

PERSISTENCE AND CHANGE

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3 Johansson's Approach to Visual Event Perception— Gibson's Perspective

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INTRODUCTION

Gunnar Johansson's paper described some of the work which established him as one of our most important perceptual psychologists. Indeed, the idea of a whole conference on event perception might not have arisen without Johansson's contributions. Everyone involved in research on event perception should be grateful for his persistence because it has taken so long for the larger psychological community, particularly in the U.S., to resonate to issues in event perception. A superficial modern history of event perception would note the pioneering work of Mach and Exner, then von Ehrenfels, Wertheimer, Heider, Rubin, Düncker, Michotte, and Johansson's earliest work (Boring, 1942; Johansson, 1978). Out of all this, however, it was not "events" which were given sustained attention, but "motion." Heider (Heider, 1926/1959; Heider & Simmel, 1944), Michotte (Michotte, Thinès, & Crabbé, 1964), and Johansson's contributions, together with, say, those of Benussi, Musatti, Wallach, and Metzger, remained interesting phenomena that were often noted but rarely pursued. The critical conditions for "uptake" have not been satisfied until recently. Thus event perception per se has a history that is arguably as old as most topics in experimental psychology, but its consolidation as a genuine subject matter worth the efforts of research programs, as opposed to a mere collection of entertaining curiosities, owes much to Professor Johansson.

Surely the most important episode in the recent growth of interest in events was Johansson's demonstration that a very few points of light could be transformed from a meaningless jumble into a *walking person* by relative motions of

those lights (Johansson, 1973). The richness of what could be seen in these displays attracted widespread interest and continues to astonish people to this day. The availability of film techniques and computer graphics have, of course, contributed enormously to the rise of interest in event perception, but Gunnar Johansson has shown from within psychology that there are actually good problems to which we can apply these technologies.

JOHANSSON IN LIGHT OF GIBSON

Besides Gunnar Johansson, the dominant influence on the thinking of many of the people at this Conference has been James Gibson. I shall examine several of the main ideas in Johansson's paper (this volume) by comparing them with Gibson's. Gibson and Johansson were well acquainted and greatly enjoyed sharpening their ideas on one another. There is even a series of articles, one by Johansson and two by Gibson, which brings their private discussions into public (Johansson, 1970; Gibson, 1970, 1977). The purpose of my paper is to make sure that the comparisons they developed continue to be noted, examined, and elaborated. I shall focus my attention on points raised at this conference in Johansson's paper and will not repeat the emphases of the earlier exchanges. The previous papers are reprinted in the collection of Gibson's papers edited by Reed and Jones (1982).

Similarities

As Gibson and Johansson repeatedly claimed in their long association, their points of agreement outnumbered (or better, outweighed—since neither was given to dwelling on shared opinions) their points of disagreement, particularly when considered relative to the larger community of experimental psychologists. First, they agreed on the superiority of changing displays to static displays for organizing perceptual experience, maintaining that a proper stimulus analysis must be a space-time analysis. They rejected any notion that a theory of static displays should be prior to a theory of dynamic displays. Second, they agreed on the importance of invariants under transformation in perception, as Johansson made clear in his chapter on the constancies in Epstein (1977). Finally they agreed that it is important to perceive one's own locomotion and that perceiving necessarily involves sensitivity to both those aspects of a changing array specific to one's own posture and locomotion, and to those aspects which are independent of the observer. Johansson's vector analyses that separate common from relative motions address this kind of cospecification.

Differences

I shall discuss four salient differences between the two positions in roughly increasing order of importance.

Semantic Differences. The first difference is terminological and should be dispensed with quickly. As Runeson and Johansson explained at the conference, the English word "event" has been used to translate the Swedish word *skeende* which Johansson originally used.

