Specificity and Information
Some Conceptual Issues for Ecological Psychologists

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Preliminaries

What is at issue in the debate concerning "multiple sources of information?" I believe the conceptual issue underlying this debate is the problem of how to understand the radically new concept of specificity proposed by Gibson in his later writings. Pittenger thinks that environmental events are "multiply specified" and Stoffregen says that they are not. Stoffregen begins his contribution with a useful compilation of questions: 1. Are the supposed several sources of information for a single event equivalent? 2. Can one source of information substitute for another? 3. Are the sources of information for a single event independent? 4. Are the sources redundant?

Each and every one of these questions – as well as the larger question at issue – can be answered only when we have a firm grip on the concept of ecological specificity. I would add that a number of puzzling empirical phenomena should also be clarified merely through better understanding of the concept of specificity. I have in mind here such a phenomena as so called "inter - modal conflict," certain kinds of illusions and the apparent "triggering"of perceptual or action responses by supposedly insufficient information.

Specificity and Information

One of the basic claims of ecological psychology is that some aspects of an ambient information array specify their sources in the environment. This claim has been taken to mean, at the very least, that some feature of the array **F** is nomically dependent on an environmental event **E** (Turvey, et al, 1981). Under evolutionarily typical circumstances, in fact, **F** is uniquely dependent on **E**, as well as nomically related to it. (It should become apparent from what follows that a more perspicuous formalization of specificity would be in terms of two "complex particulars," one in the array **(A)** and one in the environment **(E)**. Each of these particulars possesses a set of properties **(p x A)** and **(p x E)** such that at least one member of the former set is nomically dependent on at least one member of the latter set, and that there is a more or less unique relationship between the two particulars **A** and **E**.)

We know that there exist a vast number of these "pieces" of information, and that what these pieces of information seem to specify varies from case to case. But the best examples we have of information specify features of what might be called the inhabited environment. The optical horizon specifies not only the separation of ground from sky, but also the eyeheight of the observer; optical "tau" specifies the time to contact of an observer with an environmental surface; optical accretion and deletion of texture specifies the covering/uncovering of one surface by another as seen from a point of view. Stoffregen is reluctant to label such "pieces" of information as information. His argument is that these pieces do not, in fact, completely specify either the observer or the environment. This is correct: for example, on its own, optical "tau" does not specify much of anything about how the closing velocity is being accomplished (e.g. walking versus running). However, while Stoffregen's point is correct, I do not see that it makes his argument: for something to be information it must specify an aspect of the environment, but it need not specify every aspect of the environment, or even every aspect of the event in question.

From the outset of his theorizing about ecological information, Gibson wisely emphasized the richness of ecological information – there is frequently more information than can be used in any instance. As careful research on acoustic and haptic specificities, as well as optical ones, begins to add up, we now realize how prescient Gibson was. Evolving perceivers face the problem of adapting perceptual systems to an embarrassment of riches. I suspect this adaptation therefore involved the evolution of selective systems, in order to deal with this plethora of information, and enable the flexible, coordinated pickup of a great many "pieces" of information (Intra- and inter - modally).

Two Levels of Specificity

The present position implies the need to distinguish two levels of specificity: one concerned with the complete specification of an organism as situated in its environment. For the sake of terminological specificity, I will refer to the first level as the level of **structural specificity** and I will refer to the second level as the level of **functional specificity**. Both are forms of ecological specificity.

Gibson's experimental work – and those of his followers, as Pittenger rightly points out – tended to involve the testing of hypotheses concerning structural specificities. Often this involved the isolation of very particular informational structures (e.g., isolating accretion/deletion from parallax) for the sake of experimental rigor. In spite of this, Gibson's theoretical work was always developed with an eye towards functional specificity, so that the experimental discoveries could be understood in a broadly ecological context, for reasons related to both practical application and Gibson's general conceptual approach.

From a biological point of view, the distinction between two levels of specificity makes a great deal of sense. If there exist structural specificities, based on nomic relations between informative arrays and the environment, different organisms in different habitats will obtain experience of these regularities in very different ways. The evolutionary problem thus set for a perceptual system is how to select, out of all presently available specificities, those that are of greatest current relevance to one's overall situation in the environment.

