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Two visual systems and two theories of perception: An attempt to reconcile the constructivist and ecological approaches

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Abstract: The two contrasting theoretical approaches to visual perception, the constructivist and the ecological, are briefly presented and illustrated through their analyses of space and size perception. Earlier calls for their reconciliation and unification are reviewed. Neurophysiological, neuropsychological, and psychophysical evidence for the existence of two quite distinct visual systems, the ventral and the dorsal, is presented. These two perceptual systems differ in their functions; the ventral system's central function is that of identification, while the dorsal system is mainly engaged in the visual control of motor behavior. The strong parallels between the ecological approach and the functioning of the dorsal system, and between the constructivist approach and the functioning of the ventral system are noted. It is also shown that the experimental paradigms used by the proponents of these two approaches match the functions of the respective visual systems. A dual-process approach to visual perception emerges from this analysis, with the ecological-dorsal process transpiring mainly without conscious awareness, while the constructivist-ventral process is normally conscious. Some implications of this dual-process approach to visual-perceptual phenomena are presented, with emphasis on space perception.

Keywords: constructivist; dual-process approach; ecological; size perception; space perception; two visual systems; visual perception theories

> "Only connect"
> Epigraph to E. M. Forster's *Howard's End*

1. Introduction

Two contrasting theoretical approaches to visual perception are currently predominant: one consists of variants on the classical Helmholtzian constructivist-inferential approach (e.g., Gregory 1993; Rock 1983; 1997); the other, of the newer Gibsonian ecological-direct approach (Gibson 1979). On the face of it, these two theories seem quite incompatible, espousing rather contradictory views of how visual perception transpires. However, I will try to demonstrate that each of these seemingly contradictory theoretical approaches comprises a somewhat different aspect of visual perception, and that both can co-exist. These two aspects have been delimited by recent neurophysiological, neuropsychological, and psychophysical research indicating the existence of two parallel visual systems, here labeled the dorsal and the ventral systems. The central tenet to be presented here is that these two visual systems parallel the ecological and constructivist approaches to perception, respectively, in their function. In other words, it is being suggested that these two visual systems contribute to our pickup of visual information and our perception of the visual world. The ventral system is seen to function in a manner commensurate with the Helmholtzian constructivist approach, and the dorsal system in a manner much more similar to Gibson's (1979) ecological approach.

Before starting, it is important to clarify the usage of the term "perception" in this article. Perception can be defined in more than one way. It is often defined narrowly as the *conscious* awareness of the objects and events in the perceiver's environment. Such definitions are in line with the constructivists' approach, and almost totally exclude dorsal system functions from "perception," leaving only the ventral system to partake in this process. This is the tack taken by Milner and Goodale (1995) in their interpretation of their very important findings concerning the two visual systems. I will argue for a broader definition where perception is seen to encompass both conscious and unconscious effects of sensory stimulation on behavior. This broader definition is more commensurate with the attempt made here to include both approaches, the constructivists and ecolog-

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ical, under a common framework. It is also necessitated by the findings that indicate that many perceptual activities can be carried out by both systems and that they often interact synergistically in these perceptual activities. But, to assist the reader as to which system I believe to be involved in a given perception, I will refer to the dorsal system as "picking up" (information), following Gibson, and to the ventral system as "perceiving" the stimulation in question.

Section 2 will begin with a brief review of the two theoretical approaches, the constructivist and the ecological, followed by a look at some previous claims that the two approaches are not incompatible. This will be followed by a summary, in Section 3, of some of the more relevant findings concerning the two visual systems, and their currently assumed functions. These two sections are essentially literature reviews, and some readers might want to skip them, moving directly to the more central theses of this paper. Section 4 will take a second look at the two theories and some of the research carried out under their aegis and try to demonstrate the parallels between ecological theory and its research and the functions of the dorsal system, and between constructivist theory and its research and the functions of the ventral system. In most instances, the examples will be from the domain of space perception with emphasis on size perception or size constancy, that is, the invariance of perceived size with distance variant. Hopefully, choosing examples from a single domain will yield a more coherent presentation, but, of course, there will remain the question of generalization to other domains. Finally, section 5 will try to summarize the emergent dual-process approach, and show how it sheds new light on some topics in visual perception, and point to some of its relations to other theoretical accounts. A brief look at the conclusions will appear in section 6.

2. Two competing theories of perception: The constructivist and the ecological

The two competing theories, or variants on these theories, have been given a wide assortment of labels. The older theory, which I will in the main refer to as constructivist or indirect, has also been called Helmholtzian, cognitive, algorithmic, and mediational, among other labels. The newer theory, which I will usually refer to as ecological, direct, or Gibsonian, has also been called sensory, proximal, and immediate, among other labels. There are those who equate the constructivist approach with a computational theory of vision. But as has been pointed out (e.g., Epstein 1980; Hatfield 1990b), both theories can be seen as computational, the differences between them depending on what type of information those computations process. The constructivist approach is seen to process information beyond that found in the sensory stimulation while the ecological approach limits itself only to information in the stimulation. No attempt will be made to give a thorough and complete review of these theories but simply my hopefully unbiased understanding of them. The constructivist view has taken on several somewhat different stances over the years, and Epstein (1995) has recently briefly reviewed several of these. The much newer ecological approach (but see Lombardo 1987) is mainly the product of the life work of Gibson, as spelled out in his last book (Gibson 1979). Here I will simply point to some of the central themes of these theories, especially those that are relevant to what constructivists might call "space perception" and Gibson would have probably called "the pickup of information about the affordances of the ambient environment."

Let me start by stating in very general terms what I believe to be the major differences between the two approaches to perception. These relate to two interrelated topics, the richness of the stimulation reaching our sensory apparatus, and the involvement of "higher" mental processes in the apprehension of our environment. The constructivists see the stimulation reaching our senses as inherently insufficient, necessitating an "intelligent" perceptual system that relies on inferential types of mechanisms to overcome this inherent equivocality of stimulation. The ecologically oriented theorists argue that the information in the ambient environment suffices and is not equivocal, and thus, no "mental processes" are needed to enable the pickup of the relevant information. The constructivists see perception as multistage with mediational processes intervening between stimulation and percept, that is, perception is indirect. The ecological theorists see perception as a single-stage process, that is, it is direct and immediate. For the constructivists, memory, stored schemata, and past experience play an important role in perception. The ecologically oriented approach sees no role for memory and related phenomena in perception. Finally, the two approaches differ on the aspects of perception they emphasize; the constructivists excel at analyzing the processes and mechanisms underlying perception, while the ecological approach excels at the analysis of the stimulation reaching the observer. This is clearly a very oversimplified account of the differences between the two views, but I believe that it contains the gist of the main differences between them. Let us look at the two approaches in somewhat greater detail.

2.1. The constructivist approach

Of the two competing theoretical approaches, the constructivist approach is the older, more "classical," approach.

Although its roots are much older, many see Helmholtz as its modern forefather, often citing his notion of "unconscious inference" as the forerunner of current constructivistic thinking (see e.g., Rock 1977). In reality, the Helmholtzian notion of unconscious inference was more encompassing than its current equation with the "taking-into-account" notion (see below). It intertwined perceptual processes with the nativism-empiricism debate with Helmholtz, utilizing it to reinforce his empiricist stance (see Hatfield 1990a, Ch. 7).

More recently, Boring (1946) borrowed and sharpened Titchener's (1914) distinction between core and context to explicate the results of the classic Holway and Boring (1941) experiment. In that experiment observers judged the size of a disk, presented at varying distances, under conditions of increasing "reduction," that is, where more and more distance cues were eliminated. Their finding was that the more cues "reduced," the poorer the size constancy, that is, the more the judgments were of the proximal size and not of the distal size. Boring (1946) writes:

> For descriptive purposes it is convenient to say that the sensory data that contribute to a perception can be divided into a core and its context. The *core* is the basic sensory excitation that identifies the perception that connects it most directly with the object of which it is a perception. The *context* consists of all the other sensory data that modify or correct the data of the core

> as it forms the perception. The context also includes certain acquired properties of the brain, properties that are specific to the particular perception and contribute to the modification of its core. In other words, the context includes knowledge about the perceived object as determined by past experience, that is, by all the brain habits which affect perceiving.
>
> In visual perception the core is the retinal excitation, that is to say, the total optical pattern, specified with respect to the wavelengths and energies involved and the spatial distribution and temporal changes of each. Thus in the visual perception of size with distance variant, the core is the size of the retinal image. The context includes all the clues to the distance of the perceived object – clues of binocular parallax and convergence, and of lenticular accommodation and perspective, as well as the other monocular clues to the awareness of distance. . . . (Boring 1946, p. 100)[1]

Boring's is a strong constructivist stance, where the process of perception consists first of a core stimulus, a proximal image of the disk subtending 1° on the retina, and this core is modified by the context, by all the cues (clues) that yield information about the distance of the disk. This modification process mediates between the core and the final percept, the more complete the information about distance (the fewer the cues "reduced"), the more the percept matches the distal stimulus. In other words the perceptual process takes into account the perceived distance in attempting to assess the true, distal, size of the disk. This "taking into account" formulation of the perceptual constancies was elucidated in an article by Epstein (1973) where he spelled out the underlying common mechanism for seven constancies. That mechanism consists of a combinatorial process where, for each of the constancies, two independent variables yield the distal attribute. In the case of shape constancy, for example, the variables are the projective shape (the local retinal attribute in Epstein's usage or the core in Boring's) and the concomitant variable, the apparent slant, which together yield the perception of the distal attribute, in this case the apparent shape. The elegance of a common mechanism for all these constancies is somewhat marred, as Epstein points out, by the fact that empirical tests have not always yielded results consistent with it. Epstein points to several reasons for this, one of which is based on the distinction between perceived and registered variables. This refers to the fact that there may be a difference between the perception of the concomitant variable and its registration in the nervous system. Taking size constancy as an example, which relies on distance information according to this view, it is being suggested that the perceived or reported distance differs from that registered by the nervous system. Experiments attempting to verify the "taking into account" hypothesis have utilized the reported distance, but this is different from the registered distance, and it is possible that it is the latter that combines with the core size in yielding the size percept. I will return to this topic in section 5.2.3.

The most prolific proponent of the constructivist approach in recent years has been Irvin Rock (e.g., 1977; 1983; 1997). His *The Logic of Perception* (1983) is a treatise devoted in its entirety to documenting the evidence in favor of the constructivist view, and his recent *Indirect Perception* (1997) is a collection of papers seen to support that view accompanied by his introductory chapter and his comments at the beginning of the sections. The first sentence in the former work is: "The thesis of this book is that perception is intelligent in that it is based on operations similar to those that characterize thought" (Rock 1983, p. 1). A little later in the book he makes it clear that this thought-like process occurs unconsciously. Equating perception and thought processes is adopting a rather extreme position, both because it is difficult to envision the exact parallel between the two, and because it is very difficult to empirically verify its validity. In the introduction to the first section of his later book, Rock (1997) takes a somewhat different tack, explaining that indirect perception means "that perception is based on prior perception, implying a perception-perception chain of causality." This interdependency of perceptual processes is something that can be examined empirically, and, indeed, the studies reprinted in this book clearly evidence such a chain of causality. Actually, one senses a transition from the stance of the strong opening sentence in Rock's (1983) book where, in the tenth chapter entitled "Perceptual Interdependencies," Rock develops this idea based on the writings of several notable students of perception (Epstein 1982; Gogel 1973; Hochberg 1956; 1974).

