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Finding Meaning in Psychology[1]

Roger W. Schvaneveldt
Arizona State University East

In this chapter, I explore several themes in connection with a selective review of some research problems I have pursued over the years. The study of meaning is a major theme along with issues relating to the goals of scientific psychology and the demands of research on applied problems. Finally, I ponder the role of psychology in the broader scientific endeavor, and I propose a candidate for the psychological level of analysis.

Meaning

One of my enduring interests revolves around the study of meaning. Our everyday perception of the world appears direct and meaningful. Our ordinary encounters with language occur largely in dealing with the meanings expressed by language. Although meaning flows naturally from living, finding a rigorous scientific account of this natural ability is a difficult undertaking. My own pursuit of meaning in psychology has followed the path taken by experimental psychology in the last half century. That path curved away from the strict behaviorism that dominated the first half of the 20th Century toward a new found interest in mental processes and issues in cognitive psychology. Increased concern with applying scientific psychology has accompanied the empirical and theoretical developments. We have seen great strides in applying research in psychology to many practical problems, and the three honorees in this festschrift (Lyle Bourne, Jr., Walter Kintsch, and Tom Landauer) have made many valuable contributions to increasing our understanding of concepts, language comprehension, and complex semantics. The honorees have also been leaders in moving their research into applications. The participants in the festschrift have also contributed greatly to an enhanced understanding of cognition and the application of this knowledge. Now here comes my two-bits worth.

I am grateful to Lyle Bourne, Nancy Cooke, Rebecca Gomez, Alice Healy, Peder Johnson, David Meyer, Deb Roy, and Guy Van Orden for inspiration, stimulating discussions, comments, and criticisms. The responsibility is mine of course.

In the realm of signs and symbols, C. S. Peirce (1839-1914) proposed a triadic theory in the late 19th Century. His theory provides a basic analysis of the meaning of signs, symbols, or representations. In Peirce's words (Buchler, 1940, p. 99), "a sign, or representamen, is something which stands to somebody for something in some respect or capacity." For Peirce, the triadic character of signs was irreducible. For signs to function, all three elements (the sign, the signified, and the interpretant) and the relations among them must be involved. Anything can stand for anything, but there must be an interpretation to bring about the sign function. Peirce's ideas are widely accepted by semiotic scholars (Eco, 1976; Morris, 1971; Ogden & Richards, 1946). In cognitive science, however, thought about mental representation seems to have lost track of the critical role of the interpretation in realizing a representation. Representations are postulated with abandon in theories and models, but interpretation often lies more in the perspective of the theorist than in the system being investigated. Representations are used to stand in for meaning, but there is no account of how representations acquire meaning aside from that attributed to them by theorists and modelers (Bickhard, 1998; Schvaneveldt & Van Orden, 2002).

Scientific Psychology

As a consequence of several historical and cultural factors, experimental psychology has placed great value on explanations of phenomena by identifying the efficient causal factors at work. In the pursuit of reductive explanations of perception and cognition, we often discover that the meaning that was so obvious at the outset has disappeared somewhere along the analytic way, and, like Humpty Dumpty, it cannot be recaptured by putting the reductive elements back together again. Nevertheless, reductive analyses are seductively appealing.

The study of perception provides a clear illustration of major advances following from a reductive approach. Modern textbooks on perception provide great detail about the physical basis of perception including the structure of sensory systems and the details of neural processes involved in perception. We do understand a great deal about the neuroscience of perception, but I still have some nagging concerns that as this work progresses, we give less attention to certain critical psychological issues. Students may be learning less about perceptual phenomena (e.g., the constancies, contrast effects, motion, and coordination) as they learn more about brain function.

Am I alone in wondering about what's being left out as more and more effort of psychologists is devoted to neuroscience? Are we going to learn more about psychology this way, or should we just get used to the idea that with a good handle on neuroscience, we don't need psychology? I think we still need psychology, and I will return to this concern in my discussion of a meaningful psychological level of analysis. First, let's look at some attempts to identify mental modules.

Semantic Priming: Searching for a Processing Account

Sternberg (1969) proposed the additive factors method to identify independent stages of information processing. The basic logic was that experimental factors (or variables) that produce additive effects on reaction time could be assumed to affect distinct stages of information processing whereas factors that interact (are non-additive) are presumed to be affecting at least one stage in common. Finding a set of additive factors could be used to support a model of independent stages. With some further interpretation as to the nature of these stages, underlying components of mental activity could be identified.

