The International Society for Ecological Psychology

Serial Number 21 NEWSLETTER August 1994

Regular readers of this newsletter may have noticed that opportunities to read it are not (regular). Numbering issues by volume and number represented the hope that some stable routine would develop; but there is no evidence of any such impending regularity. Therefore, to fully acknowledge the occasional nature of the newsletter, and to allow one to keep track of them, they will be numbered consecutively --- period. A key to all existing newsletters appears on the last page of Volume 5, number 3 (January, 1993). That was number 19. Our last newsletter came out in November, 1993 and was labelled Vol. 6, number 1. It was the 20th newsletter. Hence this is number 21. We'll leave it at that.

FALL NORTH AMERICAN MEETING 1994 SEPT. 29 - OCT. 2 CHANGING ECOLOGICAL APPROACHES TO DEVELOPMENT

Cathy Dent-Read and Patricia Zukow - Goldring have organized a major conference on developmental psychology and ecological approaches to be held at the University of Connecticut at Storrs on the above dates.

To have another ecological meeting this fall in competition with that one seemed both unwise and impractical. As a consequence, we are treating this meeting as the 1994 North American meeting as well.

Posters and Non-developmental research reports

To accommodate people who want to share ecological research, but who are not on the Developmental conference program, we have arranged a poster session to be held in the psychology department at Connecticut (not the building for the Developmental conference). The poster session will begin on Thursday morning, September 29 to encourage people to arrive early. Rooms for spontaneous group meetings will be available. It will be very important to know in advance who will be coming for the poster session. Therefore, please contact Claudia Carello (cespa1@uconnvm.uconn.edu or Bill Mace (William.Mace@mail.trincoll.edu) if you are participating in the poster session.

A formal description of the meeting appears below. Anyone who has not yet registered for the developmental conference should do so at once! Please.

Description of Developmental Conference

American Psychological Association Scientific Conference: Changing Ecological Approaches to Development: Organism-Environment Mutualities - Storrs, Connecticut - September 29-October 2, 1994.

The goal of the conference is to contrast ecological approaches to development that have emerged independently in the areas of direct perception and knowing, epigenetic systems, and dynamic systems. Within these views, ecological refers to a consideration of whole animal-environment systems, that is, how animals and environments mutually determine each other throughout development. The aim is to delineate the advantages and limitations of each approach, so that the unique contributions of such work can be integrated into a more complete and coherent ecological theory of development than now exists in the field of developmental psychology. Participants/discussants include Jane Clark, Robin Cooper, Cathy Dent-Read, Alan Fogel, James Green, Gwen Gustafson, Claes von Hofsten, David Miller, Anne Pick, Nancy Rader, Phillipe Rochat, Ad Smitsman, Ted Wachs, Patricia Zukow-Goldring, Catherine Best, Timothy Johnston, Edward Reed, Karl Newell, Esther Thelen, Michael Cole, and Eleanor Gibson. Contact Cathy Dent-Read, Ph. D., Department of Psychology, University of Connecticut, Storrs CT 06269, (203) 486-3524, e-mail: cdent@uconnvm.uconn.edu or Patricia Zukow-Goldring, Ph. D., School of Social Ecology, University of California, Irvine CA 92717, (818) 905-6293, e-mail: zukow@psych.sscnet.ucla.edu.

We have more of the proceedings from the 1993 Fall meeting at Smith College (October 15- 16) to report.

In the last newsletter, there were abstracts of papers by John Sanders and Chris Pagano. The remainder of those that we have, including a report on the developmental symposium, are in this issue.

ABSTRACTS FROM 1993 N.A. FALL ISEP MEETING

The problem of development in ecological psychology

Panel at the North American Meeting of the Ecological Society
Smith College, Northampton, Massachusetts 01063, USA
October 1993

Eleanor Gibson, Anne Pick, and Nancy Rader, Presenters
Peter Pufall, Chair

Peter. Pufall.
Several years ago it was popular for discussions of development to grapple with, "What develops?" Without exception, answers were framed in structuralist language of a constructivist theory so that the "what" was inevitably located within the organism in some form of representational system. Answers of that kind are unacceptable within ecological psychology. The problem for ecological psychology, then, is to redefine "what" develops within its own theoretical framework in which the "unit of analysis" is the functional fit of organism with environment.. More boldly, it must confront what development is within an ecological perspective, and, for example, how, if at all, it differs from learning? Finally, cans a wholly functional approach coherently integrate conceiving and perceiving?

Eleanor Gibson:
Eleanor Gibson began by declaring that development does not pose a problem for an ecological perspective, rather development is "the greatest asset" of ecological psychology. She supported this claim with *a prolegomena* framing perceptual development and learning within an ecological point of view. The statements constituting her *prolegomena* " are not merely articles of faith...(indeed there is) evidence, both observational and experimental, (which) exists for them. I wish there were time (space) to present it here."

An ecological psychologist's prolegomena for perceptual learning and development

1. Animal - environment reciprocity: the proper unit of study is an animal in the environment in which it evolved, in a reciprocal relationship.
2. Perception-action cyclical relationship: perception and action are interdependent; perception obtains information for action, and action has consequences that inform perception, about both the self and the events that it perpetrates.
3. Learning must be studied in the species-typical environment in the context of development.
4. What is learned, generally speaking, is perception of affordances, i.e., to perceive what an object or event or layout affords for action in relation to oneself.
5. When learning occurs depends, in part, on maturing action systems: they are a rate-determining factor for perceptual learning and development.
6. How learning occurs is by exploratory use of these developing systems and observation of the consequences.
7. Exploration is a natural function of the developing system, as much as breathing. An animal spontaneously forages for information about self and environment.
8. Learning to perceive and instantiate any affordance is an example of gaining *control* of that behavior. It can henceforth be intentionally used.
9. As control increases, so does *prospectivity* of behavior, its anticipatory aspect.
10. As exploratory range, control of new affordances, and prospectivity increase, so does the *potential flexibility* of behavior.
11. Behavior occurs *with* a *task setting*. Tasks *are set* naturally in early life, by *the* demands of maintaining life and *growth* within the niche, *e.g.*, breathing, eating, maintaining comfort, and making contact with the world. They differentiate with development.
12. Tasks expand as new affordances are learned, as exploratory action broadens, and as social contacts are made. Goals differentiate with task expansion.
13. Means to ends are learned as goals differentiate (a kind of higher order affordance relationship). Selectivity is increased as means-end relations are learned.
14. As means-end relations multiply and differentiate, behavior becomes *increasingly flexible*.
15. *For* transfer of means to occur, there must be affordance and task linkages. This generally involves active perceptual learning (affordance linkage to consequences).

Eleanor Gibson ended by stating: "These propositions are truly *prolegomena.* They are presented to illustrate how I propose that an ecological, developmental approach can lead eventually to a grasp of psychology that encompasses what I consider the hall marks of human behavior: control, prospectivity, flexibility, communicative creativity, and retrospectivity. Concepts invoked in studying perceptual development are going to useful in expanding our grasp of many domains: language development, retrospective activity, and problem solving, for example. A framework for them must have reference to observables in behavior, to be within the field of psychology."

Anne Pick.

Anne Pick considered "whether an ecological perspective frames the relation between development and learning in a manner different from most constructivist theories?" and "whether an ecological view coherently portrays the relation between perceptual and conceptual development?"

