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On Vagueness and Fictions as Cornerstones of a Theory of Perceiving and Acting: A Comment on Walter (1983)

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> *"I don't want realism. I want magic!"*
> Blanche DuBois, Scene 9, A Streetcar Named Desire

Vagueness or unclarity of thought is considered by Walter (1983) as a worthy and necessary state of (human) mind for modeling. He appeals to quantum mechanics (and, in particular, non-pure states) as, perhaps, the only fruitful model by which to understand such phenomena. The analogy takes the following form: The clarity that indeterminant ideas derive from rumination and discussion parallels the reduction of uncertainty in a parameter of a submicroscopic system that accompanies its quantum measurement. Walter suggests that with an allowance for quantum-like brain states, brains can be classified as physical symbol systems—processors that read, write, store, and compare symbols—of the type described by Newell and Simon (Newell, 1981; Newell & Simon, 1976; Simon, 1981).

As a revealing aside (developed more fully in Walter, 1980), Walter (1983) asserts that both scientists' theorizing about perceiving and animals' perceiving are largely story-telling. His implication seems to be that we invent fictions that may or may not pertain to what is *really* going on but, at least, help us muddle through our laboratories and our environments. Scientists fashion explanations (in a manner of speaking) in an attempt to sort out reaction times, thresholds, and so on, while perceivers contrive hypotheses to sort out patches of color, horizontal lines, and so on. The story's relation to reality is inconsequential as long as it is useful, where useful seems to be read as leading to the next (preferably consistent) fiction. If a fiction loses its useful-

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ness to scientist or perceiver, it can be replaced with a new one—no more real but, ideally, more useful.

As he rightly points out, Walter's position is in conflict with ecological realism. Beyond that assessment, however, whatever it is that Walter describes as ecological realism bears little resemblance to the framework carved out by Gibson over some 30 years (e.g., Gibson, 1966, 1979, 1982) and elaborated by others (e.g., Michaels & Carello, 1981; Reed & Jones, 1982; Shaw & Turvey, 1982; Shaw, Turvey, & Mace, 1983; Turvey & Carello, 1981; Turvey, Shaw, Reed, & Mace, 1981). In what follows, we shall point out where Walter missteps in his treatment of realism, clarify our conflict with his strategy, and elaborate our own strategy for modeling behavior at the ecological scale of animal-environment systems (see below). In so doing, we shall attempt to show that Walter's posture on realism, while understandable in the beleaguered heroine of Tennessee Williams' play, is less sympathetic in a (reasonably content) scientist.

Alternative or Contradictory Descriptions Do Not Deny Realism

While Walter's discontent with ecological realism includes our neglect of quantum-like brain phenomena, he sees the existence of fictions—be they scientists' oft-changing models of the world or animals' deceptive behavior in times of danger or play—as a more fundamental difficulty because they belie the claim that reality can be apprehended.

> The pervasiveness of fictions, deception, play, and so on, make the whole ideology of "realism" seem rather unlikely to me, as a productive model for mammalian nervous systems. A notion of useful fictions ("useful" perhaps to be defined in neo-Darwinian terms) seems more likely than either ecological, or naive, realism, to yield an adequate description of this most complicated organ system (p. 233).

Not surprisingly, we do not agree with this evaluation of the ramifications of such phenomena. First, dubbing them "fictions" is inaccurate and misleading. And, second, it is unlikely that fictions, with the suggestion that the attainment of goals is accidental, could ever be reliably useful. Let us elaborate this argument.

The notion that science engages in the fabrication of useful fictions has a parallel in legal practice (Walter, 1980). Just as it is convenient but incorrect to conceive of a corporation as a single person in certain legal circumstances so, too, is it useful but fictitious to conceive of space as Euclidean in some circumstances and curved in others. Walter claims that science would be better served by acknowledging that its models, however useful, are fictions "because the inconsistencies between scientific views of 'reality' in different contexts will be more damaging" (Walter, 1980, p. R366).