Epstein (1982) cites several examples of such perceptual interdependencies. These he labels "percept-percept couplings" (after Hochberg 1974), where the perception of one stimulus dimension is altered by changes in a different stimulus dimension. One well-known example is Gilchrist's (1977; 1980) experiments demonstrating that the perceived lightness of a reflecting patch can be changed drastically by manipulations of the stimulus situation affecting where it is perceived to be (e.g., in a dimly vs. well-lit room) or what its physical slant is (e.g., facing the light source or not). Percept-percept couplings, according to Epstein and Rock are an anathema to direct theory in that "the cardinal tenet of direct theory cannot be sustained. The percept in question will have been removed from direct control by information in stimulation" (Epstein 1982). Epstein also presents evidence favoring a causal interpretation rather than a correlational interpretation of such percept-percept couplings. For example, a set of studies by Gogel and Tietz (1973; 1974; 1977; 1979) show that completely independent stimulus manipulations such as changes in oculomotor convergence or motion parallax, affect perceived distance in a similar manner. It should be noted that in most of the examples of such percept-percept couplings presented by Epstein (1982) and Rock (1997), the second, concomitant, variable manipulated (not the core) consists of some manipulation of the stimulus situation affecting the subject's perception of three-dimensional (3D) space. I shall return to this topic in section 5.2.2.

Returning once again to the question of size perception, Rock (1983) specifically invokes a syllogistic inferential mechanism:

> I will argue that the process of achieving constancy is one of deductive inference where the relevant 'premises' are immediately known. That is to say, in the case of a specific constancy such as that of size, two aspects of the proximal stimulus are most relevant, one being the visual angle subtended by the object and the other being information about the object's distance. (p. 240)

Like Boring and Epstein, Rock sees size perception as depending on two perceptions, that of proximal size and that of distance, together leading through a syllogism to the veridical distal percept. While there are slight differences in emphases between these three researchers, all three call for some sort of combination of proximal size information and distance information in the achievement of size constancy. In a similar manner, the same combinatorial process holds for all the constancies, according to the constructivist view. Those who adopt the ecological view, as will be seen in the next section, do not accept this view.

2.2. The ecological approach

Gibson's (1979) ecological theory as expounded in his *The Ecological Approach to Visual Perception* evolved over his entire career (see the fascinating account in Reed 1988). In that book, Gibson presented an exciting new approach to the study of visual perception that included many new concepts and new ways of looking at perception. The entire first half of the book is devoted to a novel analysis of the ambient environment and the information it proffers the observer. Gibson finds the classical approach of describing the stimuli for perception in terms of stimulus energies impinging upon the receptors completely unsatisfactory. He points to the differences between these energies and the optical information available in the ambient optic array. That information is *picked up* by a stationary or moving observer. Gibson, like Johansson (1950), calls attention to the fact that perception consists of perceiving events; that is, perceiving changes over time and space in the optic array.

Perhaps one of Gibson's most important contributions is the concept of *affordances.* Gibson writes: "The *affordances* of the environment are what it *offers* the animal, what it *provides* or *furnishes,* either for good or for ill" (1979, p. 127). Mark (1987) defines affordances as "the functional utility of certain environmental objects or object complexes taken with reference to individuals and their action capabilities." Gibson gives examples of the various affordances of surfaces, such as "stand-on-able," "climb-on-able," or "sit-on-able," and writes:

> The psychologists assume that objects are *composed* of their qualities. But I now suggest that what we perceive when we look at objects are their affordances, not their qualities (1979, p. 134).

and:

> the basic affordances of the environment are perceivable and usually perceivable directly, without an excessive amount of learning. The basic properties of the environment that make an affordance are specified in the structure of ambient light, and hence the affordance itself is specified in ambient light. Moreover, an invariant variable *that is commensurate with the body of the observer himself* is more easily picked up than one not commensurate with his body. (1979, p. 143)

Quite a few experimental studies of affordances have been published, focusing on a variety of topics such as the affordance of stairs for climbing, the affordance of chairs for sitting, or the affordance of apertures for walking through. I will return to one of these and to the concept of affordances once again in section 4.1.

Gibson's is a theory of direct perception and he describes it as follows:

> So when I assert that perception of the environment is direct, I mean that it is not mediated by *retinal* pictures, *neural* pictures, or *mental* pictures. *Direct perception* is the activity of getting information from the ambient array of light. I call this a process of *information pickup* that involves the exploratory activity of looking around, getting around, and looking at things. (1979, p. 147)

What sort of information is picked up in direct perception? Gibson suggests that there exist higher-order invariants[2] in the optic array that serve to supply the observer with unequivocal information. He musters a great deal of evidence to prove this point. Among the items of evidence he presents is a study of size perception he performed during World War II. In that study he presented aviation cadets with the task of matching the height of stakes planted at various distances in a very large plowed field with a set of stakes of varying size nearby. His finding was that size perception remained invariant no matter how far away the stake was planted: "The judgments became more *variable* with distance but not smaller. Size constancy did not break down" (1979, p. 160). Unlike the constructivists Gibson does not ascribe this size constancy to the taking-into-account of distance, but rather:

> The implication of this result, I now believe, is that certain invariant ratios were picked up unawares by the observers and that the size of the retinal image went unnoticed. No matter how far away the object was, it intercepted or occluded the same number of texture elements of the ground. This is an invariant ratio. For any distance the proportion of the stake extending above the horizon to that extending below the horizon was invariant. These invariants are not cues but information for direct size perception. . . . (1979, p. 160)

Gibson is suggesting that size constancy results from the direct pickup of invariant ratios in the ambient array. He proposes two such invariant ratios, the amount of texture intercepted and the horizon ratio. It is also noteworthy that he claims that these invariant ratios are picked up "unawares." There is no need, according to his view, for perceived distance to be involved here, nor for the inferential mental processes that the constructivists purport underlie size perception.

> . . . . both size and distance are perceived directly. The old theory that the perceiver *allows for* the distance in perceiving the size of something is unnecessary. (1979, p. 162)

Gibson's conception is one of an active perceiver exploring his environment. Eye-, head-, and body-movements are part and parcel of the perceptual process. Perception transpires continuously over both time and space. "Space" here refers not to an empty space but to the many surfaces that make up the environment, the most important being the terrain that at times reaches the horizon. The horizon is of importance as it serves as an important reference standard, and when it is occluded Gibson speaks in terms of an implicit horizon, presumably similar to what architects and others have called the eye-level plane. With such a conception, Gibson is totally adverse to the reductionist experimental paradigms. Brief exposures or looks through monocular "peep-holes" do not represent true perception in his view. In discussing the famous Ames demonstrations of the trapezoidal room or window, he writes:

> An observer who looks with one eye and a stationary head misperceives the trapezoidal surfaces and has the experience of a set of rectangular surfaces, a 'virtual' form or window. . . . The eye has been fooled.
>
> The explanation is that, in the absence of information, the observer has presupposed (assumed, expected, or whatever) the existence of rectangular surfaces causing the solid angles at the eye. (1979, p.167)

Gibson also eschews the idea that a perceptual system has a memory. He claims that "there is no dividing line between the present and the past, between perceiving and remembering" (1979, p. 253).

In his book, Gibson almost totally refrains from discussing the processes underlying perception. Perception is simply the pickup of information from invariants in the ambient environment. His only allusions to underlying processes are in terms of resonance:

> In the case of the persisting thing, I suggest, the perceptual system simply extracts the invariants from the flowing array; it *resonates* to the invariant structure or is *attuned* to it. In the case of substantially distinct things, I venture, the perceptual system must *abstract* the invariants. The former process seems to be simpler than the latter, more nearly automatic. (1979, p. 249)

In their explication of Gibson's approach, Michaels and Carello (1981) are somewhat more explicit about what they call "The Resonance Model." They refer back to Gibson's (1966) radio metaphor for perception, pointing out that "the recognition or detection of radio waves is based on principles of resonance." They suggest that the environment "broadcasts" information and that information must be "tuned in." They also point out that the radio metaphor is lacking on two counts. First, it accounts for only the perceptual part of the perception-action continuum, and second, a radio needs someone to tune it, while a perceptual system is a self-tuning device.

2.3. Calls for reconciliation and unification

In spite of the sharp contrasts between the constructivist and ecological approaches, there were those who, not long after the publication of Gibson's (1979) last book, called for seeking out ways to reconcile the two approaches. My own "awakening" came from the results of three experiments on size perception (Norman 1980). Those experiments, somewhat naively, aimed at pitting the two approaches, the constructivist and the ecological, against each other by examining the effects of object distance on size perception. The participants in the three experiments were presented with a monocular view, through a "peephole" containing an electronic shutter, of two square pieces of red Plexiglas standing erect on a surface covered with a highly textured cloth (Experiments 1 and 2) or on a dull gray textureless cloth (Experiment 3). The two red squares were never the same physical size and in most instances they were not at the same distance from the participant. The task was to judge which of the two squares was physically bigger and press an appropriate button. Response times were measured from the opening of the shutter till the correct response was made. The shutter was closed immediately after the response was made, and the stimuli were changed.

The idea behind this research paradigm was to try to determine if the constructivists are right in positing that the perception of size-at-a-distance entails a taking-into-account of object distance. Or, in contrast, whether the claim of the ecological approach is correct that perceived distance is irrelevant, with size information being picked up with the aid of invariant ratios available in the ambient array, such as texture occlusion or the horizon ratio. The analysis of the first experiment examined the response time data in terms of two stimulus parameters, the "distal ratios," the ratios of the objective (physical) sizes of the two red squares being judged, and the "proximal ratios," the ratios of the proximal (retinal) sizes of those squares. If distance is taken into account, as the constructivists or indirect theorists claim, then the response times should be affected by the proximal ratios, which are determined by the relative distance of the two squares from the observer. But if the ecological or direct theorists' claim that distance does not play a role in the perception of size is correct, then only the distal ratios should affect the response times. The results of the first experiment indicated that the proximal ratios affected response times to a greater extent than did the distal ratios. This finding is more in line with the predictions of the indirect theory. But there also was evidence of an independent effect of the distal ratios on response times, and the results of Experiments 2 and 3 further elucidated this effect. Briefly, the results of those two experiments showed that the effect of the proximal ratios on response times was contingent on the distal ratios: The smaller the distal ratio (i.e., the more different in size the two squares being compared), the smaller the effect of the proximal size. In other words, the greater the difference between the physical sizes of the squares being compared, the more direct the perception of their size. In fact, in the stimulus conditions with the greatest difference between the distal sizes (smallest distal ratios) the manipulation of distance had no effect on the response times at all. Thus, there appeared to be an indication that size-at-a-distance could be picked up without the involvement of perceived distance under certain conditions. I summarized the implications of these findings thusly:

> To sum up, it is being suggested that both direct and indirect perception occur, that they do not define a dichotomy but a continuum, and that the location of a perceptual act on that continuum is determined by some interaction of the difficulty of the perceptual discrimination required and the richness of the stimulus conditions. . . . The challenge facing the perceptual theorist is not to choose between the two theories, but to incorporate the two approaches into a common framework with the aim of delineating the conditions under which direct and indirect processes emerge (Norman 1983).