Is this cue theory? I think not, for several reasons. First, the emphasis on nomic relations is unlike the bias for statistical analyses found in most cue theories. Second, cue theory treats cues as modifying one another (becoming integrated, or conflicting, or reinforcing one another); the present account assumes, to the contrary, that the functional situation does no more than select what information is most relevant – neither the situation nor the various pieces of information modify the structural specificities (they cannot, by definition). Third, because it is assumed here that perceptual systems are selective, there will be far more variability than in cue - based models. A given functional situation may be apprehended through detection of a great variety of structural specificities. Hence, the same situation might be apprehended first by one, then by another, different set of information.

Interim Conclusion

The answers to Stoffregen's questions are thus as follows:

1. It is unlikely that two pieces of information are ever identical, if by identical one means that they share uniquely identical structural specificities; however, two pieces of information may be non-identical in this sense, and nevertheless contributes to the same functional specificity.
2. Hence, two pieces of information can be interchangeable, to the extent that they contribute to a given functional specificity.
3. The specificities of different pieces of information can be either independent or interdependent.
4. Pieces of information can be redundant for functional specificity, without being redundant (or identical) for structural specificity.

It will be useful to test this way of thinking against certain standard problems in the literature on multiple specificity. For instance, from the present point of view, Stoffregen's concerns about the lack of complete information in, e.g, Warren's studies, should be reinterpreted as a caution against extrapolating from data about how isolated structurally specific information is used to a complete analysis of a functionally specific situation – a caution I am certain all parties in this debate will endorse. Intermodal conflict, on this view, becomes a theory - laden concept which is unnecessary. For intermodal conflict to occur, one must assume that there exist cues (or structurally specific pieces of information) which **necessarily** cause the perception of a unique organism - environment situation (functionally specific). The present account denies this assumption. Like Stoffregen, I would interpret alleged intermodal conflict situations as "in the mind of the beholder" who assumes that one perceptual system "has to" register a different functional fact.

The present account also helps to underscore Pittenger's account of the successes of experimentation in ecological psychology. It is possible to give law - based accounts of structural specificity and its pickup, at least by clever use of laboratory displays and control of subjects' intentions and tasks. It is not possible, even in principle, to simulate an organism - environment situation "In vivo" as versus "in vitro." Studies of perception and action pose many challenges.

Appendix

On The Analogy Between Immune and Perceptual Function

The argument made in the above concerning two levels of specificity may sound strange to ecological psychologists. Yet it has strong roots in the biology of selectionist systems. In particular, there is a direct analogy between the role played by biochemical specificity in immunological function and the role played by ecological specificity in perceptual functioning.

The immune system "recognizes" antigens in a way referred to as "selectionist" in contrast with "instructionist". It does not, in any sense, build up a template or representation of antigens either on the basis of individual of phylogenetic experience. Instead, what has evolved is a special genetic "shuffling" apparatus that means that each individual's immune system is equipped with the capacity to recognize an enormous number of invaders, including non - biological invaders which are totally novel, and to which no ancestor was exposed. Hence, it is evident that neither the environment or the genes "instruct" the system in how to accomplish its function.

Each antigen of interest (e.g., a virus) is itself composed of a number of biochemically distinct regions, which are called **antigenic determinants** if they stimulate any immune response. the presence of antigen in the body selects a whole host of different antibody cells by virtue of these determinants. The specificity of the antibody cells is not to the antigen as a whole, but rather to particular determinants. In recent theoretical immunology the chemical structure of the antigenic determinant is called the **epitope** and this is said to be matched by a **paratope** on the antibody cells which is specific to that chemical structure. Each item of antigen thus has a unique constellation of epitopes (this is especially true of biotic invaders) so that each exposure to an antigen of that kind stimulates some of the same specific matches of paratope to epitope, but also some different matches. After the initial reaction to an antigen however, the vertebrate immune system is so organized that those cells whose paratopes were stimulated undergo a process of relatively rapid clonal reproduction, so that when another instance of that antigen is detected it is more likely to meet cells with those paratopes than it was before.

