always with the possibility of error.

It is no wonder then that the history of philosophy, logic, mathematics, and science is strewn with failures to coherently relate dynamics and symbol systems, and yet we often overlook the incredible fact that our only representation of dynamics is by formal symbol systems. This "unreasonable effectiveness" of symbols in describing dynamics, as Wigner expressed it, has nevertheless been at the root of many apparent paradoxes from Zeno's discrete description of motion ("That which moves does not move by counting." Aristotle), to Bohr's particle-wave complementarity in quantum mechanics.

I believe a strong case can be made that the roots of this irreducible complementarity of dynamical and symbolic thought is not to be found in either the objective order of nature nor in the abstract formalism of mathematics, but rather in the primitive modes of perception of living organisms. I would place the direct perception of continuous motion and of discrete objects in the same category as the direct perception of smell, heat, light, sound, etc., only at a somewhat higher evolutionary level of neural organization. The extreme sophistication with which we now view dynamics and symbols in scientific discourse should not obscure the fact that the perceptions of motion and objects have been essential for survival for many hundreds of millions of years.

This irreducible primitive complementarity of motion and objects raises the question of how an organ like the brain could be organized so as to perceive both the continuity of motion and the discreteness of objects in one common structure. Continuous dynamical models can generate discrete properties only through singularities (instabilities, bifurcations, catastrophes), while discrete models can generate continuous properties only through statistics (averages, probabilities, distributions). These theoretical properties lend weight to the experimental evidence that concurrent, distributed "neural net" models of the brain are potentially far more explanatory than the established information processing, discrete symbol systems.

These models can be used to illustrate naturally both dynamical instabilities and statistical averaging. However, to realize this explanatory potential, neural net models must be understood as dynamical analogs that operate outside the realm of effectively programmable computation. The fact that all dynamics can be represented on discrete, sequential computers with "unreasonable effectiveness" does not eliminate continuous, coherent motions as an irreducible, complementary mode of perception. One must not forget that in spite of the "universality" of symbol systems, all computation is ultimately dependent on the lawful dynamics of its hardware.

I.S.E.P. NEWS & WEATHER

By Jean-Paul de los Angeles

From around the corner and around the world, ecological topics make news. However, even the scrupulous news hound may have difficulty identifying the good news, the bad news, and the ecologically valid news. To assist you in the quest, our crack team of reporters and stringers brings you NEWS & WEATHER of ecological Psychology. The particular orientation of topics, the slant of the stories (or, is it TILT?), varies from one installment to the next, but the general perspective is the familiar one.

SCENARIOS FOR DISASTER—What are you worrying about? Nuclear war? Nuclear waste? The poisonous lake in Cameroon? Killer bees? AIDS? Mullahs? Unauthorized use of your credit cards? Authorized use of your credit cards? Your concerns may be justified, but perhaps would be more challenging and fulfilling if you expanded the scale. Now, we all know how body-scale paranoia really holds on tight, but, if we can learn from the example of the American Astronomical Society, we may improve our ecological concept and drive ourselves crazy with worry at the same time!! They worry about end-of-the-world catastrophes, an ecological circumstance few Psychologists have pondered. And, the astronomers have a highly developed notation, which should really excite the fans of formalism among us. Here is what the astronomers say.

At the June meeting of the AAS, a couple of possibilities were discussed pertaining to the extinction of our world, which, of course, includes the I.S.E.P.: the supernova model, the quasar model, and the galactic collision model. All have the ring of truth, though my favorite is the supernova model. Let's say that a giant star, no closer to us than 300 light years, explodes. We would get a blast of UV and X-rays that would destroy the Earth's ozone layer, and then UV from our own Sun would fry the planet. Extra Crispy!! You would need a #16 Ban de Soleil year- round. So would everything.

By comparison, the quasar doesn't really figure as much of a threat. First, imagine that our galaxy is unexceptional, and therefore that it has a black hole near its center. Like black holes everywhere, our Milky Way black hole is drawing in matter. This inrushing matter will eventually produce X-rays as the consequence of gravitational and frictional interactions. The radiation will excite interstellar gas to produce X- rays, too, though radiation from the quasar will also reach the Earth's atmosphere directly. Luckily, the envelope of gas surrounding the planet mostly attenuates X-rays, or it will if you stop using spray cans. Net result: No big deal for terrestrial ecology. The dynamic of this catastrophe sort of rules out living on thin atmosphere worlds (like Mars, or the Moon) but you wouldn't like life much there anyway. Exo-burritos are supposed to be awful.