It is being suggested here that such a common framework does exist. It is based on the findings concerning the existence of two visual systems, each with its specific modes of functioning, each with its complementary contribution to the organism's ability to utilize the impinging sensory stimulation in coping and behaving in its environment.

The "richness of the stimulus conditions" in the previous quote refers to the fact that in spite of my using a highly textured and well-illuminated surface, the experimental setup was not really conducive to what ecologically oriented researchers would consider a "fair" assessment of perception. The participants were given a very brief monocular view of the stimulus array; a far cry from what Gibson would consider an adequate setup allowing true perception. Yet, in spite of these limitations, evidence for direct perception of size seemed to emerge. The possibility exists that had the experimental conditions allowed binocular rather than monocular vision, and much longer exposures, perhaps entailing movement by the participants, direct perception of size might have been found for other conditions as well.

At about the same time others also called for the amalgamation of the two theoretical approaches, the constructivist and the ecological. Haber (1985) reviewed 100 years of research on perception in a paper presented in 1979 at the APA convention to celebrate the centennial of experimental psychology (and printed very much later). In that paper he noted that while the Gibsonian approach excels in its analysis of the stimulation reaching the organism, it needs to be supplemented by an adequate theory of the underlying processes along Helmholtzian lines. His conclusion: "I feel that as soon as we create a truly *Gibholtzian* theory of space perception, this merger will produce a breakthrough in our understanding of space" (Haber 1985). In an early review of the contributions of developments in computer vision to perceptual theory, McArthur (1982) found that both bottom-up and top-down processing (not his terms, but commonly used today) are required for efficient computer vision models, leading him to write:

> We can identify hypotheses about the kinds of knowledge and uses of knowledge in perception that could be regarded as 'more or less' Gibsonian or constructivist. More generally, we might regard the extreme Gibsonian and constructivist views as *end points* on a continuum, or *space,* of possible theoretical positions concerning the role of knowledge in perception. (p. 305)

It should also be mentioned that Rock (1983) also foresaw the possibility of a unified theory. In his discussion of the various theories of perception he wrote: "Varieties of each of these are of course possible, and one might develop an overall theory that combines features of each" (p. 28).

Others have noted that one of the problems in trying to find a middle way between the two approaches is that they are very different in the conceptualizations they adopt. A means for ameliorating this problem was suggested and thoroughly analyzed by Hatfield (1988; 1990b). He proposed that a connectionist analysis of perception can serve as the bridge between the two approaches. Very briefly, he showed how a connectionist model can satisfy the claims of the constructivists that rule-like behavior underlies perception, but by being rule-instantiating without being rule-following the model can also satisfy the Gibsonian strictures against cognitive mediation. In other words, the connectionist network with its hidden units and the connection weights among them can respond (or resonate) as if it is making inferences without implementation of anything more than some changes in the weights in the model. These weight changes are the "representations" of the system. Hatfield suggested that representations of this sort are commensurate with Gibson's approach.

Another attempt at bridging the gap between the two theoretical views was offered by Bennett et al. (1989; 1991). They presented a mathematical theory of perception that they suggested can also serve as a rapprochement between the ecological and constructivist views (see also Banks & Krajicek 1991; Braunstein 1994). Their theory is built around the concept of an "observer." An observer is not necessarily a perceiver but "each perceptual capacity can be described as an observer" (Bennett et al. 1991). These observers perform inductive rather than deductive inferences and such inferences can serve in both what appear to be direct or ecological perceptual processes and in the type of processes proposed by the constructivists. By transforming the two approaches to inferences of a similar nature they suggested that the gap between the two can be bridged.

More recently, Neisser (1994) has proposed a tripartite division of perception, consisting of three perceptual systems:

> 1. *Direct perception/action,* which enables us to perceive and act effectively on the local environment.
> 2. *Interpersonal perception/reactivity,* which underlies our immediate social interactions with other human beings.
> 3. *Representation/recognition,* by which we identify and respond appropriately to familiar objects and situations.

While Neisser does not go into very much detail concerning the three systems, it would appear that the first and third systems above are very similar to the two systems being suggested here, the dorsal and the ventral, respectively. The second system, the one dealing with social interactions, although very interesting, is beyond the scope of the topics being dealt with here.

In a recent guest editorial in the journal *Perception,* Heller (1997), whose central interest is in the sense of touch, also calls attention to the fact that "An important gap in theoretical positions exists between the ecological and traditional points of view." He uses the term traditional as synonymous with the "constructionist (representational) viewpoints." He then writes: "It is very possible that the ecological position and the inferential hypothesis testing views of perception are both correct, within limits . . . Thus, the distinction between the 'what' and 'where' functions of perception may help to resolve the apparent conflict between the ecological and other, constructionist approaches." This statement also bears much similarity to the thesis set forth in this paper.

3. The two visual systems: The ventral and the dorsal

The idea of two visual systems is far from new (see reviews in Jeannerod 1997, Ch. 2; and Milner & Goodale 1995, Ch. 1). In the late sixties, a group of studies produced evidence for this idea. One of the better known studies was carried out by Schneider (1967; 1969) who described experiments on hamsters where ablation of the cortical visual system (areas 17 and 18) left the hamsters incapable of demonstrating pattern discrimination but still capable of orienting toward objects. In contrast, in a second group of hamsters, undercutting the tectum, thus disconnecting the superior colliculus, had the opposite effect. The latter group of animals could make pattern discriminations but could not orient themselves in space. Schneider (1969) saw these findings as indicating that the hamster had two visual systems, one a cortical system answering the question "What is it?" and the second a subcortical system answering the question "Where is it?" At about the same time, Trevarthen (1968) who had been studying the behavior of split-brain monkeys also came to the conclusion that there were two visual systems, one a subcortical system that he called "ambient" and one a cortical system that he called "focal." The former was primarily subserved by peripheral vision and the latter by foveal vision. Quite a few other studies during that period also pointed to the existence of two visual systems. For example, Ingle (1973) provided evidence for the existence of two visual systems in the frog. Held (1970) also published a review of a wide variety of studies of perceptual adaptation all consistent with the idea that there exist two modes of visual analysis, a "contour-specific" mode and a "locus-specific" mode.

This focal-ambient nomenclature was adopted by quite a few researchers including Leibowitz and Post (1982) who summarized implications of these two modes to several quite diverse topics in vision and visual perception. Among the studies, these authors summarized an earlier study of theirs (Leibowitz et al. 1978) where they examined the effect of inducing refractive error (blur) on both size constancy and shape constancy. They found that increasing blur decreased shape constancy, but, in contrast, increasing blur had no effect on the degree of size constancy. Leibowitz and Post (1982) suggested that these differences were due to the differences between the focal and ambient systems. The focal system is very sensitive to decreases in spatial frequency, while the ambient system functions efficiently over a large range of spatial frequencies. It is suggested that the focal-ambient distinction as used many years ago by Leibowitz, Held, and others is quite similar, if not identical, to the ventral-dorsal distinction to be elaborated here. The Leibowitz et al. (1978) study is an early finding indicating that the dorsal system is involved in the pickup of size information.

The general consensus during the 1960s and 1970s was that the focal system was under cortical control while the ambient system was subcortical (e.g., Perenin & Jeannerod 1979). But Ungerleider and Mishkin (1982) presented evidence that in the visual cortex of the monkey there were two separate pathways, one they labeled the ventral stream leading from the occipital cortex to the inferior temporal cortex, and the second, the dorsal stream leading to the posterior parietal cortex. Lesioning the inferior temporal cortex left the monkeys unable to discriminate between objects of different shapes, while lesioning the posterior parietal cortex left them unable to perform a landmark discrimination task. These findings led Ungerleider and Mishkin to suggest that the ventral pathway dealt with object identification, and the dorsal pathway dealt with object location. Somewhat like Schneider, they called the ventral pathway a "what" system and the dorsal pathway a "where" system, but unlike Schneider both systems were cortical.

More recently, a somewhat different interpretation of this dichotomy has been suggested by Goodale and Milner (1992; see also Milner & Goodale 1995). Their interpretation of the functions of the ventral stream does not differ markedly from that of Ungerleider and Mishkin. They also see it as mainly involved in the processes of recognition and identification. Their main innovation lies in the functions they attribute to the dorsal stream. Rather than mapping the location of objects, they see it as a system for the visual control and guidance of motor behavior. They present a great deal of evidence showing that the dorsal stream is capable of utilizing visual information for the control of movement, and that it is dissociated from the ventral stream. According to Goodale and Milner, the major difference between the two streams is not in the visual information they process, but in the transformations they perform on the available visual information. In other words, the ventral stream transforms visual information into an exocentric (also labeled "allocentric") framework allowing the perception of the object as it relates to the visual world. The dorsal system, on the other hand, transforms visual information into an egocentric framework allowing the actor to grasp or otherwise bodily manipulate the object.

The labels "dorsal system" and "ventral system" will be used here to denote the two systems.[3] At the end of this section I will try to summarize what is known about the functions of the two systems and the differences between them. This will follow a review of neurophysiological studies, referring mainly to physiological studies on monkeys, but also some of the recent imaging work (PET and fMRI) corroborating these findings in humans, a review of neuropsychological studies of brain-damaged patients, and finally a review of psychophysical studies on healthy humans.

3.1. Neurophysiological studies

The labels "dorsal system" and "ventral system" are used in this article to connote two theoretical entities, but these labels are borrowed from, and have their roots in, two anatomical-physiological entities usually labeled the dorsal and ventral streams. These streams are located in different parts of the cortex. The dorsal stream is located in the main in the posterior parietal cortex and adjacent areas, and includes areas such as MT (middle temporal or V5), MST (medial superior temporal), LIP (lateral intraparietal), among others. The ventral stream is located in the main in the inferotemporal cortex and adjacent areas, and also includes area V4. Both the ventral and dorsal streams receive input from V1, but the dorsal stream also receives direct subcortical inputs, via the superior colliculus and pulvinar. It is this subcortical pathway that was once thought to serve the ambient visual system. Of the two streams, the ventral appears to receive its major input from the parvocellular retinocortical pathway, although it also receives considerable magnocellular input, while the dorsal stream receives its main, if not total, input from the magnocellular retinocortical pathway (see Merigan & Maunsell 1993). The differences between the parvocellular and the magnocellular pathways are important for gaining a better initial understanding of the functions of these two visual systems. Recent textbooks on vision (e.g., Wandell 1995) give detailed information on the parvocellular and magnocellular pathways, and I shall only describe them briefly here.

The two pathways are seen as originating in the ganglion cells of the retina with the parvocellular pathway in the much smaller and more plentiful midget ganglion cells and the magnocellular pathway in the much larger parasol ganglion cells. (Evidence exists for a third type of ganglion cell, the w cells, feeding into a third pathway, the koniocellular pathway described by Casagrande 1994, but not enough is known about this pathway to include it here.) The two pathways are still segregated at the lateral geniculate nuclei, the axons from the parasol ganglion cells reaching the two magnocellular layers and those from the midget cells the four parvocellular layers. This segregation continues in V1 as well as in extrastriate visual areas, with the pathways seen as splitting into three (e.g., DeYoe & Van Essen 1988; Livingstone & Hubel 1988) or even four (Zeki 1993) different pathways. It has been suggested that these pathways serve different visual/perceptual functions, but more recent evidence has indicated that these proposals of clearly segregated pathways are inaccurate, both at a physiological level and a functional (visual perception) level (see e.g., Bullier & Nowak 1995; Schiller 1996). Today, the consensus seems to be that the major difference between the two pathways is in their relative spatial and temporal sensitivities, the parvocellular pathway capable of processing information at higher spatial frequencies and the magnocellular pathway at higher temporal frequencies. It is also claimed that the contrast sensitivity of the magnocellular system is greater at low spatial frequencies (see e.g., Schiller 1996). One further important point is the fact that the magnocellular pathway is the faster of the two, with response latencies about 20 msec shorter than the parvocellular pathway (see Bullier & Nowak 1995). The magnocellular pathway has also been seen to be highly implicated in the processing of motion information (Logothetis 1994).

