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Introducing the Electronic Edition of the Newsletter

In an effort to find ways to get more Society information out faster, we'll circulate early editions of newsletters electronically, like this. This will not automatically replace the printed and mailed version (but it might for volunteers). A substantial number of members either do not use electronic mail or have not reported an address that they use.

For those of you who find this convenient, I'd like answers to 2 questions: 1. Is this sufficient, in lieu of the mailed version? If you feel no need of receiving your news by formally printed, U.S. postal service (plus that of whatever country you are in) mail, please send me a message to that effect. That is tell me explicitly that you will not require the paper version of the newsletter. 2. The capacity for providing a fully formatted electronic version is near. Please tell me if you have used or know that you can use Adobe Acrobat. Also, if you know a better formatting program for this kind of distribution, tell me that too.

MARSEILLE

It is hardly news now that we met for the Eighth International Conference on Perception and Action last July (9 - 14 July, 1995). Ben Bardy, his colleagues [e.g. Reinoud Bootsma, Yves Guiard, Michel Laurent], the graduate students from the Faculty of Sports Sciences at the University of the Mediterranean, and numerous key collaborators, combined to put on a delicious conference that ran very, very smoothly.

Board Election

An ecological definition for Psychology, or, If Psychology is the Study of Behavior, what is Behavior?

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"A perfect definition is therefore an ideal which can be approached, but never reached. In spite of this, the value to human thought and knowledge of clearly defined concepts is immeasurable."
Hermann Ebbinghaus - 1908

Defining a field of study is a political act, of course. For example, Boring, the "last structuralist", continued to define psychology as the study of human nature (Boring, Langfeld & Weld, 1948) years after most of his contemporaries had accepted the definition ("study of behavior") promulgated by the school which toppled structuralism. Likewise, the recently renewed acceptance of the term "mind" into the definition of psychology- only a few Introductory texts in the 1990s seem to consider "study of behavior" to stand alone- clearly reflects the dissatisfaction with strict behaviorist tradition associated with the rise of cognitive psychology. Since ecological psychology is a different school of thought, it may be interesting to speculate what ecological definition of psychology might be generated.

Devising such a definition might also provide a public service even to psychologists uninterested in the ecological approach, because the reigning definition is inadequate. While it is easy to see the difficulties that attend defining "mind", I will argue that the term "behavior" is equally vague. A more specific, and superior strategy, drawing from ecological concerns, would be to define psychology as the "science of reaction to information."

Some thinkers (such as Zuriff, 1985) have suggested that defining a field is unimportant, even premature. It might be objected, for example, that more established sciences like biology also have definitions with leaky borders; Biology is the "Science of Life" but the nature of "life" is an open and controversial question, while the familiar rosters of characteristics of life apply weakly to gray area entities like viruses. Many scholars ascribe to what we could call a "fuzzy logic" attitude toward definition. In the past, I referred to an attitude that psychology is what psychologists do, believing that statement to serve as a sort of "straw man" maxim for the fuzzy definition

viewpoint. To my surprise, there is at least one introductory textbook that says exactly this as justification for leaving psychology undefined (Heckel & Peacock, 1966). Furthermore, there is a sentiment that definitions are not to be used as territorial markers.

There is much more to say about the pros and cons of definition; without addressing all of it, I will start from the presumption that there is always a benefit to more precise definition of the vague, if only for scholarly purposes or, for example, to relieve the abundant confusion about the nature of psychology on the part of undergraduates and the public. It is true that a definition should not limit a field, but it could be quite useful in describing the field. If psychologists should be free to do what they do (which they should) it is still profitable, I contend, to try to describe what they do - a definition is not for delineating what the boundaries of a field should be, but what they are. In other words, an implicit definition exists and we would profit from explicating it. I also believe the examples from other fields to be misleading. The category of "life" has leaky boundaries because it is a scientific category and perhaps not a natural category (or a definite natural category, anyway). But biologists (at their more philosophical) attend to the nature of life even if they haven't reached consensus, and they agree about which cases are borderline. Psychology's defining term, "behavior", is just as porous, but we don't realize this and we don't work on developing its boundaries or agree on marginal cases.