But do the seeming contradictions entailed by different characterizations of space, for example, remove all characterizations from the realm of reality (unqualified by quotation marks)? In other words, if a given notion changes relative to changes in the problem of interest, does this relativity preclude a consideration of that notion as objective and real? We have argued elsewhere that it does not and, indeed, that the concept of an absolute reality that would be appropriate for all grains of analysis is untenable (Gibson, 1979; Michaels & Carello, 1981; Shaw, Turvey, & Mace, 1982; cf. Prigogine & Stengers, 1984, Chap. 7).

Appropriateness is the key idea here—the level of description of reality must be commensurate with the level of inquiry, that is, with the type of systemic interactions that are of interest (cf. Rosen, 1978). Although Walter (1980) says, "When making human-scale measurements, for example, precision seldom requires us to incorporate either relativistic space curvature or super-spacelike microtopological fluctuations" (p. R367), it is not disembodied "precision" that renders such analyses unnecessary. Rather, those analyses are inappropriate because human activities *do not occur at those levels.* Human (and animal) behavior occurs with reference to the animal-specific, activity-relevant properties of the environment—what Gibson has termed affordances (1979). Affordances, it is proposed, are the appropriate level of description of reality for the ecological scale. The lengthy, difficult search initiated by Grinnel (1917) and Elton (1927) to find a systematic and evolutionarily consistent way to define the econiche—the related environmental realities supporting a given species' lifestyle—has begun to focus on the view of the econiche as an affordance structure (Alley, in press; Patten, 1982).

Affordances are both relative—they are defined with reference to a particular animal—and objective—they are defined by persisting properties of the environment. As an example, consider a brink in a surface. For an animal of a given size, that brink affords stepping down; for an animal of a given smaller size, that brink affords falling off. The reality of that particular layout of surfaces as a step-down place or a falling-off place is relative to the animal. Yet the nature of those relative realities is determined by the independent character of the surface layout—for example, that it is comprised of vertically separated substantial surfaces rather than liquid ones. This echoes a point made by Lewis (1929):

> Relativity is not incompatible with, but *requires,* an independent character in what is thus relative. And second, though what is thus relative cannot be known

> apart from such relation . . . all such relative knowledge is true knowledge of that independent character which, together with the other term or terms of this relationship, determines this content of our relative knowledge (pp. 172–173).

The coexistence of contradictory descriptions of reality (e.g., step-downable vs. not step-downable, curved vs. Euclidean space) does not mean that these descriptions are fictions (cf. Ben-Zeev, in press). It simply means that different problems appeal to different aspects of reality. No one description is universally privileged (cf. Alley, in press; Rosen, 1978). Indeed, contrary to Walter's efforts to marshal quantum phenomena in opposition to realism, the same point has been made for that domain by Prigogine and Stengers (1984):

> The irreducible plurality of perspectives on the same reality expresses the impossibility of a divine point of view from which the whole of reality is visible (p. 224). *The real lesson to be learned from the principle of complementarity*, a lesson that can perhaps be transferred to other fields of knowledge, consists in emphasizing the wealth of reality, which overflows any single language, any single logical structure (p. 225, italics added).

Biased by his concern about what scientists do when they theorize about the world, Walter is confused in his attitude toward what animals (including humans) do when they perceive their environments. He claims that the fictions by which scientists think they understand the universe have parallels in those cases where perceivers are duped by deceptions. We have already argued that scientific models of natural phenomena need not be considered fictions, even if models of the same phenomenon at different levels are inconsistent. But surely there are scientific models that are just plain wrong—phlogiston, aether, and spontaneous generation, to name a few. Do these speak to the possibility of perceivers knowing reality? They do not because they involve issues of scientific realism, not perceptual realism (see Blackmore, 1979). That is to say, the question of whether or not scientists can be successful in understanding nature is independent of whether or not perceivers are successful in knowing the environment as it constrains their day to day activities. Scientists can flounder for any number of reasons—religious dogma, bad experiments, stupidity—but for animals to "move so they can eat, and eat so they can move" (Iberall, 1974) and thereby survive, they must be in contact with the facts of their environments. Animals cannot act effectively with respect to fictions.