The brunt of motion analysis is carried out in the dorsal system, mainly in areas MT and MST (Logothetis 1994). It has also been shown that in macaques, dorsal system inputs are from areas dealing with spatial or motion analysis and from peripheral representations of the retina, while those of the ventral system are from areas dealing with form and color analysis from more central representations of the retina (e.g., Baizer et al. 1991). But this simple view of the ventral system dealing with form and color perception and the dorsal system dealing with motion and spatial analysis is an oversimplification. For example, area V4 is considered to be part of the ventral system but also possesses cells that are motion sensitive (Ferrera et al. 1994; Logothetis 1994). On the other hand, there is evidence for the involvement of the dorsal system in some type of shape or form analysis. Features necessary for object identification, such as shape and size, are processed by the ventral system, but the dorsal system also has access to information about the shape and size of objects, albeit to serve a different purpose, that of performing motor movements vis-à-vis those objects, and utilizing a different framework, egocentric rather than allocentric. Sakata et al. (1997) have recently summarized a large group of studies indicating that the parietal cortex of monkeys contains at least five types of cells relevant to depth perception and the visual control of hand movements. Many of these cells were found to be sensitive to the 3D features of objects, such as shape, orientation, and size. There is also evidence from PET imaging studies that this is true in humans as well (Baker et al. 1996; Faillenot et al. 1997). The studies by Sakata and his colleagues also show that cells in the parietal cortex respond to binocular inputs, including sensitivity to binocular disparity.

Clearly the input into the two systems must combine at some point, and recent studies have also been focusing on what becomes of the information in the two systems and where it is integrated. Evidence for continued segregation of the two systems in the frontal lobe (frontal eye fields) has been reported (Bullier et al. 1996; Schall et al. 1995). Owen et al. (1996) report similar findings in a PET study of humans. More recently, Rao et al. (1997) reported a study of neurons in the monkey's prefrontal cortex, where both object-oriented and location-tuned tasks were used. Some of the neurons showed specific object- or location-tuning, but 52% of the cells showed tuning to both dimensions, leading these researchers to suggest that: "These neurons may contribute to the linking of object information with the spatial information needed to guide behavior."

To sum up, physiological research on monkeys and imaging studies on humans have produced evidence for the existence of two cortical visual systems, the ventral system that processes pattern, form, and color information, and the dorsal system that processes motion and spatial information. It would seem that recent neurophysiological findings concur with the neuropsychological and psychophysical findings reviewed below, in that both systems overlap somewhat in the type of visual input they process, but process this information for quite different purposes.

3.2. Neuropsychological studies

Many insights into the functions of the two visual systems and their dissociation have come from studies on patients where apparently one of the two systems is damaged due to some localized injury to the brain. Many of these studies have been thoroughly reviewed in Milner and Goodale's (1995) book, and I will only mention some highlights. On the one hand, there are patients who have incurred damage in their parietal lobe, and presumably some of their dorsal system functions are defective. Some of these patients suffer from what is called optic ataxia, manifesting great difficulties in making correct motor movements towards visually displayed targets, but have no trouble discriminating and identifying visual stimuli of all sorts. In a word, these patients presumably have an intact ventral system, but a damaged dorsal system. Patients suffering from optic ataxia have been described quite often in the literature (e.g., Perenin & Vighetto 1988).

In a recent study, Milner et al. (1999) presented evidence for the dissociation of the two systems in a visual localization task. They compared the pointing accuracy of a patient suffering from optic ataxia (who can be seen as having a deficient dorsal system) to that of three normal subjects. All were required to point at one of seven target positions under two conditions; no delay in pointing, and a 5 sec delay. The normal subjects, as might be expected, were better at pointing when there was no delay, but the optic ataxia patient's pointing errors were greater in the no delay condition than in the delay condition. The authors note that "the data are consistent with a dual processing theory whereby motor responses made directly to visual stimuli are guided by a dedicated system in the superior parietal and premotor cortices, while responses to remembered stimuli depend on perceptual processing and may thus crucially involve processing within the temporal neocortex." In other words, the optic ataxia patient lacking a functional dorsal system could make use of her ventral system, which comes into play after a few seconds.

Goodale, Milner, and their colleagues have carried out a large number of studies on a visual agnosic patient, DF, who suffered extreme carbon monoxide poisoning that apparently disconnected the V1 input into the inferotemporal cortex. In other words, this patient is apparently incapable of using her ventral system for analyzing visual input; that is, she is suffering from an extreme type of visual form agnosia. Not only can she not recognize faces and objects, but she is incapable of making much simpler discriminations such as between a triangle and a circle. She is capable of drawing objects fairly well from memory, but cannot copy pictures nor recognize the objects she has drawn. But DF appears to have an intact dorsal system, and is capable of carrying out visuomotor activities. Goodale et al. (1991) reported a study of orientation and size perception on patient DF. When asked to insert a card into a slot presented at varying angles, she had no trouble in orienting her hand to match the correct angle in spite of the fact that she was incapable of reporting in any manner what the orientation of the slot was. As for size perception, she was unable to tell if two small plaques were of the same or different widths, nor was she able to indicate the widths of the plaques by adjusting the distance between her index finger and thumb. Both these tasks were very simple for the two control subjects. But when DF was asked to pick up the plaques, the aperture between her fingers in preparation for picking up the plaques was highly correlated with the width of the plaques, similar to the control subjects. In other words, this subject who apparently has an intact dorsal system, but a completely dysfunctional ventral system is incapable of demonstrating perceptual cognizance of the size of the plaques, but when asked to pick them up demonstrates that size information is available to her.

Quite a few other studies of DF's visual and perceptual capacities have been carried out. She has been shown to possess color vision and can utilize this capacity to recognize natural objects (Humphrey et al. 1994). Utilizing this capacity, it was shown that she manifests the McCullough Effect, another indication that her visual system is capable of picking up orientation information (Humphrey et al. 1991; 1995). Two studies have demonstrated that she is incapable of utilizing Gestalt principles of organization of shape information. (Goodale et al. 1994; Milner et al. 1991). Carey et al. (1996) have shown that DF is capable of grasping tools and utensils quite proficiently but has difficulty in visually recognizing the correct part of the object to grab (e.g., handle). This study also showed that she is capable of responding concurrently to both size and orientation information.

Of special interest here are studies of DF's capacities to adapt to the contingencies of her spatial environment. She has been shown (Patla & Goodale 1996) to be able to negotiate obstacles during locomotion as well as control subjects do, even though when asked to estimate their height she does this much more poorly than control subjects do. She has also been shown not to differ from normal controls in the effects of the pitch of the visual field on her perceived eye level, but she could not report that pitch, an easy task for the control subjects (Servos et al. 1995). DF is highly proficient at grasping objects when she views them binocularly, but this ability is disrupted when she is allowed only monocular vision (Dijkerman et al. 1996; Marotta et al. 1997). When allowed to move her head during monocular viewing, yielding motion parallax, her grasping improves considerably (Dijkerman et al. 1999). It has also been shown that without binocular vision DF manifests serious disruptions in the size-constancy of grip aperture (Marotta et al. 1997). Marotta et al. suggest that this is due to the fact that she cannot use pictorial cues to assess the objects' distance, not allowing the further assessment of the object's size. In a related study (Humphrey et al. 1996), it was shown that DF could discriminate apparent 3D structure and orientation of shapes only on the basis of shading gradient cues and not when the edges were depicted as lines or as luminance discontinuities. A broader analysis of DF's abilities to pick up information about space will be undertaken in section 5.2.1.

3.3. Psychophysical studies

In the search for a dissociation between the ventral and dorsal systems in healthy subjects a fairly large number of psychophysical studies have compared judgmental responses to motor responses to the same stimuli. The judgmental responses can be seen as mainly based on ventral system function, the motor responses mainly on dorsal system function. Among the first to carry out such studies were Bridgeman and his colleagues. They utilized three methods to demonstrate this dissociation, studies of saccadic suppression, studies of induced movement, and studies of the Roelofs effect. Bridgeman et al. (1979) utilized the phenomenon of saccadic suppression to show that when targets are moved slightly during a saccade, these small displacements are not reportable by either verbal responses or button presses, whereas both eye-movements and pointing behavior are influenced by the change in location. Bridgeman et al. (1981) showed that the induced movement illusion affected verbal reports, but the pointing responses were veridical. In a related study, Wong and Mack (1981) used the induced movement illusion to cause the target to be reported as moving in the direction opposite to its actual movement. In contrast, the subjects' eye-movements followed the actual movement direction and not the illusory direction. When a delay was introduced, the eye-movements followed the illusory displacement, suggesting that memory of the movement was stored in the ventral system.

Smeets and Brenner (1995a) carried out a study that led them to propose that the findings of Bridgeman et al. (1981) were not the result of the dissociation of perception and action systems, but rather, arising from independent processing of velocity and position. In response, Bridgeman has recently demonstrated that similar dissociations between the two systems can occur with stationary stimuli utilizing a phenomenon known as the Roelofs effect (Bridgeman et al. 1997). This effect causes target position to be misperceived when it is surrounded by a frame presented asymmetrically. Targets tend to be misperceived in the direction opposite to the offset of the frame. When no delay was introduced between stimulus exposure and the cue to either make a judgment or point to where the target had been, all ten subjects evidenced the effect in their judgments, but five did not do so with the pointing response. Thus, at least for some of the subjects the surrounding frame did not affect the motor response. In the 4- or 8-sec delay conditions this dissociation was not found, all subjects showing the effect also with the pointing response. This, once again, suggests that the dorsal system has a very limited short-term memory. In a subsequent study, Bridgeman and Huemer (1998) used an auditory cue immediately prior to a motor response in a Roelofs effect setup. The auditory cue indicated which of two targets should be jabbed. In spite of the fact that the motor response was preceded by a cognitive analysis of the auditory cue, the motor response was not susceptible to the Roelofs effect, indicating that a prior cognitively processed cue can still prime the dorsal system response.

Several studies have compared verbal responses and motor responses in the perception of distance. Some of these have focused on short distances, where the motor responses have usually been reaching movements; others on somewhat longer distances where the motor responses have been pointing or walking (without vision). Gentillucci and Negrotti (1994) studied exocentric distance[4] perception using two response methods, a pointing response and a visual reproduction response. The stimuli were presented frontally and close to the subjects with the distances between them ranging between 5 and 17.5 cm. The two response modes yielded different patterns of constant errors, with those for the pointing responses decreasing with distance and those for the reproduction increasing. These findings led the authors to conclude that their findings "support the hypothesis that perception and visuo-motor transformations are two separate processes." In a second study, these researchers (Gentilucci & Negrotti 1996) required subjects not to reproduce the distance but to reproduce a double distance. Here the results were similar for both response modes, indicating that the doubling instruction involved the ventral system for both response modes. Related findings have been reported by Pagano and Bingham (1998) who studied the monocular perception of egocentric distance given by optic flow generated by head movements towards a target. Two response measures were used to assess the perception of distance, verbal reports and reaches. It was found that verbal and reaching errors were uncorrelated, leading, once again, to the suggestion that this was due to the independent functioning of the two systems.