Skeende refers to the ongoing aspect of change, emphasizing flows and repetition as opposed to boundedness, which might be captured by emphasizing a beginning and an end. Calling something a *skeende* emphasizes that it is a temporal occurrence, a process. Another word that could be translated into English as "event" is *händelse*. A *händelse* is an occurrence whose existence and dramatic quality is more the issue than its temporal extent. Normal English usage of "event" is more like a *händelse* than a *skeende*. Thus we speak of social events, theatrical events, sports events, musical events and so forth. Newspaper articles about highly publicized affairs that prove disappointing may call those affairs nonevents. A train's passage across a highway at a crossing would be a *skeende* if taken as just the motion of the train. But if it were eagerly awaited by a group of train watching enthusiasts, it would be a *händelse* for them, that is, a significant occurrence.

Given these two choices it is clear that a perceptual psychologist who studied the configurations of motions that Johansson did would call them *skeende* in Swedish. After consulting an Oxford professor, he chose the English word "event" as the closest translation and has used it in his English writing since 1950. It is his most general term for change. Recently Johansson defined "event" as a "generic concept denoting various kinds of relational change over time in a structure (1978, p. 677)." He has then found it necessary to distinguish among distal events, proximal events, and perceptual events. The German translations of Johansson have used *Geschehen*, or "event." That is, indeed, the word used by Heider in 1926 subsequently translated into English as 'event' (Heider, 1959).

Both Gibson and Johansson use "event" in the sense of *type* of event, as distinct from tokens. They are interested in changes like rolling, walking, approaching, receding, exploding, or melting, in general, not *this* instance or *that* instance. Together I think they deviate somewhat from the more common usage as token. An historical event in a broad sense is a singular happening. History can be thought of as a sequence of noted events, actual occurrences which do not repeat (in traditional western cosmology). *Types* of events (war) recur, but actual events (e.g., the Hundred Years War) make up the minimal nonrepeatable unit. *Händelse* seems to be more of a token word than *skeende*. Even though the token sense of event, as in a historical event, strikes me as more common than the type sense of "event," the context has been clear enough that I have never detected a confusion in the literature.

Gibson defined his terms somewhat differently and did not call all changes in structure events. The word "event" for Gibson referred to types of material change in the world, not to all changes of structure and not to changes of

structure in the optic array. He noted three major classes of terrestrial events—changes of surface layout or arrangement, changes of surface composition, and changes in surface integrity (1979, chapter 6). A fourth type of material change, changes in the relation between an animal and its environment, from the animal's point of view, was not called an event at all, but an "encounter."¹ Changes in optical structure (or acoustic, haptic, or chemical structure, where relevant) were said to carry information *for* both events and encounters without themselves being events or encounters. Thus Gibson's use of the word "event" became much more restricted than Johansson's, and readers of Gibson's later work (after 1966) should bear this in mind.

Up to this point I see little for Johansson to disagree with, although it is important for the wider audience to appreciate the differences in word use. Johansson's term *skeende* has been his natural term behind the English word "event." It is his most general cover term for change. Gibson's use of "event" has been more specific. Interestingly enough, since Gibson repeatedly stated that an event, as embodied change, must have its own beginning and end, it would appear that what he meant was closer to the Swedish *händelse*. This is not all that the difference in the two positions amounts to. One should not expect perceptual theory to divide neatly along lines established in the Swedish language. But taking this one contrast rather coarsely, it does seem true to say that Gibson was closer to meaning *händelse* than to *skeende* in his meaning for the word "event."

Slow Events. A more substantial, but still not very deep, distinction between Johansson's and Gibson's positions concerns the status of slow events. Both theorists know full well that all embodied things in this world (mathematical entities aside) change, but at a variety of rates relative to one another. An apple falls to earth much more rapidly than it ripens. Phenomenally we humans find it compelling to say that we perceive falling, but far less compelling to say that we perceive ripening because we do not see the characteristic changes of color and texture of the apple in the span of a typical observational act. At the opposite extreme, we do not say that we perceive the fast motion of the raster that generates a static TV image. Johansson wishes to use this difference to distinguish between perceptible and nonperceptible events. He has confined his interest to the "perceptible" events. From Johansson's point of view, the compelling difference between events whose changes are phenomenally evident and those whose changes are not is sufficient to warrant a division of subject matter into perceived and cognized events. On this view one can perceive the motion of the second hand of an analog watch but not that of the hour hand. That is apprehended by cognition.