What of Walter's contention that the fictions are useful? Doesn't that empower them to guide activity? It is not at all clear how a fiction, unfettered as it is by actual states of affairs, could ever be useful. What guides the construction of a fiction so that it is at least relevant to an intended action—for example, a given layout of surfaces is fictionalized as being in the realm of stepping (on) or falling (off) rather than swimming (in), squeezing, eating, ad infinitum? And by what criterion might a given fiction be deemed useful? There must be some standard of comparison. If the actual state of affairs provides the comparison, realism cannot be avoided.

Deception Presupposes Realism

Walter's example of deceptive animal behavior might seem tailor-made for a fiction framework. A mother bird saves her offspring by feigning injury so that a fox will follow and attack her in the mistaken belief that her broken wing will prevent her escape. She has created a fiction—the predator perceives an injury that does not exist—that is useful in preserving her species. Such circumstances are quite rare in nature, however; not all animals engage in deception, and, for those that do, deception constitutes a small part of their behavioral repertoires. Deception provides a disputable foundation, therefore, upon which to build an account of perceiving. Nonetheless, we would emphasize the lawful basis that allows the mother to enact a successful charade and the fox to act upon it. She must constrain her musculature in just that way that will produce postural and joint adjustments specific to a particular dynamic condition (viz., material structure too weak to support the characteristic wing movement). For his part, the fox must detect the dynamics that underlie the bird's kinematic display. In order to pursue a realist basis for deceptive behavior, we will elaborate this so-called kinematic specification of dynamics (or KSD) principle (Runeson, 1977/1983; Runeson & Frykholm, 1983).

The principle starts with the reasonable assumption that, because the body is composed of certain masses and lengths and types of joints, only certain movements will be biomechanically possible. The biomechanics will also determine what one must do to maintain balance and cope with reactive forces (those "back-generated" by the act of moving). The kinematic properties of an action (its variously directed motions, its accelerations and decelerations) are determined by the dynamic conditions that underlie it—the forces produced intentionally and unintentionally by the animal and those supplied by the surrounding surfaces of support. The KSD principle suggests that a reciprocal relationship also exists: The kinematic properties of acts are transparent to the dynamic properties that caused them. For an observer, this principle reads: The ambient optic array (see Gibson, 1979; Lee, 1974, 1976) is structured by an animal's movements such that macroscopic qualitative properties of the optic array are specific to and, therefore, information about, the forces that produced the movements.

The principle finds support in experimental investigations of human movement perception that use Johansson's (1973) patch-light technique. This methodology entails limiting an observer's view of actors (i.e., people who engage in activities) to small lights that are attached to their major joints. When a person engages in some activity, a transforming pattern of lights is generated. Perceivers find this limited optical structure to be informative about a number of properties, including metrical (length of throw of an invisible thrown object of unknown mass [Runeson & Frykholm, 1983]), biomechanic (gender of a walker [Cutting, Proffitt, & Kozlowski, 1978; Kozlowski & Cutting, 1977; Runeson & Frykholm, 1983]), and kinetic (the weight of a lifted box [Runeson & Frykholm, 1981]). Importantly, Runeson and Frykholm (1983) have shown that perceivers are not easily fooled by actors' efforts to be deceptive. Despite attempts to fake the weight of a lifted box, observers not only perceive the real weight but are aware of the deceptive intention and the intended deception (i.e., what weight is being faked) as well. Similar results are found in attempts to be deceptive about one's gender (through gait and carriage in a variety of actions)—observers are aware of both real gender and faked gender. The point to be underscored is that an actor can structure light in ways that provide information about conditions that do not exist (see Gibson, 1966; Michaels & Carello, 1981; Turvey et al., 1981 for realist accounts of this fact) while simultaneously (and unavoidably) providing information about conditions that do exist, and perceivers can be aware of both.