Other studies have looked at distance perception for distances beyond arm's reach. Some of these studies have used judgmental estimates of distance, usually egocentric distance, while others have used motor responses to distance such as blindfolded walking or pointing. The results of the studies using judgmental estimates have yielded inconsistent results, in some cases yielding quite veridical estimates, but in other cases yielding quite systematic underestimates (see review in Bingham & Pagano 1998). In contrast, the studies using motor responses have yielded veridical distance perceptual responses. Among these are studies by Loomis and his colleagues (see review in Loomis et al. 1996) who compared blind walking to distance estimates of distances up to 12 m. For example, in one experiment (Loomis et al. 1992, Exp. 2) frontal exocentric distances and sagittal depth-interval distances were shown to be judged quite differently, with the sagittal distances set to 50% to 90% more than the frontoparallel estimates to appear equal. In contrast, blind walking to the endpoints of the two types of intervals yielded equal responses. Loomis et al. (1996) ascribe these differences to a dissociation between egocentric (sagittal) and exocentric (frontal) distance perception. This claim can be interpreted in terms of the two visual systems, where the dorsal system deals with egocentric measures and the ventral system with exocentric (or relative) measures. Thus, the estimates differ because the dorsal system is less involved in the frontal estimates than in the depth-intervals, while the walking responses rely in both cases mainly on the dorsal system.

Dissociations in the perception of size have also been examined in many recent studies comparing motor and judgmental responses to stimuli presented within the context of well-know visual size illusions. These studies have yielded conflicting results, possibly related to the lack of an adequate understanding of the processes underlying these illusions. In an early, much cited, study Aglioti et al. (1995) utilized the Ebbinghaus (or Titchener) illusion, where the reported size of a central circle is influenced by the circle-size of a group of circles surrounding it. In their study, these researchers replaced the drawing of the inner circle with a thin poker-chip like token. When asked to judge the size of the target tokens the subjects manifested the illusion throughout the experiment, but when asked to manually pick up the central target token, manual grip size during the grasping movement was much less influenced by the illusion. This was seen to indicate that the ventral system is influenced by the illusion and the dorsal system is not.

Haffenden and Goodale (1998) replicated the findings of the Aglioti et al. (1995) study adding further control conditions, such as not letting the subject view her hand as it moved (open-loop conditions) and having the subjects indicate the judged size with a manual response of the distance between the thumb and forefinger. Marotta et al. (1998a) also replicated the findings of no or little illusion with a motor response in a study that compared binocular and monocular presentations of the illusion (see sect. 5.2.1). In another recent study (Westwood et al. 2000) that compared pantomimed and natural actions, these findings were also replicated. A study by Franz et al. (2000) has not replicated these findings with very similar effects of the illusion on both perceptual judgments and grip apertures. These researchers point out that in the previous studies the perceptual judgments were carried out by comparing two circles, one surrounded by small circles, the other by large circles. In contrast, the manual responses were made towards only one of the circles. They show that when the illusion's perceptual effects are studied with single-circle presentations there are no differences between the two types of responses. Pavani et al. (1999) have also reported similar results. Haffenden and Goodale (2000) have recently suggested that the discrepancy between the results of these two studies and theirs are due to the size of the gaps between the central and surrounding circles used in the latter two studies. To add to the current confusion, van Donkelaar (1999) has shown that a different motor response, a pointing response, is affected by the Ebbinghaus illusion.

Judgmental and motor responses have also been compared with other visual size illusions. Post and Welch (1996) utilized an open loop reaching task with the Müller-Lyer and two other illusions. In the case of the Müller-Lyer illusion they did indeed find that the illusion did not affect the reaching responses but did affect the judgments. But in two additional experiments using other illusions they demonstrated that these results need not be explained in terms of a dissociation between the two systems, and can be seen to depend on the subjects' egocentric localization. In a study that looked only at motor responses, Gentilucci et al. (1996) studied pointing responses to a vertex of the Müller-Lyer figure. There were four conditions: full vision of the stimulus and the pointing hand, vision of the stimulus but not of the hand, no vision of either (0 sec delay), and no vision of either with a 5 sec delay before pointing. The illusion had an effect in all conditions, but it was relatively small in the full vision condition, and increased in size over the other four conditions. In other words, the more the pointing was based on memory, the greater the effect of the illusion. In terms of the two visual systems these results indicate a growing reliance on the ventral system as memory became more and more involved. In a subsequent study (Daprati & Gentilucci 1997), the motor reaching task was supplemented by two tasks of length reproduction. Grip aperture for the length of a Müller-Lyer shaft was influenced by the illusion but this effect was smaller than that found with the two reproduction tasks.

Brenner and Smeets (1996) utilized a converging line variant of the Ponzo illusion to examine its effects on grasping responses. Disks were placed on the background that yields the illusion and subjects were asked to lift them. These researchers also found that grip aperture was not influenced by the illusory size, but they did show that the illusion did influence the force used to lift the disks. More force was applied to the perceptually larger disks. Similar results have been reported by Jackson and Shaw (2000). In a recent study, Ellis et al. (1999) compared verbal estimates and grasping responses for the center of a steel bar placed on two illusory backgrounds: the same variant of the Ponzo illusion as used by Brenner and Smeets, and for the Judd illusion (a variant on the Müller-Lyer illusion, where both arrows point in the same direction). They found that the two illusions affected both types of responses but the errors in the grasping responses were significantly smaller than in the verbal estimates. They see these results as indicative of a partial dissociation between the two systems. But Mon-Williams and Bull (2000) have recently reported a study that appears to show that the Judd illusion results "may be due to occlusion of the illusory background during the transport phase of the movement."

Servos et al. (2000) have reported similar results to those of Aglioti et al. (1995) for another size illusion, the horizontal-vertical illusion. In this illusion two equal-length lines are presented as an inverted "T" (⊥), but the vertical line is perceived to be considerably longer. The illusion affected subjects' judgments but did not affect their grip aperture. Vishton et al. (1999) also studied the horizontal-vertical illusion in a set of four experiments. While the results of their first experiment are similar to those of Servos et al. (2000), the second and third experiments showed that when subjects directed their perceptual judgments to only a single element (line) in the display, their judgments were as accurate as in the motor response. Their fourth experiment further showed that when the grip response requires taking both elements into account it is as susceptible to the illusion as the judgmental response. These findings led the authors to suggest that the differences found in studies of this type are "best described as a dissociation between relative and absolute size perception, rather than a dissociation between perception and action." Recalling that dorsal system responses to visual size are normally based on absolute size, while ventral system responses are normally based on relative size, these findings are consistent with the general claim of differential processing by the two systems, but also show that both systems can mimic the functions of the other when this is called for.

The studies of distance perception reviewed above appear to strengthen the hypothesis of the dissociation of the two visual systems, but the results of the studies on size perception are somewhat equivocal and difficult to interpret. Perhaps the reason for this difference is the fact that in the studies of distance perception the subjects were requested to carry out more natural and more ecologically valid tasks than those in the studies of size perception, all of which utilized size illusions. When faced with a novel task utilizing a visual illusion, the ventral system might at times override the functions of the dorsal system. Perhaps a better way to study the dissociation between the two systems in the perception of size would be to use techniques like those of Warren and Whang (1987) described below (see sect. 4.1).

Two studies have extended the range of the applicability of the two systems notion. These studies appear to indicate that the dissociation can also be meaningful for much more distant stimuli than those used in the laboratory studies reviewed above. Proffitt et al. (1995) had subjects judge the inclination or steepness of hills, both out of doors and in a simulated virtual environment. The angle judgments were obtained with three response measures, verbal estimates, adjustments of a representation of the hill's cross-section, and haptic adjustments of a tilt board with an unseen hand. The first two measures yielded large overestimations of hill incline, while the latter judgments were close to the veridical. They "propose that the radically different pitch estimates obtained with verbal and visual reports versus haptic adjustments are due both to the dissociation in the visual pathways that inform these two sorts of responses and to the calibration mechanisms that coordinate their functioning." In a subsequent study, Creem and Proffitt (1998) examined the effects of delays between viewing the hills and responding both verbally and haptically. With short delays the haptic responses remained veridical as in the previous study, but with longer delays they were seen to be influenced by the ventral system. It should be noted that the short delays in this study were considerably longer than those used in the previous studies (see above), reaching two minutes in comparison with only a few seconds in the earlier studies. The authors suggest that the length of the dorsal system memory might relate in some way to the amount of time necessary to carry out the motor task in question.

Recent studies have presented evidence for the dissociation of the two systems in other domains. Neurophysiological findings indicate that the ventral system receives its main input from the central portions of the retina while the dorsal system is attuned to the entire retina, leading Goodale and Murphy (1997) to test the hypothesis that judgmental responses would be more affected by retinal eccentricity than motor responses. They asked subjects to carry out two tasks, a grasping task and a categorization task, using blocks of different sizes at 5° to 70° in the periphery. They found that in the grasping task the correlation between maximum aperture and block size is maintained in the far periphery, although the amplitude of the grasp increases with eccentricity. In contrast, the categorization judgments decreased with eccentricity. More important, the variability of the grasp size did not increase with eccentricity as it did with the categorization judgments. Goodale and Murphy see these results as indicating that dorsal system motor responses to peripheral inputs are much more reliable than perceptual judgments of peripheral stimuli.

Dijkerman and Milner (1998) recently examined subjects' ability to discriminate the orientation of a square plaque tilted in depth, using two modes of response, grasping and perceptual matching. While both response modes yielded high correlations between tilt and the response extent, there were differences between the matching and grasping data. The grasping data yielded a linear function, while the matching data showed a consistent curvature. The authors ascribe these differences to the operation of the two different systems in the analysis of orientation in depth. The dorsal system requires information about the absolute stimulus properties leading to the linear function, while the ventral system can do with more categorical information for processing the relative orientation, yielding the curved function that somewhat de-emphasizes the differences between the orientations close to either the horizontal or the vertical. This study also compared monocular and binocular viewing, but no differences were found. Other studies of a similar nature have found differences between binocular and monocular viewing (see sect. 5.2.1).

Finally, if it is true that the two systems function independently and that the dorsal system functions can be carried out with little or no conscious awareness, it is possible that the two systems will be capable of simultaneously processing two different sources of visual information with very little interference. Does any evidence exist for the possibility that subjects can carry out two tasks simultaneously, one dorsal in nature and one ventral, without interference between them? Ideally, such an experiment would consist of requiring subjects to identify a visual stimulus presented foveally and at the same time give a motor response to a visual stimulus presented to the visual periphery. Little research of this exact nature has been carried out. While there is a very extensive literature on "dual-task" performance, it invariably emphasizes the allocation of attention between two tasks of a ventral nature.