¹Lecture at the University of Connecticut, Department of Psychology, October 1, 1976.

Gibson, on the other hand, did not use phenomenal experience as a criterion for delineating subject matter in his latest theorizing. Rather, he used the existence and availability of information. For him cases were divided between those in which information for an event or encounter was available and sufficiently sampled to detect it, and those in which a judgement (or behavioral commitment) was made in the absence of sufficient information or sampling. Thus for Gibson, neither the speed of an event nor the phenomenology is as important as whether or not information is being detected. An extended discussion of the continuity between slow and fast events may be found in Shaw and Pittenger (1978). Gibson was clearly committed to pursuing the pickup of information as a unified topic in a way that differs from Johansson's commitments (see Gibson, 1979, ch. 14). This is a genuine difference between them, but I repeat, not one that by itself divides them very deeply.

Point vs. Texture Displays. An advantage of Johansson's point-light method lies in the stark contrast between what one can see in the changing displays and what one cannot see in the static displays. The implausibility of discovering the rich structure of the changing pattern from analyses of individual "snapshots" is dramatized every time the film stops and we see just a jumble again. Neither memory, nor knowledge, nor any other "familiarity" account of event perception suggests itself.

Gibson, too, wished to devise displays that dramatized the priority of changing patterns over static ones. However Johansson's point-light method does not lend itself to the study of surfaces, which Gibson also emphasized. To study the specification and transformation of surfaces, Gibson turned to random textures. Gibson's student, George Kaplan (1969), created a series of displays that looked irresistibly like opaque surfaces moving over one another as long as the film was running, but like a single undivided surface in any individual frame. This was done by progressively adding or subtracting texture from each successive frame. Gibson (1979) argued that the changes involved in the concealing or revealing of opaque surfaces, occlusion, were fundamental optical properties of real, terrestrial environments. Moreover, he constantly stressed the fact that removing and adding texture were not the sort of changes to be found in projective geometry; hence projective geometry could not be the most general foundation for terrestrial optical theory.

Johansson, less concerned with finding a completely general theory than Gibson, has found projective geometry to be very useful for organizing the phenomena of interest to him and for suggesting new experiments. Gibson often lamented the fact that changes of occlusion were less amenable to analysis and experimentation than Johansson's point-light displays seemed to be, but held that this practical shortcoming did not make the facts of occlusion or their implications any less true.

Although Gibson talked about the information for occlusion in terms of the progressive addition or subtraction of texture, this should be taken only as a preliminary, practical formulation. It is not a final hypothesis, but a step in theory development. The most general principle of changes in the optic array was, for Gibson, what he called simply the disturbance of structure. What was called for then were increasingly precise hypotheses about what constituted relevant disturbances of structure and accompanying invariants for specifying events and encounters. Addition (accretion) or subtraction (deletion) of texture was a step, but such a description taken alone is too presumptive about what counts as texture.

There is a little known phenomenon called omega motion which makes this point nicely (Saucer, 1953, 1954; Tyler, 1973; Zeeman & Roelofs, 1953). Omega motion is simply another phenomenon that can be observed in a standard apparent motion paradigm. The only constraint is that the elements that turn on and off be larger than a point. They may be columns of two or more points, bars, or just discs that are relatively large. How large this "large" should be is a matter for further investigation. Assume we have two bars separated in space. Recall that beta motion is the name for clear apparent motion of a single bar induced by flashing these two bars on and off in sequence. If the alternation is quick enough there is more of a blur and the nature of the moving object is unclear. An observer sees "pure" motion. This is phi. In between, however, at about 2.5–3.5 cycles per second, lies omega motion. There is a figure-ground reversal where the interspace becomes a surface (or a shadow to some) that appears to move back and forth in front of a solid background, the edges of which are *seen* as alternately revealed and concealed. Omega motion, therefore, is an occlusion phenomenon, but the conditions for it are not readily analyzed as the addition and subtraction of texture. Refining a theory of the disturbance of optical structure to include this case along with those that have already been studied is clearly a challenge for the future. Gibson's interests and methods lend themselves to discovering and pursuing such phenomena, whereas Johansson's do not. The two approaches seem complementary in the sense that Gibson's focal interests would not lead one to discover and elaborate Johansson's phenomena either.