An ecological perspective on the relation between development and learning: Constructivist as well as "new nativist" views frequently frame, at least implicitly, the relation of development and learning as a dichotomy, between what is *given,* e.g., innate representations, and what is *learned,* i.e., constructed. Problems with this formulation include the following: (1) Once a behavior or characteristic has acquired the label *innate,* it is assumed to be explained, and not further question is asked about its development or emergence. (2) Dichotomies remove the child from the environment, and direct the focus away from the child-context as a basis for studying development--or at least invite treating the child and environment as separate components. (3) Such a view of development assumes the source of organization in behavior is internal -- mental models, neural connections, innate constraints, and the like. A view of development implied by the ecological approach and made explicit in some of Jackie's prolegomena, is one in which learning is fundamental, emerging from the consequences of exploratory use of perception action systems. Organization in development emerges as a natural outcome of gaining control of behavior and intentional use of perception-action systems.

An ecological perspective on the relation between perceiving and conceiving: Constructivist approaches assume that perceiving and conceiving are discontinuous, an assumption based in beliefs about the nature of perceiving and the insufficiency of information to support perceiving. The ecological approach entails different hypotheses: (1) that perceiving and conceiving are continuous, and (2) that conceptual change during childhood is based in children's discovery of objective properties of the world. These hypotheses are based in the ecological view of information, and the theory that perceiving is based on information, not sensations.

Nancy Rader

Nancy Rader views development as posing a challenge for ecological psychology. While an ecological perspective characterizes development in terms of the discovery of information, one must take into account changes that are occurring in the organism in order to understand the process and consequences of such discovery. In other words, we need to analyze adaptations that underlie discovery, the effects discovery has on the organism, and the effects the state of the organism has on the response to that discovery. One approach is to study instances where one directly observes changes in behavior over time under circumstances in which the information available is constant.

Two experiments are described to illustrate organismic adaptation, one in terms of the discovery of information and the other in relation to changes in "response potential." In a study looking at early infant reaching, infants view an object at Time 1, taste it at Time 2, and view it again at Time 3. While the information present at Time 3 is identical to that at Time 1, the infant's response to the object is quite different. If the object tasted sweet, infants as young as 3.5 months increase reaching; whereas if the object tasted bitter, they decrease reaching. Even infants as young as two months change their hand behavior following the tasting experience. What has changed? In this case, the perception of the object at Time 3 now includes its taste history; the perception of its affordance has changed as the result of picking up the relationship between visual and olfactory information at Time 2. In this case, the change is no different from what we would expect in a mature organism, and can be characterized as perceptual learning. There is no difference in potential to respond at either Time 1 or Time 3. A new opportunity to discover a "hidden affordance" has intervened and caused a state change in the organism. The hidden affordance of taste will now be "perceived" whenever the object is

presented.

In another series of studies, infants' responses to a visual cliff were studied in relationship to their potential to inhibit behavior. Infants who cross the visual cliff at 9.S months discriminate between the two sides in terms of looking behavior, for example, but they fail to respond adaptively in terms of locomotor behavior. They have had ample experience to discover the affordance, with many more falls recorded than their avoiding counterparts. By about 12 months of age, the crossing infants typically do avoid the cliff. How are we to account for this change in behavior to the same information? Their performance on a delayed response task (which is like a Piagetian AB task, with a delay added) indicates that these infants have a slower pattern of frontal lobe maturation than those infants who avoid the visual cliff. In this case, their eventual avoidance of the cliff at an older age appears not to derive from the discovery of the information specifying an edge. Rather, it comes about as the result a change in their potential to respond to it adaptively, as mediated by maturational changes in the nervous system.

Questions:

(Questions and responses are generally paraphrased, the quotation marks are used to set off question and answer.)

Q. "Professor Gibson, can your ideas about learned flexibility be understood in terms of a habit family hierarchy?"

EJG "The concept of flexibility, which I think we've neglected and need to work on, was worrying Hull when he wrote his papers on The Habit Family Hierarchy. The problem was right on the mark, but his mechanistic solutions are are not acceptable to me"

Q. "Ecological psychology seems to meet the standard of common sense, what are the viable competitors?"

AP "I could agree more with your view of ecological psychology, however, the cognitivist or constructivist view to which I referred has turned perceiving and conceiving on its head, imputing mentalistic models that structure and interpret sensory information. Their claim that we compute and not detect reality indicates how different their view is from a realist's."

Q. "Assuming information is constant is difficult for me to understand."

NR "Information, as it is structured by the environment is constant. What information we pick up varies over perceptual learning and development, both of which imply changes in "response potential."

Q. "We should not hastily invoke innate mechanisms to explain change? There are examples in complex systems ranging from social ethology to physical systems in which subtle changes in a variable within the system appears to regulate the organization of the system."

AP."I could not agree with you more. The constructivists to whom I referred willingly introduce innate mechanisms or constraints to give an account of infant performance that might be regulated by perceptual information."

NR "I introduced maturation in an effort to get us to examine changes in how the organism acts on, and therefore partitions information, not so that maturation is programmatic and certainly not to suggest that there were innate mechanisms which representational structure information."

Q. "Is flexibility and transfer facilitated by the kind of social structuring currently studied within a Vygotskyean perspective by people like Jim Wertsch and Jon Valsiner?"

EG "I assume that development is facilitated within a social context, and that flexibility and transfer would be affected by social transactions. However, I do not think that perspective can provide a full account of how children learn from perceiving. There are innumerable occasions when they explore spontaneously and learn successfully on their own."

Man as Scientist: The Concept of Human Nature Operant in the Developmental Psychologies of Bain, Preyer and Koffka

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This paper resulted from the author's reading of Kurt Koffka's developmental psychology text, The Growth of the Mind (1921), as back- ground for the Smith College Ecological Psychology meeting. What was fascinating was Koffka's puzzling affirmation of knowledge contained in subjective perspectives, the views of mothers, immediate and emotional, even when he realized that they did not conform to scientific standards.

An important source for constructing a secular

scientifically - oriented natural history of the individual was George Grote's politics. What the radical politician objected to most strongly was the political misuse of threats of punishments in the afterlife for misdeeds in this life to promote civil obedience. Instead he urged citizens in 1822 to attend to the natural consequences of their actions and to seek in this world to ameliorate the poor's sufferings. This radical orientation to the present helped prepare the way for the scientific study of individual human behavior. Grote served as mentor to the young radical, the Scotsman Alexander Bain, encouraging him as he began his attempts to write a natural history of the individual human mind.

Bain's first developmental writing was entitled "On Toys" (Westminster Review, 1842). Always stressing the importance of experience for development, he underlined the role of the "handling" of objects for learning. That a child in moving a compass leg by leg across a map could imagine a giant striding across countries reflects its creative, as well as physical, grasp of objects. Girls, if given a richer range of experience, might well attain greater mental achievement. Bain envisaged the mature adult as a common sense practitioner of scientific method, forming hyptoheses he called beliefs and modifying them as a result of the consequences of experience ("Human Mind," 1849).

If Bain considered the adult individual's proper action as scientific, William Preyer fully embodied that practice when he created the first development child psychology text, The Mind of the Child (1881). The The professional researcher/father entered his son Alexander's nursery three times a day each time for twenty minutes prepared to record his observations. He even made of the nursery somewhat of a laboratory by allowing his son over a span of several weeks to play with a mirror, observing him developing a sense of self. Alexander progressed from realizing that he had direct control over some of the moving objects in the mirror, that is, over the parts of his own body, to recognizing in the mirror the image of his father who had approached silently from behind him and turning to face him in actuality. the son was a budding empiricitst, before the age of three, distinquishing appearance from reality, aware of the actual location in space of his father's physical body.