Among the very few relevant studies is a study by Castiello et al. (1991). These researchers compared the timing of responses to the sudden displacement of a visual object, comparing a grasping response to that of a simple vocal utterance (Tah!). They found the mean vocal response latencies to be 420 msec, more than 300 msec after the motor response. What is relevant to the question at hand is the comparison of the results of the testing of both responses simultaneously and the control experiments where each response was examined separately. The results showed that the latencies of both types of response when executed alone were not any faster than those executed simultaneously. In other words, the dorsal grasping response and the ventral vocal response did not interfere with each other. But this study focused on response times of the two systems to a sudden and singular change in stimulation. It did not really deal with the question as to whether two continuous tasks, one dorsal and the second ventral, can be undertaken simultaneously without detriment to the performance of each.

Deubel et al. (1998) examined this question in a study aimed at examining the sharing of attention between a ventral and a dorsal task. They utilized a dual task paradigm where the primary task was a reaching response for a designated location and the secondary task called for the discrimination between "E" and "$." The reaching response was seen by the authors to be carried out by the dorsal system and the discrimination response by the ventral system. The results indicated superior performance when the discrimination task appeared at the same location as the aim point of the reaching response. These results were interpreted as arguing "for an obligatory coupling of (ventral) selection-for-perception and (dorsal) selection-for-action." While this study would appear to yield a negative answer to the possibility of independent functioning of the two systems, it might be argued that a different interpretation is possible. The aim point for each reach response was changed between trials. The subjects were informed where to reach by a pointing triangle that specified the side to reach while its color specified the exact location. According to the depiction of the two systems presented here, identifying the direction of the arrow and its color are both ventral system responses. Thus, it can be argued that the dorsal response used in their study also activated the ventral system. But it should also be pointed out that Bridgeman and Huemer (1998) (see above) showed that dorsal motor responses can follow from decisions based on ventral activity.

Ho (1998) recently reported a study that appears to indicate that ventral and dorsal system tasks can be undertaken simultaneously without detriment to the performance of each. The two tasks were a motion processing task and a RSVP (rapid serial visual presentation) letter-recognition task. The motion-processing task was presented in an annulus that surrounded the area where the RSVP task was presented. The motion stimulus was ambiguous, in that it could be interpreted by the subjects as either rotating clockwise or counterclockwise, depending on whether they employed a second-order (texture-defined) motion algorithm or a third-order (pattern-tracking) motion algorithm. The participants split into two groups, depending on their natural tendencies to see either second- or third-order motion. Briefly, the findings showed no interference between second-order motion perception and the letter-recognition task, but interference was found between third-order motion perception and letter recognition. Second-order motion tasks are thought to be processed by the dorsal system (e.g., O'Keefe & Movshon 1998), and these were shown not to interfere with the ventral letter-recognition task. Ho suggests that third-order motion processing requires ventral processing, but there does not seem to be any study corroborating this.

3.4. Contrasting the two systems

To summarize the discussion of the two visual systems, let me briefly list some of the differences between them:

3.4.1. Function. While both systems analyze the visual input, this analysis is carried out for different purposes. The primary function of the ventral system is the *recognition* and *identification* of the visual input. Recognition and identification must depend on some comparison with some stored representation. In contrast, the primary function of the dorsal system is analysis of the visual input in order to allow *visually guided behavior* vis-à-vis the environment and objects in it (e.g., pointing, reaching, grasping, walking toward or through, climbing, etc.). While these are the primary functions of the two systems, it would seem that they also participate in other functions. Thus, for example, the dorsal system would seem to be involved in the identification of moving objects, while the ventral system has capacities that parallel those of the dorsal system, such as size perception, albeit a somewhat different type of size perception.

3.4.2. Sensitivity. The two visual systems differ with respect to their sensitivities in the spatial and the temporal domains. The ventral system is more sensitive to high spatial frequencies while the dorsal system is sensitive to high temporal frequencies. In other words, the ventral system is superior at seeing fine details, while the dorsal system is better at seeing motion. Comparing the two systems with respect to contrast sensitivity we find that the dorsal system has the higher contrast sensitivity, that is, it responds to very low contrasts at relatively coarse spatial frequencies. Some qualifications here as well: there is evidence that certain complex motions are processed by the ventral system (e.g., Ferrera et al. 1994). It is also clear that the dorsal system responds to static shapes, albeit in less detail; witness the ability of DF to shape her grasp to fit the shape of an object before touching it.

3.4.3. Memory. The ventral system is the memory-based system, utilizing stored representations to recognize and identify objects and events. In contrast, the dorsal system appears not to have a long-term storage of information, but only very short-term storage allowing the execution of the motor behavior in question. Presumably the duration of this short-term memory varies with the motor behavior in question, being shorter for reaching and grasping movements than, say, walking through some aperture such as a door.[5]

3.4.4. Speed. Of the two visual systems, the dorsal system is the faster. This statement is based on the fact that the dorsal system receives magnocellular input while the ventral system receives a great deal of parvocellular input as well as magnocellular input. The magnocellular system has been shown to respond faster than the ventral system. Psychophysical studies have also shown this to be the case, where motor responses to sudden visual changes were found to be considerably faster than verbal responses to those same changes. It should be noted, however, that there are perceptual activities that clearly include a ventral component, such as reading, that appear to be carried out with extreme speed.

3.4.5. Consciousness. In our normal everyday functioning, it is probably fair to say that we are much more conscious of ventral system processes than of those of the dorsal system. Evidence for this comes from all the psychophysical studies of the dissociation of the two systems reported above, where subjects report awareness of the ventral processing, but simultaneously manifest different dorsal processing. What is more, the patient DF described above is capable of carrying out visuomotor tasks with the aid of her dorsal system, but is unaware of the features of the stimuli that made the carrying out of those tasks possible (see Milner 1995; 1997). But there also exist examples of apparent awareness of dorsal system functions and of the opposite, unconscious ventral functions (see sect. 5.1).

3.4.6. Frame of reference and metrics. Both visual systems process information about objects in our environment, but for different purposes. Ventral system functions aim at recognizing and identifying the object; for this purpose all that is needed is object-centered information. In other words, the ventral system utilizes an allocentric frame of reference. In contrast, the dorsal system must perform some action on, or in relation to, the object, such as grasping it. For this purpose it needs to know the dimensions of the object in body-centered terms; for example, how large the gap between the thumb and forefinger should be in order to pick up that block. Thus, the dorsal system must utilize an egocentric frame of reference. In order to be able to pick up the object the dorsal system must utilize absolute metrics, whereas functions of the ventral system only require relative metrics.

3.4.7. Visual input. Two aspects relating to sensitivity to visual inputs differentiate the two systems. The ventral system is mainly attuned to foveal or parafoveal visual input. Its sensitivity falls off sharply with retinal eccentricity. In contrast, the dorsal system (with its magnocellular inputs) is much less affected by retinal eccentricity. The two visual systems also appear to differ in their ability to cope with a transition from normal binocular vision to monocular vision. While dorsal system function suffers considerably when forced to rely solely on monocular vision (without concomitant motion parallax), the ventral system is much less affected (see sect. 5.2.1).

3.4.8. Similarities and synergistic interactions. All of the points above indicate differences between the two systems, but it should be mentioned that the two systems also appear to perform many ostensibly similar functions, albeit for quite different purposes and using quite different mechanisms. Thus, for example, both systems deal with object shapes, sizes, and distances. A more detailed look at the parallel processing of size information will appear below. Here it should also be noted that in normal, nonbrain-damaged people the two systems obviously function synergistically. Thus, when one picks up a hammer, the control and monitoring of the actual movements is by the dorsal system, but there also occurs the intervention of the ventral system that recognizes the hammer as such and directs the movement towards picking up the hammer by the handle and not by the head.

4. Making connections

Having reviewed some of what is known about the two visual systems, I now return to the two theoretical approaches and once again look at the parallels between the ecological approach and the dorsal system, and between the constructivist approach and the ventral system. In addition, I will present a few examples of the research carried out under the aegis of each approach, in an attempt to show how the methodology employed is commensurate with the functions of the system in question.

4.1. Ecological theory and research and its relation to the dorsal system

Towards the end of his *Ecological Approach* Gibson (1979) proposes "a redefinition of perception":

> Perceiving is an achievement of the individual, not an appearance in the theater of his consciousness. It is a keeping-in-touch with the world, an experiencing of things rather than a having of experiences. It involves awareness-of instead of just awareness. It may be awareness of something in the environment or something in the observer or both at once, but there is no content of awareness independent of that of which one is aware. (1979, p. 239)

In this redefinition we discern Gibson's conception of the perceiver as active. Perception is an achievement, a keeping-in-touch, not a passive experiencing of one's conscious responses to stimulation. This view contrasts with the constructivist perspective of a perceiver who passively examines his conscious awareness of the stimulation impinging on his senses. This view of perception as resulting from an active perceiver is, of course, consonant with what we know about dorsal system functions. It is the system that picks up information for or through action. The notion of an active perceiver will be dealt with again in what follows, but first an examination of Gibson's claims concerning "awareness."

Gibson makes a distinction between the "content of awareness" and "awareness-of," and I would suggest that the former might be equated with what is usually called "consciousness" and the latter refers to the pickup of information about our environment. This dissociation between the usages of awareness and consciousness becomes clearer as one reads on. Gibson is more specific in his "Summary of the theory of pickup" when he writes: "The term *awareness* is used to imply a direct pickup of information, not necessarily to imply consciousness" (1979, p. 250). When discussing what are clearly cognitive processes such as conveying information through speech and language, Gibson writes:

> Knowledge that has been put into words can be said to be explicit instead of tacit. The human observer can verbalize his

> awareness, and the result is to make it communicable. But my hypothesis is that there has to be an awareness of the world before it can be put into words. You have to see it before you can say it. Perceiving precedes predicating. (1979, p. 260)

Recalling that "awareness of" in the above passage need not imply consciousness, it appears that Gibson is implying that perception, or to remain consistent with my usage, pickup of information precedes conscious awareness. This interpretation is bolstered by a sentence from the passage on size perception quoted above (see sect. 2.2): "The implication of this result, I now believe, is that certain invariant ratios were picked up unawares by the observers and the size of the retinal image went unnoticed" (Gibson 1979, p. 160).

The implication, then, is that Gibsonian pickup of information involves little or no consciousness. This is consistent with the understanding of the functioning of the dorsal system where conscious awareness plays a very minor role or none at all. But how does this claim of lack of conscious awareness jibe with our phenomenal experience of clearly being conscious of all aspects of our environment, including, say, the size of objects in it? There are two somewhat speculative answers to this question. One is that the ventral system has the ability to monitor the dorsal system by bringing into conscious awareness the relevant information picked up. This, it is suggested, normally only occurs when that information is insufficient for the execution of some action, or when there is some sort of conflicting information in the stimulus situation. The other is that the ventral system has its own parallel mechanism for perceiving the environment. Thus, in the case of size perception while the dorsal system would be engaged in picking up size information in body-scaled terms enabling motor interaction with the object in question, the ventral system would be engaged in perceiving size in relative, object-centered terms enabling better recognition of that object and its comparison with other objects. Of course, a very viable possibility is that both these occur together.