My contention is that "behavior" is a vague term. Some authors of introductory texts seem to be aware of this and some do not. Many representative textbooks (e.g., Weiten, 1989; Feldman, 1990; Gleitman, 1986) include "behavior" in the definition of psychology but fail to define behavior in the text or glossary. Others (e.g., Lefton, 1991; Gray, 1991; Atkinson, Atkinson, Smith & Hilgard, 1987) define behavior but define it very broadly, either as everything an animal does, or everything an animal does which is observable. In case Introductory texts are considered too rudimentary for authority, my informal survey of Experimental textbooks revealed only one of 10 that defined "behavior" in the glossary (as "a pattern of responses usually organized to accomplish some goal", McGuigan, 1993, p. 341).

So texts which define behavior seem to link it with activity; those that don't perhaps presume this connection. Yet numerous animal activities can be identified and observed (with suitable efforts) at any given time, including digestion, cell division, reminiscence, and vision; typically the latter pair but not the first pair would be deemed psychological. "A burnt child shuns fire" but also sustains tissue damage. Both events would have to be classed as "observable activity" (in one case activity of cells, at least, and in the other activity of limbs, at least) and they are not necessarily distinguished by permanence. Yet the avoidance, but not the tissue damage, strike us as "behavior". A person may fall if he is surrounded by artificial optic flow which specifies a moving room (e.g., Stoffregen, 1985), but he will also fall if he is leaning against a support which is suddenly removed. If the unwritten distinction between these two categories of reaction could be articulated, we might have a more explicit criterion for psychology. Boring, et al. (1948) give the example of two activities involving the stomach: digestion (not psychological) and hunger (psychological); the problem was also recognized by Hilgard (1953), and others.

Knight Dunlap (1927) was responsible for one early attempt to be more specific, defining behavior as response to stimulation. Response or reaction is one of the classic criteria of living things (e.g., Keeton, 1980) and it is the criterion of principal interest to psychology. This definition thus has the advantage of expressing the linking to biology. But considering the burn example, "stimulus" is not sufficiently exclusive- both tissue damage and avoidance are responses to the same stimulation. Furthermore, the term "stimulus" may be no clearer than "behavior"; there is also a lack of consistency in how different authors use the term "stimulus", such that Gibson (1960/1983, p. 333) called it "the weak link in the chain of reasoning."

Other tactics for improving the definition have centered on qualifying the term "response." We might insist that the response also involve meaning, or the brain, or mind, or the whole animal, or at least a body part other than the one stimulated. None of these qualifiers are unproblematic. Looking for meaning as the key to behavior (e.g., Hilgard, 1957) begs the question, as does requiring an involvement of the mind with the added philosophical disadvantages. Insisting on an involvement of the brain would exclude reflexes, and would be especially inimical to ecological psychology, considering our interest in theories of action that deemphasize centralization (e.g., the Bernstein approach; Turvey, Fitch & Tuller, 1982).

On the other hand, characterizing behavior as a response involving the entire body might seem more ecological for similar reasons but it would exclude sensory adaptation, for example. Insisting on response of an unstimulated body part (e.g., Dunlap, 1927) seems to depend too heavily on semantics.
Seeing how none of the tactics for qualifying "response" are adequate for defining "behavior", progress may come from refining "stimulation." The hallmark of the ecological approach is the emphasis on information rather than stimulation, and I suggest that "reaction to information" might pin down "behavior" more fully than earlier approaches.

If we relied on the various ways the information-processing school of thought defines "information", this purported ecological definition may merely replace one vague term with two. Actually, for proponents of the "fuzzy definition" approach, referring to a connection between two imprecise terms might still be a real improvement over one, since more connections will be triggered. Of course, for ecological scholars, "information" has a more technical meaning which may go a lot further in clarifying the meaning of "behavior."