If neither supernovas nor quasars engage your worry faculty, try pondering the galactic collision tableau. That tune goes something like this: Astronomers now believe that the Milky Way and the Andromeda galaxy compose a binary system that is unstable, and presently merging. Estimated time of arrival is about 4 billion years from now, so there is still plenty of time to finish reading *Dr. Faustus.* If the solar system is disrupted by a close pass of a massive body, the Earth will probably leave its orbit for a dark and stormy ride through space. But then again, space is pretty empty, and close passes are rather unlikely, according to this model. So even with galaxies colliding, the appearance of the heavens may change and our descendants can still go on digging the scene in whatever crazy or fabulous way they want.

How will the world end? Now you know what the expert worriers have to say on the matter. Why not incorporate this new and grandiose ecological theme into your own obsessional ruminations? It works well as a way to drive yourself crazy, and is a harmless substitute for genuine intellectual activity.

MISPERCEPTIONS OF THE MONTH— ISEP member PER reports a weird observation, in which a city planner referred to a set of traffic lights under open-loop control as a "cognitive" system. Incredible! PER urged the offensive psychobabbler to re-educate his attention!

From a reader: Dear Dr. Ecology, While riding in the family van recently, my four year old daughter said that the moon was following her. Earlier that day she said that the trees were turning as she rode past them. Help!

Yours,
Disillusioned

(Don't forget to send your observations of misperception to I.S.E.P. News & Weather, c/o Dr. W.M. Mace at the familiar address.)

ELECTRONIC MAIL

I have promised to print a list of electronic mail addresses. There is not room here, but you can write to me for any that you need. I am WMACE@TRINCC or MACE1@YALEVM (both on BITNET). I have addresses for Akerstrom, Balzano, Bingham, Beek, Ben-Zeev, Bootsma, Brewer, Eppler, Fitch, Givner, Fowler, Hagen, M. R. Jones, Lintern, Mark, Mingolla, Neisser, Reichel, Remez, Rosenblum, Rubin, Saltzman, Scholz, Shapiro, Shaw, Stevenson, Todd, W. Warren, Costall, Good, von Hofsten, Pitcairn, Rogers, Runeson, and M. Smith.

EXCHANGES: FROM INFORMATION TO PEOPLE

From Gavan Lintern: Course Information Wanted

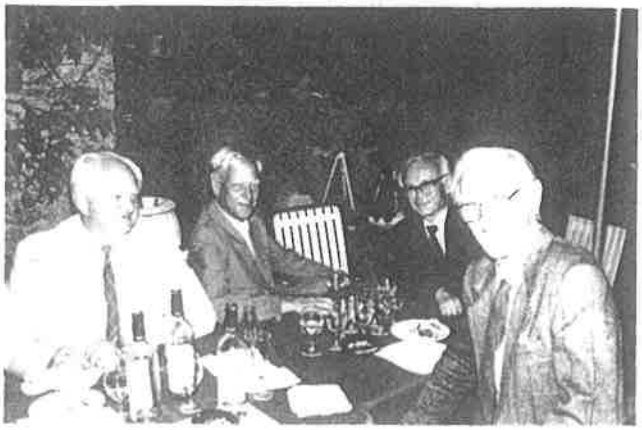
I am planning an undergraduate course in Ecological Psychology for the 88-89 academic year. At this stage, I am unaware of books or articles that might be appropriate for undergraduates. All that I have read seems too advanced. I would appreciate advice on any readily available materials that may be more suitable. Also, if anyone has taught Ecological Psychology at the undergraduate level, and would care to share their course outline, I would appreciate that too. I hope to attend at least one of the next two Perception and Action Conferences, but may otherwise be contacted at the Aviation Research Laboratory, University of Illinois, Willard Airport, Savoy, IL 61874; phone: (217) 244-8637.

From John Pickering: A U.S.-U.K. Academic Exchange

Is anybody interested in swapping departments for about a semester some time in late 1988 or '89?

I'm at the Psychology Department of Warwick University, England where I teach perception, cognition, and consciousness. I want to get better acquainted with the ecological view, and spending some time in the U.S. is going to help here. Equally, as I'm presently the only one in my department pushing an ecological perspective, a fresh presentation of it would be a good idea!

The Fulbright Foundation supports such exchanges, and there may be other sources of support. If you're interested, contact: John Pickering, Psychology Dept., Warwick University, Warks, U.K. CV478L. Telephone: (0203) 523151.



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