Runeson and Frykholm draw a parallel with the dual reality of pictures, especially as it has been described by Gibson: There is information about objects represented in the picture and information about the picture itself as an object. "The duality of information in the array is what causes the dual experience" (Gibson, 1979, p. 283). The possibility of dual awareness may speak to the dearth of true deceptions in nature. For very sound physical reasons, situations that lend themselves to single awareness deception are, contrary to what Walter seems to imply, difficult to manufacture and, in consequence, quite rare. Intraspecific threat and play behavior, on the other hand, are found throughout the animal kingdom. But it seems to be a misnomer to label these "deceptions" in the sense of trickery. Baboons who bare their teeth have not fabricated a fearsome weapon. They are suggesting that they would rather not use the ones they have. Chimpanzees who play attack-and-flee are not deluded; they behave differently in true fight-escape circumstances (Loisos, 1969). Play provides an opportunity to learn about one's environment, conspecifics, and one's own behavioral possibilities.

We have argued that characterizing perception as useful fictions is inadequate to explain behavior in natural circumstances. An explanation of effective behavior requires a realist framework with the animal-environment system as the unit of analysis. Walter, however, is skeptical of whether such an analysis is possible. We contend that his objection is based on an overevaluation of what can be distilled from brain state accounts and a misunderstanding of what "animal-environment system" means. We will deal with each of these issues in the next two sections.

Brain States are an Inadequate Basis for Ascribing Intentional Content

Walter implies that any perspective that does not advert to observations of brain states cannot provide a dynamically useful formulation of behavior. However, he prudently avoids any discussion of how observations of brain states would yield the proposed useful formulation. Presumably, Walter's advocated observations or measurements of the brain—no matter how precise or vague those measurements may be—would provide only extensional descriptions. And, presumably, a physical or biological theory of the brain strictly consistent with such observations could only be extensional. At best, observations of brain states, purely interpreted, would lead to an account roughly of the form: In the context of functional brain organizations P and Q, functional brain organization R has the capacity of inducing functional brain organization S. This would not be a dynamically useful formulation of behavior. No matter how elaborate and detailed such an extensional account becomes, it will never allow Walter to answer apparently straightforward questions about prosaic behaviors. For example, how does an outfielder know to charge in rather than retreat to catch a ball (Todd, 1981)? Why does a child, on seeing a particular surface, initiate crawling rather than walking to traverse the surface (E. Gibson, 1983)? The important ingredient missing from the foregoing brain-state based account of behavior is intentionality.

A dynamically useful formulation of behavior grounded in observations of brain states requires minimally (1) a principled basis for individuating brain states, and (2) a principled basis for ascribing content to individuated brain states. The latter refers to the problem of systematically upgrading the extensional characterizations of brain states to intentional characterizations, ordinarily expressed by intensional statements (Dennet, 1969; Fodor, 1981; but see Searle, 1983). The point is that without identifying the contents (the significances, the meanings, the message functions, the signalling functions, etc.) of brain states, the brain theorist's view of brain function in relation to behavior is empty. The intentional characterization earns for the brain theorist the luxury of addressing the question of what the brain states are *about*. From what observations and on what grounds would an advocate of the explanatory power of brain states fashion intentional characterizations? Those characterizations arise at and are the sine qua non of the ecological scale of animal-environment systems.

Intentional characterizations should not be interpreted as referring to systemic states that are in addition to or separate from those extensionally characterized. Intentional characterizations usually comprise *alternative* (discrete, symbolic) descriptions of a system's states, descriptions that complement the extensional (continuous, dynamical) accounts of *how* a system is doing what it is doing. Pattee (e.g., 1973, 1977) has been foremost in identifying the problem of understanding how these two complementary modes of description of any complex system can be treated in a physically consistent way. The ecological approach to perception and action has been concerned similarly with the complementarity of intentional and extensional characterizations (e.g., Carello, Turvey, Kugler, & Shaw, 1984), but it has been concerned more directly with elaborating the extensional basis for ascribing intentionality to states of the animal-environment system in a principled manner (e.g., Gibson, 1979; Kugler, Kelso, & Turvey, 1980, 1982; Turvey et al., 1981). This strategy has been chosen because the principled ascription of content to the states of a system rests ultimately on the accuracy and specific predictions of the extensional account of the system. As Dennett (1969) puts it:

> The ascription of content is thus always an ex post facto step, and the traffic between the extensional and the intentional levels of explanation is all in one direction (p. 86).