In the initial brief review of the ecological approach (see sect. 2.2), the concept of affordances was introduced. This concept can also be seen to tie in with the idea of dorsal system processing. As was noted above, Gibson's examples of affordances include "climb-on-able," "sit-on-able," and others. All these require some action by the observer, climbing, sitting, and so on. Gibson notes that "the affordances of things for an observer are specified in the stimulus information. They *seem* to be perceived directly because they *are* perceived directly" (1979, p. 140). Following from the previous discussion, it is then suggested that affordances are picked up with little or no conscious awareness. This idea also ties in with what we know about the dorsal system. In the review of the neuropsychological evidence for the dissociation of the two visual systems above (see sect. 3.2), I discussed the studies by Goodale, Milner, and their colleagues on patient DF. This patient was shown to be able to perform visuomotor tasks without being able to report anything about the stimuli that she manipulated or reacted to. It was suggested that this patient's ventral system was disconnected and she relied totally on her intact dorsal system. Much of the initial research on this patient focused on her ability to grasp objects, and Gibson also touches upon the affordance of graspability:

> To be graspable, an object must have opposite surfaces separated by a distance less than the span of the hand. A five-inch cube can be grasped, but a ten-inch cube cannot (Gibson 1966b, p. 119). A large object needs a "handle" to afford grasping. Note that the size of an object that constitutes a graspable size is specified in the optic array. If this is true, it is *not* true that the tactual sensation of size has to become associated with the visual sensation of size in order for the affordance to be perceived. (1979, p. 133)

The last sentence is, of course, a gibe at Berkeleyan empiricism, one of the forerunners of Helmholtzian constructivism. More to the point is the fact that Gibson's description of the affordance of grasping is consistent with the findings concerning patient DF, who is capable of picking up the size, shape, or orientation information concerning an object without conscious awareness, and utilizing that information to act upon the object.

To sum up, the concept of affordances serves to tie together the connection between the visual information in the ambient array and the actions taken by the observer with respect to the objects in that array. This tie between perception and action fits in nicely with what we know about the functions of the dorsal system, a system that picks up information relevant for actions. Gibson reiterates the connection between perception (information pickup) and action many times in his book. For example, when comparing knowledge and perception he writes:

> The direct perception of a distance is in terms of whether one can jump it. The direct perception of a mass is in terms of whether one can lift it. Indirect knowledge of the metric dimensions of the world is a far extreme from direct perception of the affordance dimensions of the environment. Nevertheless they are both cut from the same cloth. (1979, p. 260)

Thus, Gibson is saying that the direct perception of the affordances of objects enables the organism to act appropriately with regard to those objects, and that this occurs without any mediational mechanisms such as recognition of the object. Some of Gibson's writings on this topic have been criticized as indicating that objects are recognized directly. Recognition without recourse to representations in memory is indeed hard to fathom. While Gibson was very explicit in stating that "To perceive an affordance is not to classify an object" (1979, p. 134), some of his statements are indeed problematic. Examples are, his writing that apples afford eating or postboxes afford letter mailing (1979, p. 139). What is more, in the beginning of his chapter on affordances he writes: "This is a radical hypothesis, for it implies that the 'values' and 'meaning' of things in the environment can be directly perceived" (1979, p. 127). In terms of the dual-process approach discussed in this article, it is suggested that only what Neisser (1989) labeled "physical affordances" (see Palmer 1999, p. 411) are perceived directly. These are only the functional properties of objects and not their "meanings." In other words, when one directly picks up the affordance of a chair, one does not directly recognize it as a type of furniture labeled "chair," but rather one directly picks up the information that that object contains a surface on which one can sit. In a similar manner, it is suggested that rather than saying that the postbox affords letter-mailing, it would be better to say that the slot in the mailbox affords inserting an object of appropriate size and shape.[6]

In section. 2.2, I pointed out that the ecological approach, in contrast to the constructivist approach, does not deal in any depth with the processes underlying perception. This is not simply an omission on Gibson's part. It stems on the one hand from Gibson's dissatisfaction with the mentalistic mediational processes invoked by the constructivists, but more importantly, to my mind, from his very different conceptualization of the underlying processes of perception. The only allusion Gibson (1979) makes to something resembling underlying processes is when he talks about resonance or attunement (see sect. 2.2). But resonance is not really a "process" in the sense of a taking-into-account constructivist process. A body or system resonates to some impinging energy due to its internal structure, it does not process that energy in any way. What is more, resonance does not depend on memory other than the built in features that resonate to something.

It is in this sense that Gibson prefers to talk of a perceptual system that functions without recourse to memory. It is not a cognitive mechanism that is called up when a familiar stimulus occurs. Presumably the Gibsonian perceptual system picks up invariants in the ambient array by resonating to the features of that array. No "cognitive" memory mechanisms, such as, say, schemata need be invoked. How does such a conception match what we know about the functions of the dorsal system? First of all, it is claimed that the dorsal system has no representational memory to speak of, certainly nothing more than a few seconds or minutes to allow some action to be performed. Thus, the lack of memory posited by the ecological approach matches what is known about the dorsal system. What of the concept of resonance? Is there any way in which the functioning of the dorsal system can be said to be resonating to the visual input reaching it? An attempt at an initial answer to this question will be made below (see sect. 5.2.1).

It is enlightening to compare the research methods used by those adhering to the ecological approach to those adhering to the constructivist approach. To this purpose, I will briefly describe some studies of visual size perception in this and in the following section, each carried out in the "tradition" of each of the two approaches. The emphasis will not be on the results of these studies but more on the methods, aiming to show that the methods chosen are appropriate for the study of the visual system of relevance to each approach.

Relatively little research on *visual* perception of size has been carried out by ecologically oriented researchers as they have preferred to focus on the *haptic* perception of size. It is probably not fortuitous that this group has chosen to study haptic perception (see e.g., Turvey 1996), as the sense of touch requires a great deal of motor behavior that is controlled by the dorsal system. What is more, the haptic system is much less representational in its nature than vision. For example, in Barac-Cikoja and Turvey's (1991; 1993; 1995) studies of haptic perception of size subjects were required to assess the size of gaps between two blocks by wielding unseen rods. This is not a "judgmental" response about size, but a motoric manipulative response where the subject adjusts the gap between two visually presented blocks to be equivalent to the felt size of the gap. In other words, an attempt is made to limit the involvement of judgmental or ventral mechanisms. These researchers succeeded in arriving at an equation that depicts the very systematic relations between haptic perception of size and the physical parameters of stimulation. Importantly, that equation only contains physical measures of the rod wielding without "mentalistic" conceptualization such as "taking distance into account" (see Barac-Cikoja & Turvey 1995).

One study by this group did investigate the *visual* perception of size. Garrett et al. (1996) sought parallels between visual and haptic perception of size. Could a similar equation to that found for the haptic perception of size be found for vision? The method used to study visual size perception was based on the method used to study haptic perception. Pairs of blocks were placed at one of three distances from the observer. The gaps between the blocks were adjusted to one of three gap sizes and the subject had to match the seen gap with a manual motor response of adjusting the gap between a pair of blocks to the observer's left. The subjects were allowed to look back and forth between the far and near displays. The experimental method in this study differs from the constructivist size perception experiments to be described in the next section. Subjects were given a binocular view and allowed to move their heads, and no time limitations were imposed. What is more, they responded with a motor response rather than a judgmental response. All these conditions, it is suggested, are conducive to inducing dorsal system function in preparing the response to the gap size.

Quite a few studies have attempted to test and validate Gibson's concept of affordance. These have been carried out, of course, in the Gibsonian tradition and deal with such topics as the affordances of stair-climbing, sitting, or ball-catching, among others. One especially interesting study (Warren & Whang 1987) focused on the affordance of apertures for walking-through. The first experiment in this study, in my estimation, is the most direct examination of pickup of size information by the dorsal system. The size information picked up was the width of an aperture the subjects had to walk through. The subjects were asked to walk through apertures of differing widths and the extent of their shoulder rotation was measured. As might be expected, these authors found that the smaller the aperture, the greater the shoulder rotation. In order to better understand this relation they chose two groups of subjects, one large (taller and broader shoulders) and one small. When the relation between aperture width and rotation was plotted for each of these two groups separately, it was found that the two groups yielded parallel but distinct functions, with the large group rotating their shoulders to a greater extent for each aperture size. But when rotation angle was plotted not as a function of aperture width but of the ratio of aperture width to shoulder width, the functions overlapped. This was seen by the authors as evidence that aperture width is picked up in body-scaled terms.

Why is this a direct examination of pickup of size information by the dorsal system? Because the subjects were required to *act* vis-à-vis a given stimulus situation, that is, a given width of the aperture. They were not required to make any perceptual judgments that would have involved the ventral system in the task. The task occurred over time and the subjects were not limited in any way in time or space in performing the task, with the exception that there was a fast walking condition. In contrast, in the second and third experiments in this study, subjects made passability judgments about the aperture without actually walking through the apertures. The second experiment compared such judgments in two conditions: one, static, with a reduction screen, the other, moving, allowing head movements. Warren and Whang (1987) point out that "the results of the two studies do not offer striking convergence between the two tasks [walking and judging]," with the subjects in the second study judging narrower apertures as passable. This, I would suggest, is due to the fact that in the first experiment, dorsal system pickup of size information was mainly involved, while the second experiment entailed a combined effort of both systems, with the ventral system playing the major role. The reason for the latter claim is that no significant differences were found between the static and moving conditions; and, as I will try to show, the dorsal system relies quite heavily on movement (see sect. 5.2.1).

Warren and Whang's third experiment also utilized passability judgments. Its purpose was to compare a condition with a normal flat floor, with one in which the floor was raised a bit. The latter condition biased the pickup of eye-level plane information and yielded the expected overestimation of aperture width. The subjects were also asked to give distance estimates, and raising the floor did not bias these. The authors suggest that "this casts doubt on the explanation that the shift in passability judgments is due to a shift in the perceived absolute distance of the aperture." While the latter interpretation is a clear possibility, these results can also be interpreted in terms of the distinction between perceived and registered distance (see sect. 5.2.3). In a word, there is a difference between the reported perceived distance (ventral) and that picked up by the dorsal system.

4.2. Constructivist theory and research and its relation to the ventral system

In contrast to the theoretical concepts and experimental methods of the ecological approach outlined in the previous section, those of the constructivists parallel what we know about the functions of the ventral system. In this section I will point to some of these parallels. For example, at the beginning of his book *The Logic of Perception,* Rock (1983) discusses perceptual theories and says:

> A summary statement of the kind of theory I propose to advance in the remainder of the book. My view follows Helmholtz's (1867) that perceptual processing is guided by the effort or search to interpret the proximal stimulus, i.e., the stimulus impinging on the sense organ, in terms of what object or event in the world it represents, what others have referred to as the "effort after meaning" (p. 16).

In other words, Rock is conceiving of perception as an effortful, but unconscious, attempt at identifying an object or event. As was pointed out above, it is the ventral system that has the capacity to identify objects and events. Identification must be based on some information stored in some representational system. Once again, it is only the ventral system that has a representational memory; the dorsal system has been shown to lack more than a very brief memory needed to carry out some given action.

In his characterization of theories of perception, Rock (1983) suggests three types of theory. One he labels "stimulus theory," which is akin to some of Gibson's earlier thinking. The other two are versions of "constructive theories." Rock labels one of these as "spontaneous interaction theory" where "the determinant of perception is not the stimulus but spontaneous interactions between the representations of several stimuli or interaction between the stimulus and more central representations" (p. 31). Rock sees the Gestalt theory of perception (Koffka 1935) as fitting this rubric. Another example along the lines of this theoretical approach is Wallach's (1948) attempt to explain lightness constancy in terms of stimulus ratios. As Rock notes "there is a great deal of similarity between modified stimulus theory and the spontaneous interaction theory" (p. 34), and I would venture to add that spontaneous interaction theory is in some ways compatible with ecological theory. The two theories are similar in that they ascribe much of perception to information in the stimulus, but the Gestalt approach also adds mentalistic processes, such as the effects of familiarity on perception.