This whole process was dubbed "clonal selection" by Macfarlane Burnet in the late 1950's. The idea is two fold: first at the level of antigenic determinants and of corresponding antibody paratopes, there are chemical affinities which produce highly specific matches; second at the level of immune response (i.e., of antigen - antibody interaction) there are ensembles of those lower level specificities which allow functional recognition and response to occur, despite the significant local variations from time to time and context to context.

The chemical relationship at the level of epitope and paratope is a lawful one, based on rates of reactions. It is called *affinity* and is measured in terms of kinematics as follows:

(Where Ag= Antigen; Ab = uncomplexed antibody; Ag-Ab = Bound complex; k1,k2 = rate of forward and reverse reaction) Where $k < 10{,}000$ liters / mole one usually considers this relation non - specific. Thus, these structural specifications of paratope to epitope can and should be measured in isolation, as separate lawful relationships between a bodily structure and an invader. (I am ignoring idiotypic networks for the sake of simplicity here)

However, specificity at the level of antibody - antigen relationships — what I am tempted to dub functional specificity — is not predictable from affinities. For one thing, the rates of reaction of any two items will be affected by third and fourth items, so that binding as measured in isolation cannot predict in vivo performance. For another thing, the history of an individual's immune system will bias it in favor of recognizing previously detected antigens and determinants. Thus whereas paratope-epitope specificity is measured in terms of affinity, antigen - antibody specificity is measured in terms of *avidity*, the half saturation of antibody.

Note how the history of a particular immune system will yield changes in avidities without changing affinities at all. Thus, if you have been exposed to antigen A#1 which happens to share a high proportion of antigenic determinants with A#2, it is quite possible that on your first exposure to the latter, your immune system will "cross-react" and respond as if to A#1. This sort of process is actually exploited these days in the preparation of vaccines. Often a virulent strain is bred out so that it becomes attenuated (it loses those determinants related to the actual production of disease). These attenuated viruses can still be used in (some) vaccines, because sufficient exposure to them will produce immune response to their virulent cousins. With genetic engineering technology one can directly delete some of the viral components that are disease-producing, thus producing a kind of artificially attenuated vaccine. (I hope the analogous implication for certain sorts of illusions is not lost on readers)

It is thus because the pieces of the immune system are uniquely specific to pieces of antigens that the system can function as a while to recognize and respond to antigens. The piece-meal specifications are nomic, whereas the functional specificities are historically developed coalitions of these nomic processes, and can and do differ from time to time, context to context, individual to individual, without undermining the lawful relations upon which the system is built.

One should not stretch this immune - perception analogy too far. The immune system is immensely complicated, with an enormous diversity of cells, and a cluster of relatively non - specific functional components, none of which was referred to herein. The key point I wish to press is simply this analogy between two levels of specificity, structural and functional, with a selectionist system accounting for the linking of the two levels. If I had more space I would try to spell out some of the plausible neural substrates of structural and functional specificity. What Edelman calls a **global mapping** would seem to be the smallest candidate for a perceptual system paratope. Such a mapping integrates the motor components of information pickup with the diverse central mappings of peripheral receptor sheets; hence, in principle at least, some such mappings could be capable of detecting significant environmental invariances. A given event, however, would clearly implicate a cluster of such global mappings and these would need to be coordinated via the process of **reentrance** as Edelman suggests in order for a functionally specific perceptual act to emerge. From this point of view, some of the models developed by Finkel and Edelman (of *The Remembered Present*) in which several different "submodalities" of vision (e.g., motion, occlusion, brightness) are reentrantly linked and then shown to be capable of at least a limited kind of object recognition take on considerable interest.

Reference

Turvey, M. T., Shaw, R. E., Reed, E. S. & Mace, W. M. (1981). Ecological laws of perceiving and acting: In reply to Fodor and Pylyshyn (1981). *Cognition, 9*, 237 - 304.