Underlying Paradigms of Perception. I turn now to the most significant of the differences in my discussion. One of the critical aspects of any scientific theory is the canonical situation it is constructed around, such as the motion of a single particle in Newtonian mechanics. What "image" does a theorist have in mind when developing particular scientific concepts? Some properties of these images will be explicit, others strongly hinted at, and still others thoroughly camouflaged. In perception, most students have adopted the spatial imagery that Johansson mentioned as their canonical model of an instance of perceiving. This is the "Distal-Proximal-Percept" model underlying any linear causal theory of

perception and made most explicit by Brunswik, Heider, and Koffka (Gibson, 1970/1982). One takes it that occurrences at a sensory organ (proximal events) are somehow caused by prior spatial-temporal occurrences so that a causal chain going from *distal stimulus* to *proximal stimulus* to *percept* is set up. Professor Johansson has maintained that Gibson emphasized the distal-proximal relation in his ecological approach and that he (Johansson) has stressed the proximal-percept relation.

This does not capture Gibson's approach, however, because Gibson reasoned from a model situation that could not be analyzed as a chain of events from environment to experience. Gibson questioned both the idea of a *stimulus* in perception (1960, 1967) and the proximal-distal metaphor. The clearest idea of what a stimulus might be is that which causes a response, as in the common view of a reflex; a goad or a prod to an animal was Gibson's usual example. A stimulus, properly speaking, is imposed. It *impinges* on one's *receptors*. But of course few people, certainly not Johansson, believe that either a distal or a proximal stimulus really *causes* perception. There are too many slips between the stimulus "givens" and the percepts. Customarily the gap between putative stimulus and percept as response has been filled with internal representations and/or processes, two of whose theoretical functions are to perform the conversion from stimulus to response and to account for failures of the two to correspond. As long as the stimulus-response model acts as a framework for thinking about perception, it will not itself be examined by experiment. Experiments can address the questions: What is the stimulus? What is the response? What lies between the two? They need not be taken to address the question: Is the stimulus-response framework proper? Yet this was one of the primary objects of skepticism for Gibson in his later years. Instead of *stimuli* Gibson offered his idea of *information*, which is pattern (optic, acoustic, etc.) specific to its sources and which is just *there*. Once there, it can be *used* (or obtained) by animals as a functional resource.² Gibson's information can be *clarified* by animals at a variety of levels of detail—all of which exist—depending on the interests and capacities of the animal. Gibson was never satisfied that a stimulus-response view could adequately characterize the *exploratory* and *guidance* functions of perceiving. The S-R mold has been far better suited to characterizing the classification and identification functions of perception. To draw the sharpest contrast, the S-R view has the environment doing something (impinging on, stimulating, etc.) to the animal, whereas Gibson thought of the animal as doing something (clarifying, obtaining, investigating, using) to the environment. Even though he distinguished between performatory and exploratory activities, it must be emphasized that his notion of exploration was also quite performance oriented when compared to more typical theories of attention. Gibson often insisted that he alone had a theory of perception which supposed a truly active observer. More

²The resource image is Ed Reed's.

traditional approaches sometimes say they have active theories but mean it in a Leibniz–Kant sense of mental activity, the sort of thing meant to reconcile stimulus and response relations, not to contravene them (see Gibson, 1976, 1979, ch. 14; Richards, 1976).

Johansson has been able to make progress on problems of interest to him without rejecting the S–R framework, just as Gibson did in earlier times when he was pursuing a psychophysical program. Johansson does not seem to worry about the details of his framework (that would be like philosophy) as long as it helps him devise interesting experiments and demonstrations. Gibson rejected the S–R paradigm because he concluded it was *false*, even though he too found it a more pliable paradigm than his own for experiments.