To the extent that the extensional basis for a system's phenomena is underestimated and/or unknown, the intentional characterization of the system is likely to be ungrounded and fatuous; ordinary systemic states get ascribed near magical functions or powers (section below). And this latter statement identifies, in a nutshell, the danger and inadequacy of seeking an account of behavior, as Walter advocates, in observations limited to brain states.

The Animal-Environment System as the Appropriate Unit of Analysis

Walter focusses his attack on realism on Turvey and Carello (1981). He discusses the position thusly:

> This position claims that the joint situation of an organism and its environment is the only correct fundamental concept for brain/mind modeling . . . I regard their presumption that a state of the brain-and-environment nexus *can be observed* as a fatal flaw in ecological realism. In my view, the state of a mammal's brain cannot, in most situations, usefully be observed . . . without so severely interfering with that state, by your observing . . . that the state will change in an unpredictable and uncontrollable way . . . (p. 231).

Interestingly, the word "brain" never appears in the Turvey and Carello manuscript. Indeed, eschewing brains as the appropriate entities to model for an understanding of psychological phenomena is at the heart of using *ecological* to modify our brand of realism. We are interested in how organisms (including humans) are able to perceive their propertied environments in a way that will allow them to behave effectively with respect to those environments. A runner—be it human, gnu, or cockroach—does not steer around representations or brain states; it avoids real obstacles and goes through real openings. Couching problems in such terms is not, as Walter claims, simply a "programmatic and descriptive phase" that ecological realism is going through. The "dynamically useful formulation of behavior" that Walter asserts is unavailable from our strategy not only is found in a realist framework but, we would argue, can only be provided by such a perspective. One of Gibson's favorite examples—the problem of controlled collisions in locomotion—will be used to buttress this argument.

As an animal moves through a cluttered surround, it sometimes steers around objects, sometimes contacts them gently, and sometimes collides with them violently. In order to control encounters with the environment, activity-relevant (dynamically useful) information must be available. This includes information specific to what is moving (e.g., the animal or the objects that surround it), direction of locomotion, obstacles and apertures in one's path, time to contact (if it should occur), and force of contact (if it should occur). This information has been demonstrated by a number of investigators (e.g., E. Gibson, 1983; J. Gibson, 1979; Lee, 1976, 1980; Lishman & Lee, 1973; Schiff, 1965) to exist in what might be termed the morphology of the optic flow field (Kugler, 1983; Kugler & Turvey, in press; Solomon, Carello, & Turvey, 1984). We will highlight some of the findings here but for detailed analyses, the reader should refer to the cited works.

Although the problem of distinguishing one's own movement from displacements of the surround has been a long-standing puzzle in orthodox accounts of perceiving, Gibson (1979) provided a simple solution, viz., global, smooth change in the optic array specifies egomotion, local discontinuous change specifies motion of an object in the environment. Moreover, one's direction of locomotion is also specified by the form of the optic flow field: Global optical expansion specifies forward movement (where the focus of expansion specifies the point toward which one is moving) while global optical contraction specifies retreat (where the focus of contraction specifies the point from which one is moving). If the appropriate flow fields are generated, the appropriate actions will be constrained (e.g., in the face of simulated global optical expansion, a person will make postural adjustments backward to compensate for the perceived forward movement [Lishman & Lee, 1973]; when confronted with local optical expansion, a person [or animal] will duck [Schiff, 1965]). The same sort of analysis distinguishes obstacles from apertures: A closed contour is specified as an obstacle when there is a loss of structure outside the contour during approach; it is specified as an opening when there is a gain of structure inside the contour during approach (J. Gibson, 1979). Infants as young as six months will duck from approaching obstacles but try to look inside approaching openings (E. Gibson, 1983).