Information in the ecological approach implies structure in a medium, structure that specifies the structuring event (e.g., Gibson, 1960/1983; see useful discussion in Kugler & Turvey, 1987, p. 9). Defining psychology as reaction to information effectively discriminates between the class of topics that psychologists study and topics that are usually not considered psychological. For example, the proposed definition distinguishes the two reactions of the child to a burn. Tissue damage is response to extreme heat, while avoidance is response to information about extreme heat (its association with a certain pattern of optical information, e.g., moving, orange-red, etc.) which can take place without irreversible effects of the structure on the skin. Addressing Boring's example, digestion is a response to food while hunger is a response to information about food (low glucose levels, low stomach volume, high stomach motility, etc.)

For ecological psychology, information involves exchanges of energy that are minuscule compared to physical interactions, that do not ordinarily involve exchanges of mass (Kugler & Turvey, 1987). When a support is withdrawn, the mass of the leaning person undergoes an abrupt change in gravitational force but there is no such transition when he experiences optic flow. A similar point is made by Dusenberry (1992) who also notes that informational exchanges, unlike physical exchanges, can be ignored.

So if the ecological concept of information is employed, "reaction to information" would be a solid way to define behavior, and Psychology would be parsimoniously described as the "science of reaction to information." (Information exists even when not picked up (Gibson, 1966/1983), so trimming the definition to "science of information" would not serve). By way of closing, I will tentatively attempt to head off one leak in the proposed definition, although, mindful of Ebbinghaus' statement, I believe that identification of borderline cases is not a lethal exercise for a definition.

Information is a very broad category for ecological theories; for definitional purposes, I would add one qualifier. If information is structure in a medium, then a cellular component building proteins according to DNA, or a computer responding to programmed magnetic states, would seem to qualify. On the molecular scale, DNA transcription may fail the "exchange of mass" test but computer activity probably does not. Both cases are covered if we require the medium of information to be nonexclusive. Without this limitation, there is a danger that nearly every exchange of energy can be described as informational. DNA proteins and computer programs involve structure in which is created and picked up by similar components; they are essentially exclusive. When air is compressed and rarefied, a generic "channel" is exploited and this information can be picked up and responded to by biological structures totally unrelated to those biological, mechanical, or meteorological events that structured the channel. Under this qualification, a computer "reading" text might be said to exploit a generic optical "channel" rather than an exclusive magnetic "channel". This last example (and similar ones) might not fit comfortably in psychology, but to me it at least represents artificial psychology in a way that a computer going about its day-to-day business should not.

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My wife ran across this in an oceanography text she was consulting in prep for her own course. This is "unconscious inference" or "mental computation" gone wild:

High-pitched and more varied sounds are produced by toothed whales. These sounds fall into two categories: clicks and whistles. Both are used for communication, and some clicks are used for echolocation in which the animal's brain involuntarily determines the distance to an object by multiplying the velocity with which a sound signal travels and returns from the object by the time it takes to reach it and dividing this product by 2.... [the equation follows this passage in the text, modified below to accommodate the limitations of this typing scheme]

$$D = (V \times T)/2$$

Thurman, H. V. (1993). Essentials of oceanography (4th ed.). New York: Macmillan. Page 310.

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Theme Music

One of the highlights of the Marseille meeting was the music. Yves Guiard composed ISEP music that was played as a theme song to reconvene each session after a break. Yves thinks this might be one of the first times that a professional society has had a musical logo. He would, however, like some competition. Yves has discussed the idea with Robert Remez and Ed Reed. If anyone wants to get in on the composing, or if you know a willing composer, Yves would love to hear from you. Contact him by email or regular mail in these ways:

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I know this is terribly unreasonable because we all are so short of time, but how about resuming this discussion about creation of ISEP music? We need not hurry up, but the general impression I got from preliminary conversations with you, Ed Reed, and Robert Remez in Marseilles was that this sort of idea might be worth pursuing, capitalizing on spare time (if there is any!).
What I could do, if you don't object, is to trigger an email discussion on the topic with the above mentioned people to start with, and see what happens.

The Senses as Fiction

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I am writing this essay to ask a strange question. Why do we believe in the existence of perceptual systems? I have been wondering about this for some time. I do not question Gibson's concept of perceptual systems. Rather, I am questioning the 's' at the end: On what basis do we believe in "the five senses"? In this essay I question whether there is, in fact, a good justification for this hoary assumption.