The spatial aspect of the distal-proximal-percept metaphor may itself be questioned further. John Dewey had questioned it (1896), Gibson's mentor, E. B. Holt questioned it (Holt, 1915) and Gibson himself was finally questioning it by the 1960s (Gibson 1970–1982). What these critics pointed out, after William James, was that there is a critical distinction between links in a physical causal chain and objects of action and perception. Holt argued that even for physics proximity was not the primary relation. He maintained that the prior question of a science was "what is an object or animal *doing*?" Answering the question, for him, required finding the proper objects of the action or motion. It raised the question, "of what is behavior a constant function?" Thus a Newtonian falling body is lawfully understood relative to the center of the earth rather than various successive places measured by a meter stick that it might be falling past. Holt noted that, in distal-proximal terms, that of which behavior is a constant function seemed to recede further toward the distal end as one considers living matter relative to nonliving, and "higher" organisms relative to "lower." A rock, then, is Newtonian, but a plant may move toward some value along a light gradient, and Little Red Riding Hood goes to grandmother's house. The idea that physically proximal "objects" are not to be confused with objects of action or perception is embodied in the distinction between physical and epistemic (or intentional) objects discussed by Shaw & Bransford (1977). Some of Gibson's more explicit reasons for rejecting the distal-proximal metaphor were stated at the 1970 conference on Ecological Optics (1970/1982). He listed five:

1. Surfaces can be distinguished from light emitters as sources of structured light. Treatments in terms of proximal stimuli concentrate on light at the eye and therefore cannot make this distinction.
2. The distal "object" can never itself *be* a stimulus, but the term "stimulus" is often used ambiguously to refer to both.
3. Gibson's pet idea of the 1950s, that texture gradients were proximal stimuli for slant relative to the line of sight, did not empirically work out.
4. The more general notion of "higher order variables of stimulation" seemed unclear and unlikely to become more clear.

5. Gibson's analyses of motion (1968) led him to the conclusion that the retina should not be taken as a frame of reference for defining what was meant by "motion." Distal-proximal thinking, on the other hand, would seem to have no choice but to consider the motion of points relative to the retina as primary "stimulus" for the perception of motion.

The alternative "picture" of the relevant model of visual perception that Gibson developed (analogues would also hold for other modes of perceiving) was that of an *ambient* optic array, something for animals to be *inside of* or *immersed in*. It is then something that can be sampled from, consistent with the explanatory imagery I tried to convey in the earlier discussion of the S-R portion of the perceptual paradigm. He thought of the structure of the optic array in terms of nested solid angles packed up against one another. Taken in this way, an optic array is a plenum; it is filled. There are no empty spaces. Thus there can be no points, no motions of points, no velocities or accelerations of points. Rather, there are changes in the overall structure of the plenum. Some of these changes, and their underlying invariants, may *specify* the motions of detached objects in the air, but this is not to be confused with the idea that isolated points of light moving relative to the retina are the underlying stimuli for an environmental event of a similar description.

Gibson's latter day ecological theorizing may be termed a *terrestrial materialism* as well as a realism. He stressed not only the primacy of surfaces as the behaviorally relevant interface between substances and media, but the absolute priority of the surface of the earth, bounded by the sky, as the frame of reference for understanding both perception and action. Johansson has often used his studies of embedded frames of reference to argue against Gibson, but it is important to realize that for Gibson the ground is the *ultimate* frame of reference in which events and encounters, in particular, are nested—if not the *only* frame of reference.

I have dwelled on this last difference as the major one because the distal-proximal stimulus analysis is so rarely questioned that it is often taken as a truism, or at worst, an innocent working assumption. Even though Gibson and Johansson stimulated and supported one another for nearly 30 years, Johansson has not acknowledged how radical a change of approach to perception Gibson was advocating.

Having staked out Gibson's ground, I return to Johansson's. At this Conference he has been humble and has heaped praise on Gibson. Even though this paper follows his lead and emphasizes Gibson's ideas, it is not meant to yield fully to Johansson's modesty. The world of experimental psychology is just beginning to catch up with Gunnar (Restle, 1979; Cutting, 1981). His career has been fertile with fascinating phenomena. By giving psychologists robust, tantalizing displays to investigate, he has surely done more to establish the autonomy of event perception than the cleverest of theories could do at this time.

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