Preyer's chronological schedule of nursery visists reflected in miniature the enormous concurrent cultural shift in which Greenwich standard time and responses to time schedules were imposed upon the world. By 1820 behaviorism's domination of American experimental psychology had trivialized action as a source of knowing, by defining it in terms of automatic responses to external cue - to - action stimuli. Koffka might well have said to the behaviorists: the unexamined life does not warrant experimentation.

Information in Heider's Dynamic Theory

Darren Newtson
University of Virginia

At the end of his career, Fritz Heider identified himself as an ecological psychologist. The validity of his claim follows from his lifelong focus upon the analysis of the distal stimulus in object and social perception. Heider reasoned that if a variable in the stimulus field can act as a medium to specify an invariant property of the environment, then behavior, which varies, can specify a constant psychological state in another, such as an intention. If this is true, then at least part of our mental processes are tangibly perceptible to each other.

Heider then proceeded to analyze the information available in ongoing behavior that would specify such perceptions. He identified two such properties: one, termed equifinality, is defined as the invariance of outcome over variation in local conditions of action; and two, what he termed "local control" or sensitive adjustment to variation in local conditions. If these properties are present, then a causal invariant is specified in the mind of the actor; if it is absent, the cause of the behavior is located in the environment. This is the class of invariants that distinguishes action perception from movement perception. Cognitive theorists subsequently treated these properties as the data to be entered into post-perceptual inference processes; thus Heider's reasoning was used as the basis for the formulation of attribution theory. Heider, however, regarded causality in behavior as directly perceived; while he gave an algorithm for the combination of information to unambiguously specify causality, he noted that, as he put it, the equation IS the data.

Heider's second distinctive contribution was balance theory, the important pre-cursor of cognitive consistency theories. Heider's central focus was upon how the perceptual system works, and he regarded it as a general

maxim that all psychological processes, from perception to abstract inference, were organized to enhance adaptation of the organism to the environment. Thus he interpreted Gestalt principles as biases that existed because they enhanced the veridicality of perception. He himself rejected the Gestalt view of object perception.

His balance theory followed from his observations of affective invariance in human social relations. The balance principle is in fact a highly robust property of social affiliation, and attempts to find "unbalanced" relationships for study have failed due to the scarcity and instability of such relationships. Given this systematic bias in the ecology, Heider reasoned that persons would show similar biases in cognition about relationships. The result was the first theory of "cognitive" consistency.

His interest in the analysis of "common-sense" psychology followed from similar assumptions: one should study "naive phenomenology" in order to gain insight into the nature of human social behavior. Perceptual bias could inform the investigator of invariants in the environment. For this reason, his classic analysis of social perception is titled THE PSYCHOLOGY OF INTERPERSONAL RELATIONS, and not "the psychology of interpersonal perception."

Young James J. Gibson's, Alexander Bain's, and the New Realists' Program

Robert E. Shaw
The University of Connecticut

What was the Zeitgeist for young Gibson at Smith regarding the mind - body problem? Distinctions were being made and polemical arguments offered for neutral monism versus a sundry of other views (e.g. mind - body interactionism, parallelism, and identity). The New Realism of E. B. Holt and others, with its core of radical empiricism from William James, opposed the Idealist doctrines asserting that

1) the known or perceived object is dependent for its existence on the act of knowing, and that

2) the immediately perceived object is a state of the perceiving mind (i.e., a representation).

If, as Locke's philosophy implied, we can know only the immediate contents of consciousness, then our thoughts can spawn new thoughts only about themselves -- not about anything outside themselves. Thus no *intentionality*, no 'aboutness' relation -- all thinking ends in an epistemic 'cul de sac' (e.g. Fodor's 'methodological solipsism').

Berkeley correctly denounced Locke's notion that material objects are meaningless when treated as independent of experience. But he failed to recognize the important distinction within experience between the *person sensing* and the *object sensed*. As Bertrand Russell argued, this error is like confusing Johnny's being a nephew with his being a person; being a person does not depend on being a nephew -- although there are no nephews who aren't persons.

This confusion is indicative of Idealism which is based on a Doctrine of Internal Relations as opposed to Realism which is based on the Doctrine of External Relations. The Doctrine of Internal Relations is most dramatically exemplified by Hegel's Absolute Idealism (i.e., "There is no truth except the whole truth!" "True knowledge can never be attained by examining anything in isolation; it only appears as it truly is in the context of the whole to which it belonds"). By contrast, the Doctrine of External Relations asserts that no object is essentially changed by its participation in the whole, save with respect to the functions it assumes by the place it occupies in the whole. Individual identity is an objective rather than a subjective property. Although causal relations between objects may change their relations to other objects, no object's identity is compromised by such relations.

The philosophy of Alexander Bain, the Scottish psychologist, set the stage for radical empiricism, pragmatism, the New Realism, and functionalism, and thereby strongly influenced Gibson's later development of ecological psychology. Like Leibniz, Bain (1875) endorsed mind - body parallelism, contending that sensory inputs cause consomitant physical and mental reactions that do not interact (contra Descartes). Before james, and anticipating Gibson, Bain recognized the existence and the functional holism of the perceiving - acting cycle. Young Gibson reproduces in his graduate notebooks a diagram possibly derived from Bain, indicating his lifelong attempt to rework the New Realists' program so as to avoid the fatal flaws that led even its most devoted adherents to abandon the view (e.g. Holt). His degree of success is evaluated.

Visual guidance of rapid targeted reaching

Geoffrey Bingham, Jennifer Romack & Michael Stassen
Indiana University

Movement scientists have long debated the coordinates in terms of which reaching movement might be planned and then controlled. Some have argued for joint based coordinates while others have advocated hand based coordinates. The latter have been supported as being more convenient from the perspective of visual information for planning and guidance. These have usually been described as Cartesian coordinates anchored in the environment, but the variables that are actually used for visual guidance remain to be discovered .

We have investigated the visual information used to guide rapid targeted reaches. We have derived two Tau-type variables defined in terms of spherical coordinates anchored in the optic array. The first optical variable, $\tau_\phi = \phi/[d\phi/d\tau]$, is the (momentary) time until ϕ equals 0, that is, the time to close the visual angle between the hand and target. The momentary value of τ_ϕ, which estimates this time is only accurate if $d\phi/dt$ is constant. Of course, $d\phi/dt$ varies along a unimodal profile characteristic of limb movements. Others hypothesized and tested the use of this variable (Bootsma & Oudejans, 1993; Lee, 1993). The second optical variable, $\tau_R = [1-S(I_T/I_H)]\ H/[dH/dt]$, is the (momentary) time until R equals 0, that is, the time to close the radial distance to the target (without regard to direction)[1]. I is image size (of either the target or the hand) and S=hand size/target size. Scaling by S means that the observer must have an appreciation of the target size in hand size units to be able to use τ_R. This variable has not been considered previously by other authors .