The Assumption of Separate Senses

All theories of perception known to me embody an implicit assumption that perception is achieved through several different sensory modalities. This assumption of separate senses serves as justification for basic aspects of essentially all theory and research on perception and related fields (e.g., epistemology, cognition, sensory neurophysiology). Boring (1950, p. 182) refers to the division of perception by senses as one of psychology's "primary principles of classification", attributing it to Aristotle, Locke, and Berkeley. Berkeley took the existence of separate senses as a basic premise of his philosophy:

Sitting in my study I hear a coach drive along the street; I look through the casement and see it; I walk out and enter it. Thus, common speech would incline one to think I heard, saw, and touched the same thing, to wit, the coach. It is nevertheless certain the ideas intromitted by each sense are widely different and distinct from each other... (Berkeley, 1709, in Boring, 1950, p. 185).

Berkeley does not explain why he considers hearing, sight, and touch to be "widely different and distinct from each other". What is the basis of this assumption? One obvious source is the existence of anatomically distinct receptor systems for vision, hearing, taste, smell, and touch. A second justification is the fact that perceptual stimulation involves several different

forms of energy: “Seeing involves the activity of extracting information from light radiation; hearing occurs when a creature gains information from pressure waves of certain sorts; smell and taste involve the extraction of information from chemical features of the environment... touch incorporates the capacity to obtain information about things via mechanical contact of some sort”, (Heil, 1983, p. 8).

The apparent use of distinct anatomical structures to pick up information from qualitatively different forms of stimulus energy provides a ground for the division of perception by sense modalities. Is this categorization appropriate? Are the differences between sensory anatomies, or between light, sound, and so on, fundamental, such that they should form the most basic categorization of perception? Do we truly have “air-tight sensory modalities”, (Ryan, 1940, p. 660)? It may seem incoherent to suggest that perception could be categorized in any other way, but such a thing can be done. For example, rather than studying vision, touch, hearing, taste, and smell, we might study the perception of the layout, of self-motion, of objects, and so on. If there are credible alternatives to the assumption of separate senses, then some rationale must be offered to motivate its retention.

The assumption of separate and independent senses is so basic that it is implicit even at the introductory level. Undergraduate textbooks on perception are organized in terms of individual senses, with chapters on vision, hearing, touch, and so on (e.g., Matlin & Foley, 1992; cf. Bruce & Green, 1985; for a rare partial exception see Dember & Warm, 1979). No justification for this parsing is thought to be necessary. The assumption of separate senses is implicit in theory and research in areas of cognition such as learning, attention, memory, and imagery, each of which is commonly considered in the context of individual sensory modalities (e.g., “visual cognition”, Pinker, 1985; “visual imagery”, Neisser, 1976; “auditory information processing”, Hawkins & Presson, 1986; “auditory imagery”, Reisberg, 1992). It is reflected in the existence of sense-specific journals (Vision Research, The Journal of Auditory Research, The Journal of Vestibular Research) and in theoretical treatises attempting to account for perception within a single modality (e.g., Cutting, 1986; J. J. Gibson, 1950, 1979/1986; Handel, 1989; Katz, 1925/1989; Marr, 1982; Wechsler, 1990). In this atmosphere research on the interaction of senses relies almost entirely on theoretical constructs developed in the study of individual senses (e.g., Dichgans & Brandt, 1978; Klatzky, Lederman, & Matula, 1993; Lackner & DiZio, 1988; Lewkowicz & Lickliter, 1994; Loomis, 1993; Marks, 1987; Saldana & Rosenblum, 1993; Walk & Pick, 1981; Welch & Warren, 1986). The assumption of separate senses may be the most basic premise of psychology, and among the most basic of epistemology. It is accepted pervasively and without explicit justification. I have been unable to locate an explicit justification of the assumption of separate senses in the philosophical, perceptual, comparative, or neurophysiological literatures (e.g., Boring, 1942; J. J. Gibson, 1966; Heil, 1983; Milne & Milne, 1962; Pieron, 1952; Sherrington, 1920; Stein & Meredith, 1993; Welch & Warren, 1986; cf. Uttal, 1981). Classen (1993) has suggested that our concept of the senses is, to some degree, a social construction.