The application of this method can be illustrated by a collection of word recognition studies (Becker, 1979; Becker & Killion, 1977; Becker, Schvaneveldt, & Gomez, 1973; Meyer & Schvaneveldt, 1971; Meyer, Schvaneveldt, & Ruddy, 1975; Schvaneveldt & Meyer, 1973). A major focus of this work was to develop our understanding of the nature of semantic priming effects, but several of the studies can be interpreted in the additive factors framework. Figure 16.1 shows a summary of these experiments along with a possible interpretation. Recall that a priming experiment is performed by presenting a priming stimulus followed by a target stimulus. The target is either a word or a non-word, and the task is to determine which by pressing one key for a word or another key for a non-word as quickly as possible. Here we are only looking at reaction times for word targets. Three variables are of interest. Targets were presented under different quality conditions (e.g., varying intensity or varying clarity). The High Quality targets are responded to more rapidly than Low Quality ones.

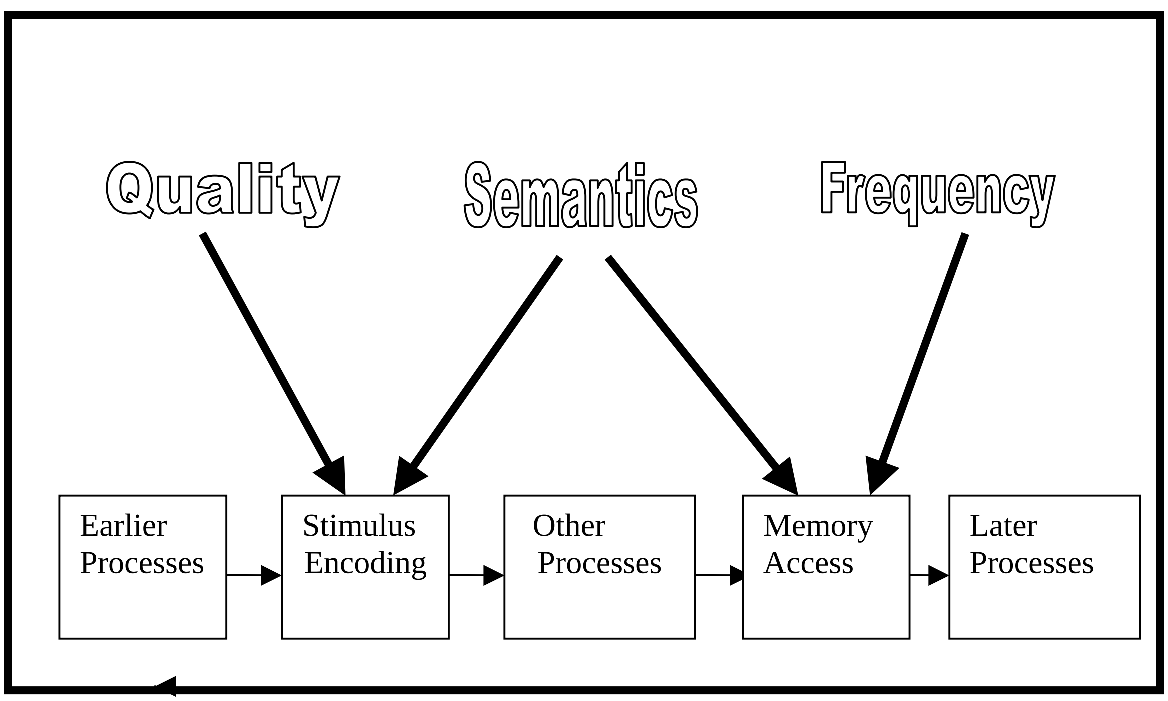

Figure 16.1. Information processing stages and effects of experimental factors.

Semantics were manipulated by having targets either related to the priming stimulus (e.g., prime = “doctor” and target = “nurse”) or unrelated (prime is a word not related to the target). Responses are faster when targets are related to the primes. Finally, the target words were either relatively common words (high frequency) or relatively uncommon words (low frequency). Responses are faster to high-frequency words.