We suggest that these variables are used to guide reaching in the context of a "Constant Tau Strategy." We hypothesize the use of a constant τ_ϕ during the decelerative portion of the fast phase of a targeted reach with constant $\tau_\phi \equiv 100$ms. A general level of constraint on τ_R that allows for variations in the amount of overlap in the control of the two τ variables is to use $\tau_R \geq \tau_\phi$ with $\tau_R \rightarrow \phi$ as $\phi \rightarrow 0$. In a successive strategy, τ_R would not begin to approach τ_ϕ until ϕ was 0.

Experimental investigation:

4 participants made rapid reaches to place a stylus into a target hole in a 5cm disk. Seated participants directed their reaches from a "launch platform" located next to their hip to a target located in front of, and just beyond and above their knee. Within this abstract we report on two of four visual conditions: A "clear goggle" monocular condition in which the non-dominant eye was covered and the visual field restricted to about 45°. Means computed over blocks of 10 reaches in each condition yield the following.

Goggle condition:

1. Mean τ_ϕ trajectories descended to a constant value of about 100ms at a ϕ of about 20° and maintained the value through the end of the fast phase terminated at about 5°.
2. Mean τ_R trajectories remained at larger values than τ_ϕ descending to a value of 200ms at a ϕ about 10°.
3. The coefficient of variation for τ_ϕ, remained at about 10 - 15%, especially during the second half of the fast phase while that for τ_R dropped to about 20% during the final portion of the fast phase.
4. τ - dot_ϕ as well as τ-dot (although we know of no optical correlate) trajectories did not exhibit constancy until the final portion of the fast phase, that is, for $\phi \cong 20°$ to 5° where a constant value of 0 was maintained. τ-dot_R dropped to 0 near the end of the fast phase .

First reaches in displacement condition

1. Compared to relatively straight movement paths towards target in the clear goggle condition, the visually perturbed paths veered, near the midpoint, towards the apparent target location and then veered back towards the actual target location finally reaching

the target.

2. Measurable corrections moving the hand toward the actual target location occurred about 160ms after the hand came into view but 50ms to 130ms before the hand would have failed to occlude the target at its apparent location. Presumably, the visual realization occurred 65ms preceding the measurable correction. If not via failure of anticipated occlusion of the target by the hand, then how was the perturbation to visual direction discovered?

3. The solution, we suggest, is violation of the condition that $\tau_R \geq \tau_\phi$. We found that the τ_R trajectories approached a perturbed τ_ϕ trajectory that we computed using the 10^o displaced target location. Until the correction, the perturbed τ_ϕ trajectories behaved just as the clear goggle τ_ϕ trajectories. The result was that the τ_R trajectories crossed the actual τ_ϕ trajectories. Corrective movements occurred about 80ms after τ_R dropped below τ_ϕ.

These results provide some support for the use of these two τ variables for the guidance of the hand to a target during the final portion of the fast phase of a reach.

A more complete version of this paper as well as a bibliography is available from the authors.

European Workshop on Ecological Psychology - 3

On July 6 - 8, 1994, German members of the Society from Bochum, led by Rainer Guski and Wolf Heine, sponsored the third European conference at a "castle with a moat," as Wolf described it to me in prospect. The program included "bicycling and survival in the Westphalian woods."

The format of the program is one the participants seemed to like very much. There were keynote lectures followed by commentaries on each of the three days. These keynote talks were given by Claire Michaels & Peter Beek, Gregor Schöner, and Gerda Smets. Involved in commentary and discussion of these talks were Reinoud Bootsma, Ad Smitsman, Onno Meijer, Daniela Corbetta & Esther Thelen, Yves Guiard, John Jeka, Patrick Green, and Ruth Kaufmann-Hayoz. The symposia offered were: (1) Social Affordances and the problems of units of analysis, (2) Discrete vs. continuous movement and the dynamical approach, (3) On 'active' touch, and (4) Using information on visual motion: Possible smart mechanisms and ecological tuning.

In addition, one morning session was set aside for "interactive talk."

The program book, full of lengthy abstracts, would be an excellent addition to any library in ecological psychology. Unfortunately, no more are available. Keynote speakers and commentators are willing to expand their remarks into chapter length papers for an edited book, and that possibility being actively pursued by the organizers. You may yet have a chance to share in the excitement of that meeting.

The 1996 European conference will be held in Delft, The Netherlands.

ICPA 8
Marseille
July 9 - 14 1995

Surely all readers of this newsletter know that the Eighth International Conference on Perceiving and Acting will be held in Marseille, France. The dates are July 9 - 14, 1995. Benoît Bardy, the organizer, along with many other active Society members in Marseille, has been moving right along with the organization of the meeting. The final menu of Symposia is now being developed from the initial set of more than 20 possibilities. If you are not receiving communications about this meeting, please contact Ben right away.

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POSTER DEADLINE

From our Amsterdam meeting (1991) on, the Poster books have become a major item. The last one, from Vancouver (see ad later in this newsletter), was published by LEA and can be found in Books in Print, as well as computer databases for library holdings. The arrangement for the Marseille meeting will be the same.

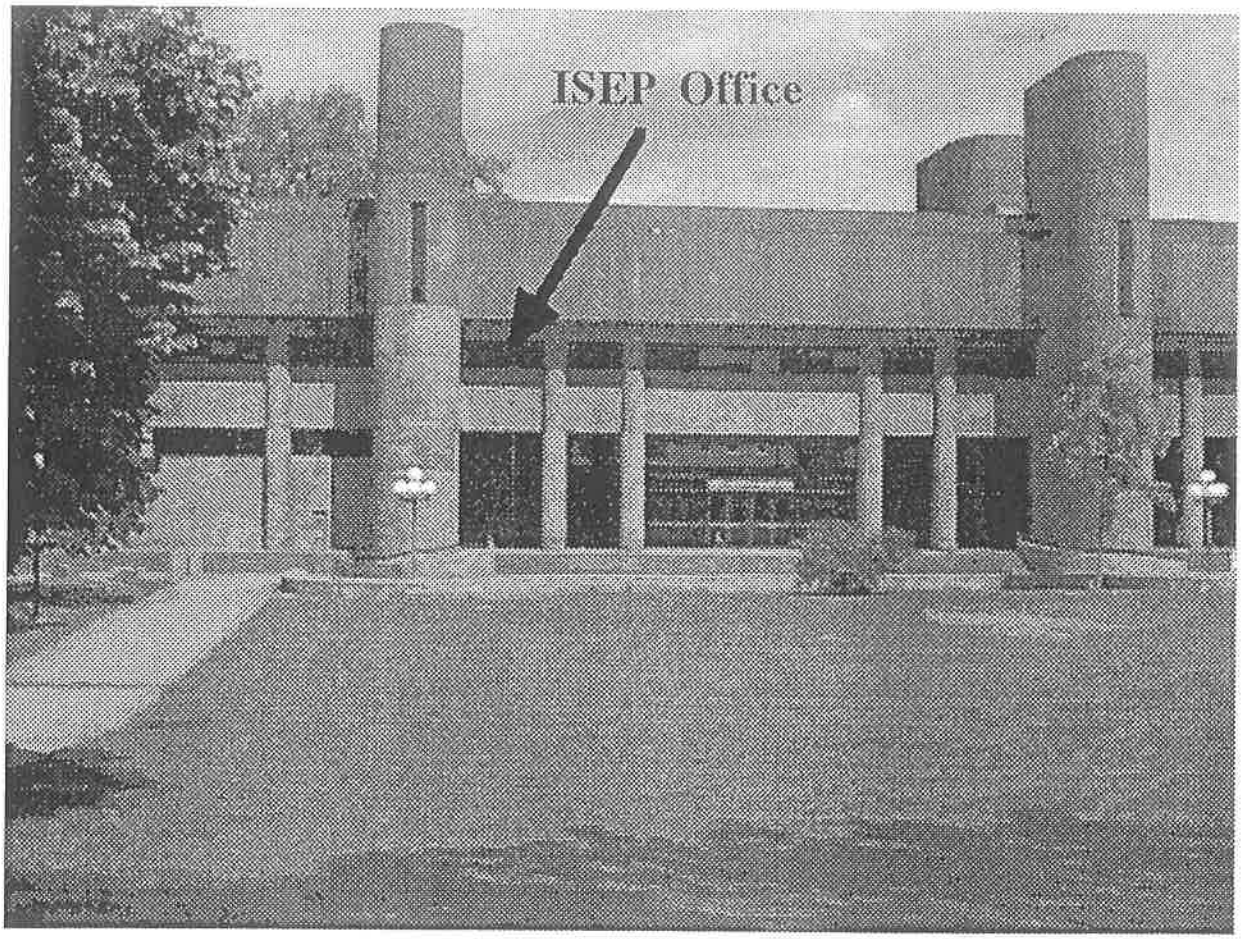
Because the books have to be printed and mailed to France to arrive before the meeting, there will be a very early (near the first of the year), inflexible deadline for poster submissions that hope to be included in this published volume. You'll get something from Ben very soon, but look carefully to see what the deadline is and arrange your production time accordingly.