Anatomy and energy

Whether we construe them as sensory modalities or as perceptual systems, why do we classify the senses as we do? As stated above, one motivation is differences in sensory anatomy. Sensory receptors have different anatomy, and

different anatomical locations (e.g., eyes, ears, tongue, nasal cavity, skin, muscles, joints). However, an argument based on anatomy depends upon the prior acceptance of the assumption of separate senses, that the senses exist and operate either exclusively or primarily as independent units. The anatomical differences do not, by themselves require this assumption. As an analogy consider bi-manual coordination (e.g., Beek, 1989). Like the eyes and ears, the hands have different anatomical locations, but they work together inseparably in many activities, such that the activity is a product of their interaction and is not reducible to the activity of individual hands. Examples are playing the violin, threading a needle, archery, and clapping. A similar argument might be made about the fingers within a hand (holding a needle, grasping a baseball), about bi-pedal coordination (walking), and about the integrated action of all parts of the body (acting, dancing, or swimming).

A perceptual example is binaural sound localization. Sound typically arrives at one ear first, then at the other. The delay between arrival at the two ears provides information about the location of the sound source relative to the head (Gibson, 1966). The ears function as an indivisible unit in picking up this informative relation (this may be why we have two ears, instead of one). Another example is binocular stereopsis. The optic array differs at any two points of observation, such that relations between samples of the optic array taken simultaneously at two locations provide information about the visible layout. This information is an irreducible relation between the two array samples. Accordingly, the eyes function as an indivisible unit in binocular vision.

In general, it is possible for anatomically, physiologically, and neurally distinct structures to work together to achieve irreducible, coordinated end products. By irreducible I mean that the activity in question ceases to exist, or is qualitatively altered, if not performed through the integrated action of anatomically distinct units. This is distinct from ways in which scientists might analyze action. It is possible to study one hand of a violinist, but there are no one-handed violinists. We cannot learn about binocular stereopsis by studying the right eye, nor about bi-aural sound localization by studying the left ear. Thus, the anatomical differences between ears, eye, and so on, are not a sufficient reason for parsing perception into vision, hearing, taste, touch, and smell. In any event a concentration on receptor anatomy would be odd within an ecological perspective, where the organization of perception is presumed to have a functional relation to action: “Vision” is not an action category or unit.

Another possible basis for the differentiation of distinct sensory systems might be forms of stimulus energy that are peculiar to each. However, this is also problematic. One cannot generate an a priori list of “potential stimulus energies” without prior knowledge of sensory anatomy. For example, defining vision as “perception on the basis of light” requires a definition of “light”. Only a narrow band of the electromagnetic spectrum is associated with vision (and, thus, called “light”). The electromagnetic spectrum, in and of itself, is a continuum that has no inherent partitions. Thus, defining vision in terms of electromagnetic energy requires an appeal to visible light, at which point the definition becomes circular. Note that some species are sensitive to other parts of the spectrum (e.g., some insects and birds), as is also true in audition (e.g., dogs). In addition, other senses respond to some parts of the electromagnetic spectrum. For example, infrared radiation is perceived as warmth (and can be used by certain species of snakes to “see”; this is dependent

on receptors that are different and separate from the eyes). Similarly, certain forms of vibration are involved in touch, while others, differing only in frequency, are involved in hearing (the concussion of fireworks can be felt as well as heard). Again, a concentration on forms of energy would be odd within an ecological perspective, since ecologists stress that perception is dependent on information, and that energy, per se, is irrelevant.

The above examples suggest that neither receptor anatomy nor stimulus energy provide a justification for the classification of perception into separate senses. I am not aware of any other basis for accepting the assumption of separate senses. I see this as a considerable problem for perceptual theory in general, and for ecological theory in particular. Would anyone care to take a stab at this?

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1There are, of course, studies of these phenomena, but they are almost always undertaken within a single sensory modality (for example, the literature on perception of “biological motion” consists exclusively of studies of vision; e.g., Blake, 1993; Johansson, 1973).