If an animal wishes to steer around objects, it must move in such a way that optical expansion is centered in openings rather than obstacles. In order to contact objects (and to vary the force with which they are contacted), two more optical flow properties are needed. The inverse of the rate of dilation of a topologically closed region of the optical flow field (e.g., that structured by a wall) specifies the time at which a moving animal will contact that region. The derivative of the time-to-contact variable is information about the imminent momentum exchange: If it is greater than a certain critical value, the animal will stop short of contact; if it is equal to that critical value, the contact will be soft; if it is less than that critical value, there will be a momentum exchange and the contact will be hard (Kugler, Turvey, Carello, & Shaw, 1984; Lee, 1976, 1980).

Notice that these properties do not exist in the animal or in the environment but *are only defined for the animal-environment system.* The components of the system are not ruled by the indeterminacy that governs conjugate variables in quantum mechanics. That is to say, an exact description of one component does not mean that the other component cannot be determined. On the contrary, measuring one of the components in isolation not only fails to provide an understanding of the system but gives a misleading picture of the component that is being measured. This is the problem of overdecomposing a partial system from the total system that includes it (Turvey & Shaw, 1979; cf. Ashby, 1963; Humphrey, 1933; Weiss, 1969). Although science requires decomposition to a certain extent in order to make its problems manageable, the parsing of systems cannot be done cavalierly. An unprincipled selection of a system in which a phenomenon is thought to reside may make the phenomenon appear capricious and compel the scientist to attribute magical powers or content to the partial system (Ashby, 1963; Turvey & Shaw, 1979). The appropriate grain of analysis, however, may reveal the law-governed determinacy that is unavailable in the partial system (Weiss, 1969).

For example, if we take a climber-stairway system (Warren, 1984) as an instance of an animal-environment system, several points can be illustrated. First, there is optical information for a category boundary for action—perceivers can see which of a variety of stairways (constructed with risers of varying heights) are climbable in the normal way (i.e., without using hands or knees). Second, there is a perceptual preference for stairways that would be easiest to climb (as determined by measures of energy expenditure during climbing). Third, both of these relationships can be described by a method of intrinsic measurement, in which one part of given system (e.g., on the animal side) acts as a *natural standard* against which a reciprocal part of the system (e.g., on the environment side) can be measured (Warren, 1984; Warren & Shaw, 1981; cf. Bunge, 1973; Gibson, 1979). Thus, the critical riser height/leg ratio, indexing the action boundary, is .89 whereas the optimal ratio, indexing minimum energy expenditure, is .26. These ratios are the same for all climbers, short and tall. Finally, each of these ratios is a measure of animal-environment fit; each is an index of the state of that system. Notice that, unlike Walter's quantum systems, the state does not change by measuring it and predictions are not invalidated by observations. For a given individual, if the ratio of riser height to leg is less than or equal to .89, the stair will be climbable; if the ratio equals .26, that stair will be (relatively) energetically cheap to climb. Those relationships do not change. And nowhere in this analysis is it suggested that brain states can be or ought to be observed.

Brainstates are Not the Touchstone for Theories of Knowing

Walter would not deny that behaviors like stairclimbing are observable without interference from the observer but he would, no doubt, claim that they are not useful or worthwhile to model.

> I have (Walter, 1980) characterized those aspects of behavior that *are* predictable from less severely interfering observations, as rather gross and physicalistic (contrasted with "psychodynamic"); they seem to obey a correspondence principle or classical limit. They also tend toward conspiring to give a systematically misleading impression . . . that they are a closed system, adequate to describe the brain (pp. 231–232).