Of the possible interactions among these factors, one (Quality x Frequency) produced additive effects (no interaction). High- and low-frequency words showed the same effect of quality. The other two possible two-way interactions were observed. Semantic effects were larger for words presented with low quality compared to the effect for words presented with high quality. Also there is a larger effect of Semantics for low frequency words compared to the effect for high frequency words. The Quality x Semantics interaction led us to suppose that semantic priming was affecting the encoding of the stimulus, a conclusion that was supported by other studies (Schvaneveldt & McDonald, 1981). We had long supposed that semantic priming would affect memory access, which seemed to be confirmed by the Semantics x Frequency interaction. However, the additivity of quality and frequency implied that these two factors affect different stages. This conclusion was contrary to the predictions of several models of word recognition.

At the time of this work there was some real enthusiasm over the prospects for the additive-factors methodology for providing insights into component mental processes. The enthusiasm waned, however, perhaps because additive factors are hard to come by. There are a number of reports of additive factors (see Sternberg, 1998 for several examples), but in some areas of research such as word recognition, it is more common to find interactions among variables. Also, different experiments sometimes disagree about additive and interactive effects. For example, Norris (1984) observed an interaction between quality and frequency that undermined our independent two-stage account. Theorizing in word recognition moved toward more interactive models with the verification model (Becker, 1979; Becker & Killion, 1977; Becker, et al., 1973), the activation-verification model (Paap, Newsome, McDonald, & Schvaneveldt, 1982) and the dynamic resonance model (Grossberg & Stone, 1986).

In addition to these developments, consider the extensive discussion about the decidability of alternative models of mental processes on the basis of behavioral data. (Anderson, 1978; Townsend, 1972; Utall, 1990). In short, it is possible to construct distinct models that equally fit any given set of data, making it difficult to argue that data uniquely support any particular model. Thus, like the fate of Donders’ subtractive approach to the discovery of mental activities in the 19th Century, Sternberg’s additive factors method with all of its improvements did not provide the discovery tool we had hoped for. The problem is that additivity (and more generally, modularity) may be an inappropriate model for mental processes in general (Thelan & Smith, 1994; van Gelder, 1997; Van Orden, Holden, & Turvey, 2003). Perhaps cognition results from complex dynamics distributed over brain, body, and world operating over multiple time scales of evolution, development, and performance. If so, modularity would hold only under very limited conditions. I return to this point later.

Cognitive Psychology and Applied Research

An often-cited example of the failure of reductionistic psychology to scale up to real world problems comes from the difficulties faced by psychologists brought into the war effort in World War II. Included among these psychologists are some of the giants of the field, e.g., Donald Broadbent, Paul Fitts, and James Gibson. They found that their theories and findings were simply not up to the task of understanding and improving training, performance, or the human factors of technical systems. Having a reductionistic account of behavior did not illuminate the issues raised by actual people performing meaningful work. Some accounts (e.g., Lachman, Butterfield, & Lachman, 1979) attribute this demand for relevance as one of the primary factors leading to the development of cognitive psychology in the mid 20th Century. Keeping questions about the relevance of research in mind does have an important impact on the course of the research. There are always choices to be made about the appropriate level of analysis for research. Trying to address an applied problem can exert powerful constraints on theory and experiment. My own experience with applications led to Pathfinder Networks.

Pathfinder: Applied Semantics

My first acquaintance with applied issues came when I undertook an analysis of the development of knowledge in fighter pilots around 1980 in collaboration with several colleagues (Schvaneveldt, Durso, Goldsmith, Breen, Cooke, Tucker, & DeMaio, 1985). With my interests in semantics in cognition, this seemed like a natural undertaking. Unfortunately, my earlier work in semantic priming proved to be of little use. Although we considered approaching the problem with those methods, the interesting questions in this applied domain seemed to demand methods for characterizing knowledge rather than identifying the details of mental processes.