IBERALL BOOK

Many of you know Arthur Iberall. He was a very active participant at the Vancouver meeting. Ibby is a physicist who has specialized in fluid dynamics and thermodynamics for about 50 years. He's worked for the National Bureau of Standards and has had his own consulting firm. Ibby always has been deeply immersed in solving practical problems (the list of technical innovations he's contributed to is remarkable -- from space suit development to SCUBA to precision scales for weighing astronauts). What marks his work as relevant to ecological psychology is that he sees science as all of a piece ("one science or no science," he says) and he sees no gap between pure and applied work. To physicists with less taste for what is sometimes engineering, his work looks like "dirty physics," a label he wears proudly. He has collaborated with neuroscientists (Warren McCulloch, Rudolfo Llinas), physiologists (Yates, Cardon), other physicists (Soodak), political scientists (Wilkinson), and anthropologists (Arensberg, Moore). Since the 1950's he and Soodak have been working out an approach to physically (in strong contrast to mathematically) complex systems that they call homeokinetics. The February 1994 issue of Physics Today has a relevant exchange of letters on the subject between the physicist, P. W. Anderson and Iberall. An earlier précis of homeokinetics appeared as the lead article in Science in 1978 (Vol. 201, p. 579). Gene Yates published many pages of homeokinetics work in the American Journal of Physiology (Section R) in 1978. Ibby's physics, to himself, has always been social physics. He wanted to emphasize and use those features of physics that allowed one to go from scale to scale, from atomic particles to the cosmos, from physics to biology to culture and society, in a non-spooky, unified way. He's devoted himself literally to the goal of developing social science as physical science --- not on an analogy to physics, but as a genuine extension. The first full description of the whole system was published in his 1972 book, Toward a General Science of Viable Systems, (McGraw - Hill).

There now is a new book out, Foundations for Social and Biological Evolution, that contains the most recent set of essays representing the development of homeokinetics through the 1980's. This book is self - published by Iberall's Cri-de-Coeur Press, 5070 Avenida del Sol, Laguna Hills, CA 92653. [ISBN 0-9638799-0-1, Library of Congress Catalog Card Number 93-90828.] You can obtain a copy from that address for $14 (U.S.).

The fact that this work is not yet published by an established publisher is quite an interesting story in the history of science. Ambitious work, especially work not in anyone's particular mainstream, is hard to evaluate. A great deal is submitted every year that is merely eccentric at best and is not worth the time of a serious scholar. Ibby has had the attention of serious scholars throughout his career (as one can see from the very partial list of his collaborators) and some of us are betting that continued attention will be more than amply repaid with insight.


ISEP Office

The International Society for Ecological Psychology

Included with Serial Number 21 NEWSLETTER SUPPLEMENT August 1994

Two Ecological Couples: The Gibsons and The Barkers

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When I received an invitation to contribute a chapter on "Ecological Psychology" to the Encyclopedia of Human Behavior my first response was to request further details about the subject to be covered. I consider myself to be an environmental rather than an ecological psychologist, but I am aware that some people use the terms interchangeably. To the media, for example, environment and ecology are often synonyms as in the ecology/environmental movement. On the other hand, I recognize that the technical meanings of the terms are different, and this difference is accentuated by the adjectival status of "ecological" or "environmental" in relation to the noun "psychology." Definitions of "environmental psychology" tend to be unbounded and fuzzy at the edges. Most textbooks describe it as the branch of psychology concerned with the relationship between people and their environments. In contrast, ecological psychology does not lack specificity or clarity. There are at least two very clear, but very different meanings of the term. Each is associated with the work of a particular individual (or couple) and has an over-riding theory or approach, a complex technical vocabulary, and a specific research locus. There is no overlap between research conducted by individuals identified with one or the other branches of ecological psychology. Different researchers identified with different approaches conduct research on different topics and publish papers in different outlets intended for different audiences.

When I contacted the editorial office of the Encyclopedia of Human Behavior for clarification, I learned that they specifically wanted coverage of ecological psychology since they already had under contract other authors to write sections on environmental psychology and environmental cognition. My pointing out that the term had more than one meaning did not trouble the editorial assistant, who was mainly concerned with whether or not I would accept the assignment. Upon reflection, I realized that situation is not uncommon in the social sciences. There is the famous case of Allport (1935) reviewing dozens of definitions of attitude before coming up with a new one. My antipathy to multiple definitions is based more by my role as a teacher than as a researcher. Typically, I avoid presenting multiple definitions in class as they tend to confuse students.

When I accepted the assignment, I did not know how many meanings existed, but I knew there were at least two. That ambiguity became a motivating force in resolving what the Gestalters would have called tensions in an inconsistent figure whose parts are clear, but do not fit together into a cohesive whole. Writing the section provided an opportunity to compare terms such as "ecological psychology," "psychological ecology," "ecological sampling," and "ecological validity,"2 and to lay the groundwork for integration among the different approaches.

Another motive for writing the chapter was personal contact with the principal figures in ecological psychology. I had been a student of Roger Barker's at the University of Kansas and continued corresponding with him until his death in 1991 and have corresponded with his wife and coworker, Louise Shedd Barker, since then. Barker had been an associate of Kurt Lewin at Iowa. I was a colleague and next-door neighbor of the Gibsons at the University of California, Davis, and attended probably the last formal university seminar J. J. Gibson taught, about a year before his death in 1979. My Ph.D. had been in perception, which was Gibson's lifelong concern. However, my interests shifted to naturalistic social behavior, which was R. Barker's major concern. I was also influenced by the action research approach pioneered by Lewin (1946), who is often credited with the first usage of the term "psychological ecology."