Though "gross" may be used pejoratively, perceiving and acting are unabashedly macrophenomena. Walter's implication that the only interesting behavior is a microbehavior will sever him from consideration of a gannet's dive for a fish (Lee & Reddish, 1981), the baseball fielder's catch or a deep fly ball (Solomon, Carello, & Turvey, 1984; Todd, 1981), and his own efforts to avoid destruction on the San Diego Freeway (Gibson & Crooks, 1938). While microphenomena may have their place, that place is not a privileged one. They need not and will not serve all of science. Once again, this attitude is not idiosyncratic to ecological realists. Rosen (1978), for example, in stressing the functional and organizational character of certain physical systems, observed:

> What seemed to be emerging from such considerations was apparently the antithesis of the reductionist program: instead of a single ultimate set of analytic units sufficient for the resolution of any problem, we find that distinct kinds of interactions between systems determine new classes of analytic units, or subsystems, that are appropriate to the study of that interaction (p. xvi). [These] families of analytic units, all of which are equally "real" [are] entitled to be treated on the same footing; the appropriate use of natural interactions can enormously extend the class of physical *observables* accessible to us . . . (p. xvii, italics added).

Once again we see the theme of appropriate levels of reality, this time directed at the question of what counts as an observable for physics.

We suspect that Walter would not be sympathetic to the above line of argument, countering that we ought to focus on what qualifies as a legitimate observable for psychology, instead of physics, for problems of knowing. This is apparent in his contrasting "physicalistic" with "psychodynamic" aspects of behavior, charging that the former are not "adequate to describe the brain." This is where his emphasis on vague states of human mind during thinking, rumination, and the like clashes most dramatically with our concern for the very unvague states of animal-environment systems during perceiving and acting. In his desire to understand brain (as the seat of mind), Walter holds thinking and, in particular, vague thinking as the focus of any theory of epistemic agents. But for us, reliable and reproducible behaviors must be the touchstone for any account of knowing. In infinitely varying settings, organisms are able to produce the same appropriate behavior consistently, adapting it to the particular circumstances. For example, countless times a day a bird will take off from a variety of surfaces of support at a wide range of heights and fly toward other surfaces of support at varying distances away, alighting on them gently. Sometimes it will steer around trees or pet cats and sometimes it will have a direct flight. Obstacles to and paths for locomotion and the appropriateness of accelerations and decelerations can be neither indistinctly specified in optical flow fields nor unreliably detected if the bird is to locomote through its cluttered terrain successfully. It is these kinds of behaviors, not indeterminate contemplations, that should provide the standard against which to judge the adequacy of theories of knowing.

The example of a bird in flight is an important one because it contains one feature—collisions with plate glass windows—of the sort that Walter, among others, uses to try to refute realism. The style of the argument can be characterized as follows: A bird who sees the window as an opening and flies into it has not perceived reality correctly and has not acted effectively. But in situations of so-called perceptual "mistakes," we embrace the distinction drawn by Lewis (1929)—ignorance of reality is not to be equated with erroneous knowledge of reality. A window does not structure the optic array at all points of observation so as to specify the substantiality of the transparent surface. The bird is ignorant of that aspect of reality because information about that aspect is not available to those points of observation along the bird's approach. Information about substantiality is available, however, to other points of observation, viz., on those paths where the optic array is structured by more reflective angles of the glass. When information about an obstacle to locomotion is not available, a bird will not change its path of locomotion. Perception in the first case is veridical; perception in the second case is "veridical but partial" (Lewis, 1929, p. 176).

A Final Note

The ecological approach addresses common behaviors under the general rubric of controlled collisions (Kugler et al., 1984) or controlled encounters (Gibson, 1979). Such behaviors cut across species and allow us to highlight the very small number of design principles responsible for the wide range of activities that nervous systems support. While the processes that thinkers go through in conceiving and refining their ideas are intriguing, they should not provide the starting point for an explanation of perception in the service of activity. Putting them at the forefront of things to be explained is an apotheosis of the exotic and likely to be premature. As a parallel, consider the rainbow, which has fascinated philosophers and scientists for centuries. An adequate quantitative theory that accounts for all of the features and quirks of that phenomenon awaited the development of geometrical optics, and an understanding of the wave and particle-like properties of light, polarization, and the complex angular momentum method (Nussenzveig, 1977). We may have to be similarly thorough in uncovering those *fundamental* principles at the ecological scale on which the reliable and reproducible behaviors of epistemic agents are based and on which an acceptable account of thinking will rest.

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