In a way, we were in the position of the psychologists in WWII who had problems to solve, but the approaches they brought with them were inadequate. I did know a thing or two about Multi-Dimensional Scaling and Cluster Analysis so we devised a rating task that would produce judgments of proximity (or relatedness) for concepts found in the arena of air-to-air combat. We began to see some interesting differences in the scaling solutions coming from the data of student pilots and instructors, but we also began to reflect on the fact that much theory in cognitive psychology was based on semantic networks whereas our scaling solutions had other forms. The obvious next question was to ask how one would construct a network based on proximity data. Our answer was to develop what has come to be known as the Pathfinder scaling method (Schvaneveldt, 1990; Schvaneveldt, Durso, & Dearholt, 1989). The method basically defines a criterion for including links between concepts by preserving those links that maintain the shortest paths (or the links of greatest relatedness).

Examples of Pathfinder networks are shown in Figure 16.2. The top and bottom panels are derived from data obtained from participants with differing

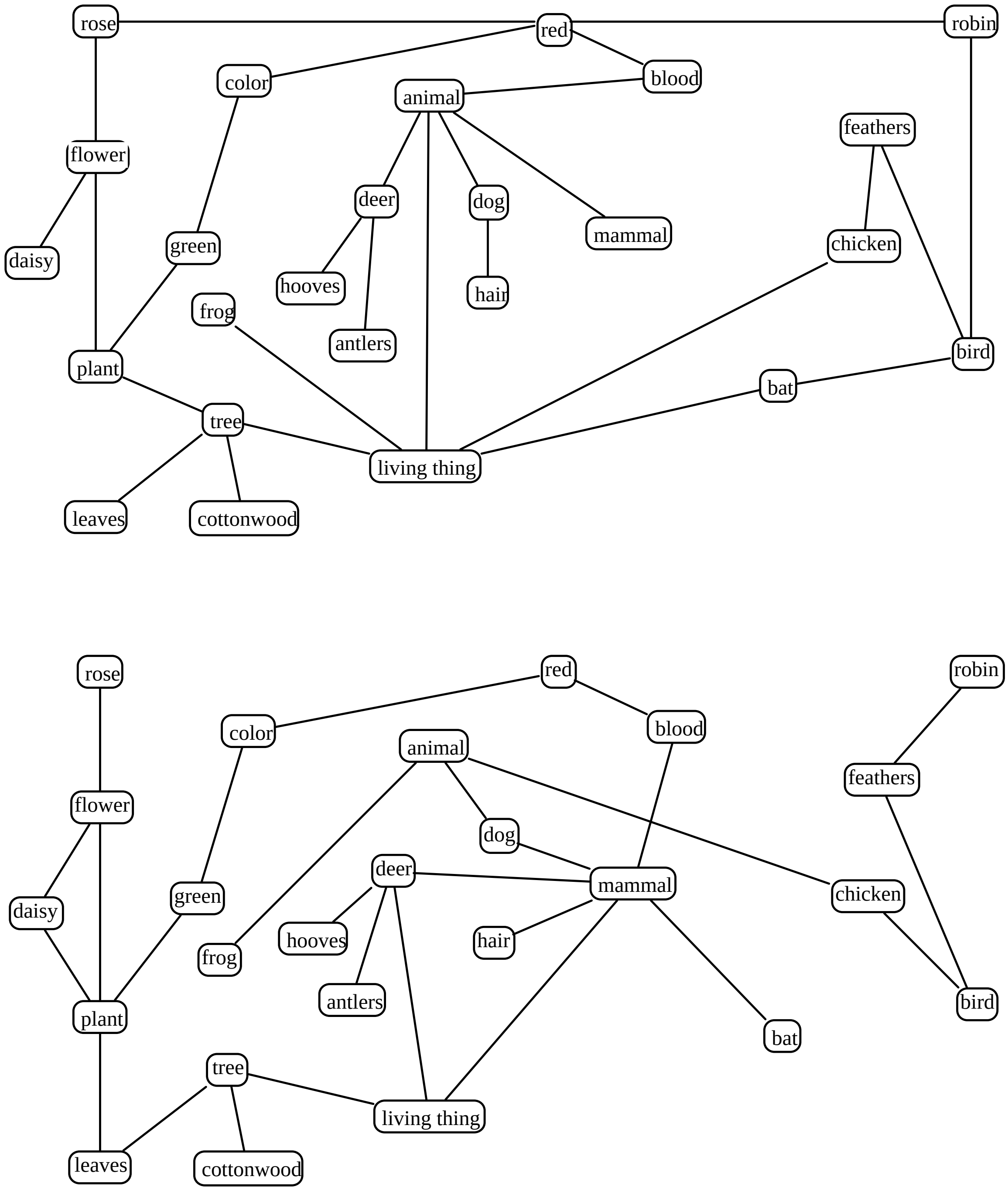

Figure 16.2. Networks from student Ratings (see text).