Gibson, who had attended meetings of Lewin's "topological psychology" discussion group, was intrigued by Lewin's (1936) attempts to formulate his theory of behavior as locomotion, with fields, valences and vectors. Lewin's theory of virtual locomotion was the basis of the Gibson & Crooks (1938) analysis of the visual guidance of automobile driving. Lewin had coined the term Aufforderungscharacter which became the basis of Gibson's concept of affordance (Gibson, 1979, p. 138). Gibson had also been influenced by the ecological sampling approach of Brunswik (1956).

At the start of his career, Gibson was known as an experimental psychologist who studied human perception, but with a strong secondary interest in social psychology. Barker also began his career in experimental psychology doing his masters thesis under Walter Miles on finger maze learning. His career path changed when Miles received a grant to study aging, which became the subject of Barker's doctoral thesis. In 1931, Barker worked in Calvin Stone's animal laboratory at Stanford, mostly with rats. Barker's interests shifted again when Lewin came to Stanford as a visiting professor in 1932-33. Barker subsequently joined Lewin at the Iowa Child Welfare Station in 1935, thus starting Barker's career in child psychology, which became his lifelong interest. With his colleagues at Iowa, Barker compiled what was believed to be the first "reader" in child psychology (Barker, Kounin & Wright, 1943).

The major research specialties of the two men were not only different, they were differentially valued by their peers. Perception was a hard-nosed experimental field and occupied the attention of the greatest psychologists of the day, while child psychology was a fuzzy, unbounded field of study, often considered to be part of home economics. At the University of Illinois, Barker realized that being a psychologist at a College of Education was "distinctly lower class" (1979, p. 2150). Barker was rescued from this lowly status by his former mentor, Lewis Terman, who offered him an appointment at Stanford's Psychology Department. Unfortunately, there proved little demand for child psychology at Stanford, and Barker was told to look for a job elsewhere. A two-year stint at Clark followed before he applied for the Chairship at the Kansas University Psychology Department, which had recently experienced considerable turmoil and was looking for new people. This was the only academic administrative position held by Barker during his careers. Barker did not particularly enjoy being Chair, and bailed out after three years. In his autobiography, he mentions the apprehensions of his former Stanford colleagues when they heard he was going to become a Chair, since they were acutely aware of his "...deficiencies in keeping appointments, answering correspondence, arranging schedules, making and keeping budgets, lack of gregariousness, and so forth" (1979, p. 2124). Neither Barker or Gibson aspired to climb the academic hierarchy, deriving their primary satisfaction from research and writing.

Books by the Gibsons are heavily theoretical. Barker saw himself as atheoretical, believing that more data were needed in order to develop a theory in a new research area.. His students disagree in the belief that ecological psychology fits most dictionary definitions of theory, e.g., an idea or plan of the way to do something, a systematic statement of principles, or a formulation of apparent relationships. Barker was reluctant to make causal statements. He relied exclusively upon non-reactive observation which yields relationships based on correlation rather than causality. I would argue that it is possible to develop a theory based upon correlational statements so long as there is a consistent method and a technical vocabulary, and possibilities for making clear predictions capable of proof and disproof. Bechtel (1977) and Wicker (1987) transformed Barker's lists of research questions into testable propositions in such areas as staffing theory, which describes the potential effects of having too many, too few, or the optimal number of people in a behavior setting. Similar types of predictions can be made for other indicators based on observational data, such as permeability of settings, or the degree to which a behavior setting is open or closed to non-occupants.

The relationship between the two ecological psychologies is very different from that between the various schools of psychoanalysis, behaviorism, or humanistic psychology, where there have been frequent encounters and verbal sparring among representatives from different camps. Nothing like that is found in ecological psychology. The little verbal sparring that has occurred has been directed outward against those who study behavior under artificial rather than natural conditions. Regarding the two pioneer figures in ecological psychology, Schoggen (1989) states, "....we know of no evidence that Barker, in writing about ecological psychology, was aware of Gibson's work, and Gibson's 1979 book makes only a passing reference to Barker" (p. 304). The two men worked independently on

different issues and wrote for different audiences. Interestingly, the same separation also existed between R. Barker and E. J. Gibson, who both shared a common interest in children's behavior. These researchers, who were peers in age, shared a field of study, and placed themselves under a common rubric, paid little or no heed to each other's work. This separation has continued on to the next generation of researchers.

Both branches of ecological psychology developed out of gestalt psychology, which itself rose as a reaction against the reductionism of behavioristic psychology. The train of intellectual development has not stopped, since ecological psychology is seen as a progenitor of environmental psychology, whose textbooks typically describe R. Barker and J. J. Gibson as pioneer figures in the field. Comparing environmental with ecological psychology reveals their many differences. Environmental psychology is eclectic in method, theory, and subject matter; field and laboratory studies are done on the same issues. There is no over-riding theoretical framework; some researchers follow a behavioristic approach, while others follow humanistic psychology, or attribution theory. Environmental psychology is less holistic than ecological psychology, and typically studies limited classes of phenomena, such as the effects of noise upon academic performance or the influence of crowding upon altruistic behavior. This type of research is more manageable, less time consuming, and easier to summarize and publish than is work in ecological psychology. Most researchers engaged in psychological studies of person - environment relationships identify themselves as environmental rather than ecological psychologists.

Both J. J. Gibson and R. Barker specify a particular situation in their lives that defined their own positions outside the mainstream of psychology. For Gibson it was his work for the US Air Force in the Second World War, where he found the results of laboratory-based studies using stationary observers and stationary stimuli to be of little or no relevance. In flight everything is in motion - - the eyes, the head, the body, the observer's locus, and the stimuli perceived. Gibson recognized that the limited application of laboratory studies was not confined to the unusual situation of flying an airplane, but was characteristic of the real world where moving observers confronted moving stimuli. He became an advocate for the use of solid models in perception studies, as compared with the typically two-dimensional stimuli used in laboratory experiments on form perception. In his air force research, he developed " a nagging suspicion that nobody ever really sees a flat form in life; that is, a picture of a thing. One sees a continuous family of perspective transformations and infinity of forms, that somehow specifies the solid shape of the object" (Reed & Jones, p. 18). Gibson concluded that his task was not simply to develop a new type of psychology appropriate for flight training, but one that would be generally applicable to all human perception. After being discharged from the air force, he returned to teaching at Smith College and wrote The Perception of the Visual World (Gibson, 1950). Barker, too, had been influenced by the Second World War. Due to an osteomylitis infection beginning at age 13, he did not serve in the military, but was able to apply his research skills to the study of rehabilitation services for war casualties. His own disability, which caused some impairment in locomotion and occasional major inflammations, was a major factor behind his lifelong interest in the psychological aspects of rehabilitation for the physically disabled (Barker, Wright, Meyerson, and Gonick, 1953).