expertise in biology. Can you tell which is which? The top comes from students in the introductory psychology participant pool, and the bottom comes from graduate students in biology. The differences in the networks appear to reflect differences in these concepts with increases in expertise. Most notable is the way *mammal* is connected in the two networks. In this set of concepts, *mammal* is more central for experienced biologists. Such differences have been the basis of numerous studies in aviation, medicine, computer programming, training, and education.

I wish to emphasize the way in which considerations of real problems led to a push towards increasing the complexity of the problem to be tackled, and by letting the demands of a real problem place constraints on what is meaningful. There was a push away from highly controlled laboratory tasks and variables toward less controlled but more relevant tasks and analyses. In this case, it was away from lexical-decision tasks and semantic priming toward direct judgments of relatedness and psychometric scaling of concepts and their relations. The Pathfinder scaling method has proven extremely fruitful with applications in many areas.

In summary, one of the strengths of much work in cognitive psychology is that it has addressed problems approaching the complexity found in the real world, and, as a result, the progress made in understanding cognition has had clearer implications for applied settings (see Durso et al., 1999). Some good examples are found in recent work of the festschrift honorees. Bourne (2001) and Bourne, Healy, and Beer (2003) bring psychology to bear on understanding terrorism and military conflict. Kintsch (1998) extends his work on language comprehension to cognition more generally. Landauer and Dumais (1997) introduce a new method of deriving complex semantics with a wide array of applications. With so much to applaud, I should just stop here and celebrate, but I'm afraid there is more work to be done. So we continue.

The Psychological Level of Analysis

With some success in application and with the new enthusiasm of investigating the neural substrate of cognition, we might conclude that psychology has finally come of age and we can forge ahead in the established paradigms. Cognitive science can provide a functional account of behavior as well as a map of this account into the brain. This is a pretty picture, but there are some detractors. Do these endeavors exhaust the domain of psychology? Searle (1980, 1992) has claimed that the functional approach to mind misses the target. He is concerned with the general problem of getting semantics into (or out of?) a mechanical syntactic system of which contemporary computer models of mind consist. Dennett (1989) agrees that syntax does not determine semantics. Jerry Fodor is as strong an advocate of representational/ omputational theories of mind as can be found, but, he too, questions the completeness of this approach for leading us to an adequate theory of mind (Fodor, 2000). He is particularly concerned that this approach will never come to grips with the adaptability of the human mind, or abductive reasoning (an idea traceable to C. S. Peirce). These and other concerns raise doubt about the possibility of finding a complete theory by following the most heavily trodden paths (Clark, 1997; Schvaneveldt & Van Orden, 2002). How might we confront such concerns?

Consider some thoughts from William James and from George Miller. James' view is perhaps most clearly expressed in his "Essays in Radical Empiricism" which he developed later in his career as a capstone of his thought (James, 1912). In the essay, "A World of Pure Experience," he wrote:

> To be radical, an empiricism must neither admit into its constructions any element that is not directly experienced, nor exclude from them any element that is directly experienced. For such a philosophy, the relations that connect experiences must themselves be experienced relations, and any kind of relation experienced must be accounted as 'real' as anything else in the system. (p. 42)

James was not proposing a new mental chemistry. Rather, he proposed taking pure experience as the ground of knowing while avoiding metaphysical assumptions that go beyond direct observation (Heft, 2001). By this means, James tried to avoid the dualism that leads to the mind-body problem among other thorny issues.

George Miller has long promoted the view that psychology is the science of mental life. In 1985, he argued that the constitutive problem of psychology is the explanation of conscious experience. He went on to say that this requires understanding affect, volition, and intention in addition to understanding cognition and intelligent systems.

Perhaps immediate experience is what is missing from scientific psychology. The desire to create an objective science has left aside subjectivity. To be sure, cognitive scientists rely in their subjective experience to deliver ideas and conjectures, but we seem to rapidly seek the security and objectivity of operational definitions and models, never again to make contact with the subjective source of these ideas. It seems to me that scientific psychology is the discipline where a science of the subjective should be found.

By following the lead of James and Miller, psychology should at least include a concern with immediate experience. Hasn't the field been there before? Isn't that where Wundt and introspection methods began? Well, yes and no. Yes, the mind can be the focus of psychology without the commitment to basic elements and their combination, and introspection is not the only avenue of investigation. By taking immediate experience as the raison d'etre of psychology, we may have a useful criterion for defining a unique psychological level of analysis. In this paper, I can only begin to point to some of the implications of this way of thinking. A particularly difficult problem concerns the proper place of so called "unconscious mental processes." On James' view, the unconscious may be a problem for physiology rather than psychology. On the side of inclusion, we need to pay more attention to phenomena like those Miller said should be addressed. Volition, consciousness, and intention may not be easy problems, but they may be essential if we are to develop a psychology that is meaningful in the context of the actual lives of real people. There are promising ideas concerning intention and control that deserve attention.

Intention and Control

Darwin, himself, was something of a psychologist. He studied the adaptation of earthworms to experimentally imposed alterations of their environment. In the course of these studies, he discovered what properties of the environment the worms were capable of detecting, and how they adapted to sharpness (for example) by avoiding glass shards and by dealing with the blunt end of pine needles in preference to the sharp ends. Reed (1996) takes such behavior as evidence of intentional action.

Although scientific psychology often approaches the study of intention through the incorporation of goals in models, we do not yet have an adequate account. Intentions are a major part of ordinary experience. Surely we encounter intentions in the laboratory whether we enjoy the contributions of a cooperative participant or despair at the behavior of an uncooperative one. Performing any laboratory task requires having intentions appropriate to engaging the task. If we fail to induce proper intentions, we probably end up discarding the data.

Work in cognitive science has generally dealt with intentions through the implementations of goal structures in cognitive models such as ACT-R (Anderson, 1996) and EPIC (Meyer & Kieras, 1997). This general idea can be traced to the General Problem Solver proposed in the 1950's (Newell, Shaw, & Simon, 1958). These modeling efforts have produced useful results in the forms of models of complex behavior, often within applied settings. There are still some aspects of intention that need further investigation.

In everyday experience, understanding people's intentions enter crucially into our understanding of events as we negotiate a social world. As a clear example, consider the law. The same act can be punished as manslaughter or various degrees of murder depending on the intention of the perpetrator and when, if there was intent, the intent arose.

Despite the value of intentional attributions in every day life, getting a scientific handle on intention has proven difficult. The difficulty seems to lie in our inability to reconcile the common concept of intentions with a causal account of behavior. Philosophers deal with this issue in an area of study known as "action theory" (Juarrero, 1999).

If we can have an account of behavior and the brain, why do we need to take intention into account (cf. Churchland, 1989)? One reason is that it seems likely that useful generalizations about behavior are to be found at the psychological level of analysis. Dennett (1989) points out that treating systems as intentional (a broader notion than intentions to act) allows an observer to interpret behavior in rational terms. The crucial issue for Dennett is whether the intentional stance captures important generalizations. Because intentional thinking pervades common sense, it may be expected to be particularly relevant to contacting people's experience in applied settings. Applications often involve appreciating and affecting people's experience.

From considerations of intentions, we may have a basis for explaining behavior even if we cannot predict it because fulfilling intentions does not entail any specific behavior. I can satisfy an intention to communicate with a friend by calling, writing, emailing, or visiting. If you knew of that intention, you could use it to explain my behavior, but you could not predict it. You might have predicted that I would attempt to fulfill the intention. Just as in the law, intentions are often given more weight than behavior in assessing responsibility.

Juarrero (1999) offers an interesting account of intentional action by viewing intentions in the context of constraints operating in complex systems. On her view, an intention establishes a high-level constraint on behavior that serves to direct action in general toward the fulfillment of the constraint by creating certain attractors in the space of possible behaviors. Many other constraints including physical and social factors acting on multiple time scales also influence the flow of behavior, but the causal processes in such a system include circular causality resulting from "top-down (self-causal) control at work in intentional action" (Juarrero, 1999, p. 176).