The defining event for R. Barker was his work in Lewin's laboratory in Iowa. Although Barker admired Lewin's skills as an experimentalist, and his remarkable ability to bring complex social variables such as social atmosphere or leadership style into the laboratory, Barker was concerned about the effects of the experimental situation itself upon the outcome. He believed that the demands of the laboratory environment could not help but influence the behavior studied; therefore, the only solution was to go outside the laboratory and study social behavior free from all experimental interventions. While working in Lewin's laboratory, Barker had co-authored the classic paper describing how children exposed to frustration regressed in their behavior (Barker, Dembo, & Lewin, 1941). Some years later, Fawl (1963) under Barker's tutelage, studied children's response to frustration under natural conditions and came up with different results. Instead of regressing in their behavior, children subjected to frustration under natural conditions typically withdrew from the situation, a response largely precluded by the behavioral constraints of the laboratory. Barker's disenchantment with laboratory experimentation remained a salient feature of his career. In contrast, Gibson continued to be interested in laboratory studies and encouraged his students to do them, although he advocated the inclusion of proprioception into the research designs. Movement had no special significance

in Barker's approach. From the standpoint of research economics, movement made life difficult for Barker's observers who subsequently opted for place-centered research (remaining in a single location and recording all the behavior occurring there) especially in places under a single authority system such as a school or a hospital rather than person - centered research, which required the tracking of a single subject throughout the day. Gibson's subject matter can be described as the world in motion, while Barker's world centered around behavior settings conspicuous more for their stability than their flux (i.e., people enter and leave while the setting remains intact). Barker also documented how people could change their environments by creating new settings and letting others atrophy.

Both men entered psychology for altruistic reasons, specifically the possibility of "doing good" by focusing the powerful tools of science upon the human affairs (Barker, 1979; Gibson, 1967). They were both political iconoclasts, although clearly on the left side of the political spectrum. Both men spent many of the formative years of their careers as intellectuals during the great depression. Gibson (1967) describes himself as becoming a "left-winger and joining the Labor Movement" (p. 14), attempting to organize college and high school teachers. He was one of the founders of the Society for the Psychological Study of Social Issues, of which Barker was also a long-term member. Gibson was an active supporter of the loyalist side during the Spanish Civil War, and in 1946 was investigated by the FBI for possible "communist affiliations." As a result of these investigations, he lost the security clearance necessary for further air force research. Barker (1979) describes himself when he entered psychology as "a thorough-going, naive idealist, and ardent reformer" (p. 2139).

Toward the end of their respective careers, both Gibson and Barker independently rejected "ecological psychology" in favor of more specific terms. Recognizing the dominance of vision in human perception, Gibson coined the term "ecological optics" to describe the visual information available for perception. Ecological optics rested on several distinctions not common in physical optics, such as that between luminous and nonluminous bodies, light as radiation and as illumination, and radiant and ambient light. Barker, in his last books, tried to disassociate his work from the field of psychology and called the field "eco-behavioral science." Associates of both men have not adopted their recommendations, and continue to use the more generic term "ecological psychology" because of its explicit recognition of the interdependence between organisms and their surroundings. The work of Gibson and Barker has not become the basis of new fields, so much as for alternative approaches in existing fields. Specifically, Gibson's work is an alternative model for perception research, while Barker's approach is considered a sub-field of environmental psychology. The papers of both men have been preserved in archives maintained by their former universities. Gibson's working notes and correspondence are kept in Cornell University's Olin Library, while records of the Midwest Psychological Field Station, including many of Barker's notes, are housed in the Kansas University Spencer Research Library.

Radix, Going to the Root

Reed (1988) described Gibson as offering "a radically new vision of how we see our world" (p. 12). Barker did not provide a new vision so much as heightened awareness of person-environment congruence. In his assertion that better predictions about human behavior could be made from knowledge of place of occurrence than from personality test data, Barker challenged fundamental assumptions about the value of psychological data. Barker's writings open one's eyes to power of settings to influence behavior. Settings such as soda fountains, gas stations, rotary club meetings are virtually excluded from the literature of mainstream social psychology. A search of the index terms in Psychological Abstracts for the past 50 years yielded no reference to gas stations, a ubiquitous and important behavior setting in the western world (Sommer and Wicker, 1991). Gibson also called attention to the mundane, but this was not the power of behavior settings that concerned Barker, but environmental facts, such as what constituted a stone, a stick, or a tree (Gibson, 1979). Gibson and Barker avoided neologism wherever possible, preferring to describe phenomena in ordinary sense language. This was a deliberate choice, in that both men believed that they were describing an objective outside world rather than constructing a new, artificial world, interpretable only by other psychologists. However, the need to construct totally new epistemologies required an occasional new or at least unfamiliar term, as in affordance which Gibson borrowed from Lewin and Barker's use of synomorph, to describe the correspondence between behavior settings and the actions of their occupants.

Gibson criticized the work of his colleagues because it lacked relevance. At one point he defined psychology as "The effort to find answers to the wrong questions; the study of problems chosen to be convenient to study, instead of relevant " (Reed, 1988, p. 6). He later added that "Experimental psychology today suffers from the defect that what is known is mostly irrelevant, and that what is relevant is mostly unknown" (Reed, 1988, p. 6). "Relevance" in this context referred to the pressing issues facing individuals and society. Gibson wanted psychology to be a force for improvement in the human condition.

Barker also believed that most psychologists studied unimportant aspects of behavior. However, Barker's concept of unimportance was based less on the lack of relevance to pressing social issues that so troubled Gibson, than the lack of external validity and generalizability because behavior was studied under artificial experimental conditions. Rather than arguing, as did Gibson, that psychologists study primarily what is convenient, Barker maintained psychologists tend to overlook what is convenient and obvious, in favor of what is theoretically significant and arcane.

Barker became disenchanted with laboratory methods when he attempted to understand behavior outside the laboratory. Faced with the task of describing the lives of children in a small Kansas community, Barker found that the kits of research methods, and transported from his training in the best graduate schools, contained no procedures for investigating the everyday lives of children at breakfast, in music class, or at worship service. Barker (1987) concluded sadly that laboratory methods "brought no help, and some hindrance, and there was much we had to unlearn" (p. 1414). He expressed his disenchantment in terms similar to those used by Gibson, "After almost 100 years, scientific psychology could provide us with no procedures, concepts or technical language for describing its phenomena as they occur intact, outside laboratories, clinics, and testing and interview situations. It was as if botanists had no way of describing plants in the field, except after taking them to the laboratory and cutting them into parts according to the investigator's designs" (Barker, 1987, p. 1415).

Although Barker steadfastly refused to adopt an interventionist strategy in his own research, he acknowledged the potential of these methods for improving social life in communities. The explanation of his seeming reluctance to intervene was his belief that he lacked the time and the skills to do this properly (1979, p. 2157), but he noted with approbation the efforts of his students and colleagues to apply ecological methods in hospitals, schools, housing projects, and other public settings. Among Barker's colleagues and former students, Wicker (1979/1984) developed the clearest strategy for including interventions in an ecological approach to community issues.

Barker and Gibson opposed the dominant paradigm of psychology because it did not deal with those questions that each considered important. Gibson wanted to know how things looked to people; Barker wanted to know how people behaved. In both cases, the question refers to actual events under real world conditions. It is possible to ask how each man's work would have changed if they had switched research problems. If Gibson wanted to understand children's social behavior, would he have adopted Barker's use of non-reactive observation? Conversely, if Barker had wanted to understand how things looked, would he have turned to the psychological laboratory as a locus of investigation? I believe that clear answers to these questions can be given and these answers indicate that the differences between the two branches of ecological psychology are more fundamental than the problems chosen.