The issue of intentional action can be seen in the context of a broader question about the locus of control for human activity including human experience. Where lies such control? Behaviorism places control in the environment with the stimulus. Symbol systems localize control in the environment, homunculi, and the syntactic properties of symbols. Neuroscience tends to favor the brain as the locus of control. A complex systems account distributes control more widely, but control is generally seen as emerging from the interaction of people (considered at multiple levels) and environments (also at multiple levels) where multiple hierarchical constraints interact to bring about human activity (cf. Patee, 1973).

Thus, there are alternatives to taking mechanistic processes as the model for psychological theory. The mechanistic approach comes to us from the tradition of logic and positivistic philosophy. Psychology may be the only empirical science with entrenched bands of organized resistance still holding firmly to this tradition (Toulmin & Leary, 1985). This is not to say that models and formal systems are not valuable in trying to understand complex systems. It is to say, however, that we should not take such models as equivalent to the system being modeled, and we should be open to a broader view of causal processes such as that found in complex self-organizing systems. As a part of a broader view, consider some examples of what an ecological approach brings to the study of psychology.

Information and Aviation

In the tradition of William James, James Gibson has suggested an alternative approach to psychological theory. Consider his work on vision. He begins with a particular visual experience and asks what information in the optical array could support the experience. He emphasizes the importance of a dynamic view of the optical array. That is, information may unfold in time so perceptual processes must be sensitive to the dynamics of the flow to acquire the critical information. Compelling examples of such information come from Gibson's

(1979) analysis of information in optical flow as agents and objects move about in the world.

Gibson's perspective brings interesting changes in the way perception is understood. First, because environments are seen as providing sufficient information to specify perceptions, there is no need for "unconscious inferences" to fill the gap between sensation and perception. Second, rather than starting with the "facts" of anatomy and neurophysiology to arrive at perceptual experience, one starts by identifying environmental information that can support experience and then one looks to see how the nervous system can extract that information. Arguably, the Gibsonian account accords better with immediate experience. We do experience the world. James would likely approve.

Gibson's approach emphasizes characterizing the information in the environment as a first step in developing a psychological analysis. Some of my recent work in aviation focuses on trying to characterize the environment inhabited by pilots in terms of the information they need to perform successfully in various phases of flight (Schvaneveldt, Beringer, & Lamonica, 2001). This analysis has proved to be useful in a variety of ways. In the initial work, we discovered some interesting differences between the information deemed important by pilots with more expertise compared to novice pilots. In most cases, expert pilots gave higher priority to more information elements, which may stem from their ability to handle more information. There were a few cases where novice pilots assigned higher priorities. An example is the priority of information about vertical velocity during takeoff and landing. To the uninitiated, it would seem that knowing about your rate of ascent or descent should be important as you depart or arrive at an airport. However, expert pilots say, "Get the pitch and power right, and the vertical velocity will take care of itself." Apparently, novices want to make sure it is taking care of itself. They are probably unsure about the pitch and power being right. The experts concentrate on the parameters they directly control.

Another use of our work in identifying information priorities comes from its relevance to evaluating aviation information systems. We developed an evaluation method that essentially compares the priority of information elements to their availability in various phases of flight (Schvaneveldt, Beringer, & Leard, 2003). This method helps to identify where critical information is not as accessible as it should be as well as identifying clutter (information that is not needed but is present anyway). Our method should be useful to system designers as well as to certifiers of new information systems.

The Future

I can imagine a future scientific psychology that is more inclusive than what we find in the mainstream today. If we consider the core problem of psychological investigation to be the experience of evolved, animate, adaptive, agents, we can develop a psychology that is continuous with biology even if it is not reducible to biology. From this perspective, immediate experience can be located at the interface between individuals and the world they inhabit (Heft, 2001; Reed, 1996). It is plausible that cognition, including consciousness, is not localized in the head but rather in the interaction of an observer and a world just at it appears to be in our experience (Clark, 1997; Hutchins, 1995; Velmans, 2000). This science of psychology will exploit models and formalisms acknowledged as such. It will also have room for bodies and environments along with the complexities of their interactions. Conscious experience will be a natural part of this scene because it is a natural property of various organisms, and the meaning I have been looking for will be central. I am eager for this future to arrive.

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