Gibson developed an interest in social psychology early in his teaching career, after he began teaching the course as a matter of necessity, when the person who had been teaching it at Smith died. Gibson believed that social psychology offered a way of studying people in their economic, political, and social contexts. He undertook studies comparing individual versus group judgments and saw applications of this research in understanding jury trials, regional planning, and individual paired with group medical practice. Gibson undertook studies of ethnic stereotypes and wrote a paper "The Aryan myth" which argued that social psychological research could serve as a corrective to distortion and irrationality (Gibson, 1939). Gibson abandoned this work because he felt that the field of social psychology, as it was being practiced, was not addressing the important social issues of the day. However, he retained an abiding interest in social issues throughout his life. It is, therefore, comparatively easy to imagine Gibson returning to research on social behavior. If this had occurred, I see little or no

likelihood that he would have adopted the non-reactive methods used by Barker. Had Gibson specifically concentrated on children's behavior, there seems every likelihood that he would have employed the approach used so productively by Eleanor J. Gibson, who was able to conduct experiments with strong ecological validity. I also see him adopting the action research model pioneered by Lewin (1946), whose idea on locomotion had influenced Gibson's ideas about proprioception. Action research has the potential of combining social intervention with the quest for valid scientific knowledge.

There is no published record of Barker conducting research in perception. This is not mentioned as a specific concern in his autobiography (Barker, 1979). However, as a well trained psychologist in the 1920's, he could not have escaped from exposure to this important research area. Subsequently, he was an associate of Lewin who applied the Gestalt theory of perception to social behavior. If Barker had switched to the study of perception as a mature scholar, I doubt that he would have used the laboratory methods endorsed by Gibson. Based on other segments of his career and his writings on methodology, I see Barker focusing his efforts on responses to the actual stimuli in daily life, answering the questions of what people see around them and how they see it. Gibson paid much attention to the frequency with which various stimuli appear in people's ordinary lives, e.g., he discusses how a stone or tree is perceived as an observer walks up to it, but not how often a stone or tree is consciously noted as a relevant element in the observer's world. To study an internal process such as perception, Barker would probably have opted for structured interviews or running introspective narratives of how people viewed the world as they undertook their daily activities. Barker credits his time at the nursery school at the Iowa Child Welfare Station with making him aware of the value of detailed, narrative records of behavior, using ordinary description language in situations where the observer intrudes little or not at all (Barker, 1979).

I will illustrate Barker's approach to perception using the example of misperception, which was a popular research topic in the early 1950's. At that time, investigators presented subjects with vague and ambiguous stimuli to see if these would be interpreted according to the subject's needs. The work was almost entirely laboratory-based and the range of stimuli was extremely narrow. The results were inconsistent and difficult to interpret. I believe that Barker would have accepted misperception as a valid research topic, but rejected the way it was being studied (i.e., testing the responses of college sophomores to unusual stimuli selected by the experimenter and presented under artificial conditions). Instead, Barker would have asked how often people in their daily lives experience misperception, what types of stimuli are most likely to be misperceived, under what circumstances is misperception most likely to occur, and what are its social and personal consequences? These issues were rarely addressed in the hundreds of studies that took place under the need-perception rubric.

If my analysis is correct, the difference between the approaches of Gibson and Barker goes far deeper than a choice of different topics. I maintain that neither man would have adopted the other's methods had their research topics been switched. In terms of core epistemology, Gibson was an experimentalist while Barker was a naturalist. I regard these as fundamentally different strategies for obtaining information about behavior, although they may reflect similar views of the nature of reality.

The Spousal Connection

Both J. J. Gibson and R. Barker acknowledged the contributions of their spouses to their work. Louise Shedd Barker was trained in biology and her work at the Hopkins Marine Station of Stanford University influenced her husband's approach, particularly in the area of methodology. The concept of the field station and use of non-reactive observation are more typical of the natural sciences such as botany and geology, than the laboratory-based physical sciences such as chemistry and physics from which experimental psychology originated. She also worked as a high school "home visitor" for truant children. L. Barker was part of the research team in the Midwest Psychological Field Station. She did much of the contact with parents and organizations to obtain permission to make observations . She continued to do these tasks during field work with her husband in the UK. On her own she wrote very little in the field of psychology, although she was active in historical preservation in Kansas.

Eleanor Jack Gibson remains a major figure in developmental psychology, the well published author of widely read books and articles. Her work is described in detail in textbooks, and her biography included among

those of eminent psychologists. Her research accomplishments are recognized by grants and prestigious awards. A former student of J. J. Gibson at Smith, she collaborated with him in the early stages of her career in studies of perception before beginning her pioneering work in perceptual learning. Like her husband, E. J. Gibson viewed perceptual learning in adaptive terms, as extracting increasing amounts of information from the environment. E. J. Gibson writes more clearly and succinctly than her husband, using examples and terms that students and lay people can understand.

It is not possible to assay relative contributions within each spousal pair without recognizing the dominant gender ideologies of the times. In both couples, the male of the household was the major breadwinner during child-rearing years. E. J. Gibson held a succession of non-permanent, low-paid research positions during these times. It was not until late in her career that Cornell University belatedly recognized her international reputation and elevated her from non-tenured ranks to an endowed chair. Following her marriage, L. Barker did not pursue an independent career in her own field (biology) or in psychology, but rather chose to become a valued member of her husband's research team. Commonalities between the approaches

The two approaches to ecological psychology share very little in method or subject matter. Gibson and his associates focused their attention on laboratory studies of perception, while Barker and associates tended to ignore perception and direct their attention to naturalistic social behavior. What the two approaches share is a set of fundamental assumptions taken from the larger field of ecology from which both originally drew their inspiration. Among these assumptions are: (a) the belief that an organism and its environment should be treated holistically as an interdependent unit; (b) a commitment to the philosophical doctrine of realism, in the belief that the environment exists in itself apart from the mind's consciousness of it; and (c) an emphasis on the importance of studying psychological processes under real-world conditions. For Barker, the last assumption meant leaving behind the laboratory as a locus for investigation, although Gibson did not share this view. It may be relevant to note that the impressive advances in simulation, particularly in virtual reality technology (Tart, 1992), give support to Gibson's decision to continue with laboratory studies. Similar advances have not been made in simulations of social behavior, particularly in mundane, ubiquitous environments which, at least for the present, supports Barker's decision to abandon the laboratory for the study of social behavior in its natural contexts.

Neither Gibson nor Barker provided a comprehensive psychological ecology involving all intra-psychic and social processes. This seems largely a tactical omission, the result of time and resource limitations. Both men believed their approaches extended to all psychological processes, but they did not have the time to devote themselves to issues beyond perception, in the case of J. J. Gibson, and social behavior in the case of R. Barker.

Their associates show signs of extending the range of applications. Wicker (1987) recommends a combination of quantitative and qualitative approaches with the latter being used to study the purposive goal-directed behaviors of individuals in behavior settings. It is easy to imagine this work intersecting with the goal-directed studies of E. J. Gibson (1969) and her students. As future investigators in one or the other camps extend the range of topics studied, the two approaches are likely to merge without, in my opinion, becoming a dominant paradigm. While the general principle of the interdependence of organisms and environments is widely accepted in psychology and the other social and behavioral sciences, most research in these fields does employ an ecological model. This is due largely to the complexity and expense of research designs undertaken from an ecological perspective which require the systematic analysis of organism - environment relationships under natural conditions studied over time. It is much simpler and more economical to employ a stimulus-response model in studying a single variable or conducting a multivariate analysis under controlled conditions at a single point in time. Because this situation is not likely to change in the immediate or distant future, ecological approaches are likely to remain interesting alternative models, dutifully mentioned in textbooks more than they are used as research strategies.

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