Rock finds the spontaneous interaction theory lacking in its ability to explain certain phenomena: "perceptual constancy cannot adequately be explained on the basis of higher-order features such as relationships, ratios, or the interactions to which they give rise" (p. 36). It is for this reason that Rock opts for the second constructive theory that he labels "cognitive theory," a theory that maintains "that the correlate of perception is not the stimulus per se but interpretations or inferences made from it concerning what the object or event is in the world that produced it" (p. 32). Rock sees this approach to perception as incorporating a homunculus, or executive agency where "the better explanatory model here would seem to be one of a higher agency of mind comparing a percept with a specific memory on the basis of certain criteria of what constitutes an adequate match after isolating the latter by some process of internal scanning" (p. 39). Note that the comparison to an item in memory is the type of function carried out by the ventral system.

The previous section (4.1) included descriptions of two studies relating to size perception in the ecological vein with the aim of showing that they are commensurate with dorsal system functioning. In a similar manner, I should like to look at two studies relating to size perception in the constructivist vein. The first is a study that Rock (1983) chose to describe in the section on size constancy in his chapter on unconscious inference (Ch. 9). In that study, Rock et al. (1982) created the illusion of a receding plane using drawings of three-dimensional cubes and their appropriate shadows on a set of three upright textureless boards. The cubes were drawn so as to yield equal sized proximal images. The researchers also saw to it that the edges of the tops of the cardboards were blurred and could not be discriminated. The subjects were asked to report the arrangement of the display, and nearly all reported seeing a flat receding plane (although somewhat tilted upward). They were also asked to compare the size of the top (far) and bottom cubes (near). The results indicated partial size constancy. Looking at the methods these researchers used, it should be noted that they created a very unnatural stimulus situation, one that probably could not occur in a natural scene. The textureless environment severely limited the available information. By having the subjects look through a peephole, they prevented head movements. These manipulations made the pickup of size information by the dorsal system very difficult. The dorsal system normally requires movement and/or binocular viewing for it to function adequately. Movement, binocular viewing, and textures were all missing from the Rock et al. (1982) setup. When dorsal system functioning is limited by "special" laboratory conditions, the ventral system is called on for help. This, together with the fact that the subjects had to make verbal comparisons, which also called the ventral system into play, all leads to an analysis of size perception by the ventral system in this study with very limited intervention of dorsal mechanisms.

In his *Indirect Perception,* Rock (1997) chose to reprint many studies that yielded evidence of percept-percept couplings. None of the studies chosen was a direct study of size perception per se, but one was a study that looked at both speed constancy and size constancy (Rock et al. 1968). Its purpose was to lend support to an indirect theory of speed constancy, in contrast to the Gestalt theory (Wallach 1939), which notably, Rock says (p. 206) "might be thought of as direct." Speed constancy refers to the fact that perceived speed does not appear to change with changes in the viewing distance of the moving object. The Gestalt theory suggests that this occurs because the speed is judged as relative to some frame of reference, and the ratio between the speed of the object and its frame of reference remains constant over varying distances. In contrast, the indirect theory claims that speed constancy is a function of size constancy, constancy of the distance traversed by the moving object. In other words, "speed must be perceived by taking distance into account" (p. 206). In the experiments, the subjects made both speed and size judgments, and for both tasks the results appear to show that distance is taken into account when constancy is achieved. But, once again, the exact results are not of primary interest here, rather, the methods used. In the speed judgment task the subjects judged the speed of luminous circles, and in the size judgment task the size of luminous triangles. Both tasks were carried out in complete darkness, in order to preclude the subject having a frame of reference. These tasks were presented under two conditions, a binocular condition and an artificial pupil condition. In the binocular condition it was presumed that accommodation and convergence would supply distance information. In the artificial pupil condition the subjects wore patches over both eyes with a 1-mm pinhole in the right-eye patch, precluding input of information from both accommodation and convergence. As a whole, the experimental set up is one that leads to much ventral involvement. First, judging speed and size in total darkness with no additional background is a very impoverished and unnatural situation. Second, the subjects were required to make verbal judgments, which would call the ventral system into play. Thus, once again it is claimed that the very experimental paradigm used here leads to the involvement of the ventral system, whereas in a natural information-rich environment speed would be processed in the main by the dorsal system.

To sum up this section on the two theories and their experimental methods, it was seen that the theoretical stances of each theory parallels what is known about the functions of a given visual system: The ecological theory parallels the dorsal system, and the constructivist theory the ventral system. What is more, the experimental methods used by the adherents of the two theories are commensurate with the functioning of the respective visual system. The constructivists in their attempts at isolating the effects of single variables use highly reduced laboratory conditions, and these in turn favor the predominance of ventral system functioning. Followers of Gibson, on the other hand, in trying to create ecologically valid experimental conditions, present their subjects with much richer stimulus conditions. These researchers often opt for motor responses rather than verbal judgments and this leads to a much greater involvement of dorsal system functioning.

5. The emergent dual-process approach and some of its implications

5.1. A dual-process approach

This article has put forth the hypothesis that both approaches to perception, the ecological and the constructivist, are valid descriptions of perception, but of *different aspects.* This hypothesis leads to what I have labeled "the dual-process approach," an approach that bears a great deal of similarity to previous suggestions (e.g., Bridgeman 1992; Neisser 1994). The hypothesis is that perception consists of two systems functioning more or less in parallel. One system that is similar in function to Gibson's (1979) direct perception and labeled dorsal here, and a second system similar to Rock's (1983; 1997) indirect perception and labeled ventral here. The first, the dorsal system, picks up visual information mainly to allow the organism to function in its environment. It does this more quickly than the ventral system, and in the main without much involvement of conscious awareness, and as such does not encumber the cognitive system with the task of "interpreting" the stimulus input. It is suggested that nearly all the information pickup for, or enabling, the performance of well-ingrained actions or behaviors, are carried out by the dorsal system. In contrast, the ventral system primarily serves in the recognition and identification of objects and events in one's environment. It compares visual inputs to stored information in a quest for a meaningful interpretation of those inputs. When needed, the ventral system also participates in other perceptual activities, such as different aspects of space perception like the perception of size and distance. As it is the system of which we are normally conscious it has, in a fashion, "the last word" in our judgmental interpretation of stimulation reaching our senses.

While the two systems have different functions, it should be emphasized that there is a great deal of cross talk between them, and they normally function in synergy. At times dorsal system processing can enter consciousness via the ventral system after the event. What is more, the ventral system often is involved in what appear to be dorsal functions. Some examples: (1) When the dorsal system is faced with difficulties in picking up the necessary information, due to, say, insufficient information or conflicting information, the ventral system can be turned to for help (see Norman 1980; 1983); (2) When the visuomotor behavior in question is complex and not yet well learned, as in the case of, say, novice tennis players, many functions that are later performed solely by the dorsal system are supported by the ventral system (see Williams et al. 1999); (3) When some visuomotor activity leads to some type of judgmental or comparative response, or simply when some verbal response is required, then the ventral systems participates as well; (4) When there is some time delay between the visual input and the required motor output, the ventral system is called upon to temporarily store the visual information as the dorsal system is incapable of bridging that delay.

The question of the relation between the two systems and consciousness is a thorny one. On the one hand, it seems fair to say that the dorsal system functions without much involvement of consciousness, and that the functioning of the ventral system is normally accompanied by consciousness. This generalization is also in accord with the differences between the two theoretical approaches as they were outlined above. The review of Gibson's ecological approach above (in sect. 4.1) indicated that Gibson did not see the direct pickup of information as demanding consciousness. The suggestion, then, is that the Gibsonian pickup of information is carried out without consciousness by the dorsal system, and that the apparent conscious awareness of certain dorsal system processes often is an after-the-fact epiphenomenon resulting from the transfer of the information to the ventral system for registration or assistance when needed. But it should be emphasized that there are reasons to believe that not all dorsal system functions occur without conscious awareness, and not all ventral system functions occur with conscious awareness. Thus, for example, the phenomenon of vection (e.g., Dichgans & Brandt 1978), where a large moving display induces the illusory perception of self-movement, clearly involves dorsal system function, and yet the perceiver is conscious of the illusory movement. In contrast, the phenomenon of masked semantic priming (e.g., Marcel 1983), where a masked stimulus that is not reportable influences a later response, appears to be a ventral system activity without conscious awareness.

I am making the claim here that the dorsal system is incapable of recognizing objects. This contrasts with the approach of Neisser (1994) where in his tripartite division of perception (see sect. 2.3) he suggests that there is one case where direct perception takes part in the task of representation/recognition, that of assigning objects to basic-level categories: "Perceivers do not use subtle surface features to identify *chairs* and *hammers;* the relevant representations are defined in terms of affordances and shapes."[7] Neisser's suggestion is not consistent with the theoretical approach outlined above, which does not (at least at present) leave room for any type of recognition by the dorsal system. His suggestion can, though, be interpreted in the light of the discussion above (see sect. 4.1) concerning Gibson's affordances. When we say that a person picks up the affordance of a chair we are not saying that he recognizes the chair qua the chair concept, but that he is picking up the chair qua sit-on-able surface. It is not clear if Neisser would agree to the latter interpretation when he says that it is "defined in terms of affordances and shapes." This would probably depend on what he means by "representation" in the above quote. If it refers to some fleeting impression, then he might agree, but if it refers to some long-term stored representation, then he would probably not agree. Of course, the stand I am taking here is probably too strong and the truth lies somewhere in between. Witness Marcel's (1998) astounding findings that blindsight patients are capable of picking up word meanings in their "blind" fields. It has been suggested that the "feats" of blindsight patients are carried out by the subcortical parts of the dorsal system. This would lead to the conclusion that the dorsal system is capable of processing word meanings! An alternative possibility would be that blindsight can also be, in part, due to direct connections between the LGN (lateral geniculate nucleus) and the extrastriate cortex. We will have to wait for more research to further elucidate these findings.

5.2. Some implications of the dual-process approach

Very many implications follow from the dual-process approach, both for a better understanding of perceptual phenomena in general and for possible solutions to unanswered theoretical questions. It will not be possible to deal with all of these here, and only a small sample will be presented to try to point to the advantages that can be accrued from a dual-process approach. First, an attempt will be made to outline the differences between Gibsonian "invariants" and constructivist "cues." This will be followed by a look at two theoretical issues. The percept-percept couplings of Epstein, Hochberg, and Rock will be interpreted in a somewhat new light, as will the distinction between what have been called "registered" variables as opposed to "perceived" variables. This will be followed by a brief attempt to analyze a perceptual phenomenon, that of overconstancy in the perception of size. Finally, I will try to show that this approach also has implications for applied topics, by very briefly looking at some of the visual aspects of driving.

5.2.1. Dorsal invariants versus ventral cues. Most textbooks of Perception begin the chapter on space perception with a list of cues for distance and depth perception. These cues are usually classified under various headings such as oculomotor, monocular, and binocular. They serve, in the constructivist tradition, in the perception of space; for example, these cues yield the perception of distance that in turn is taken into account in the perception of distal size. Gibson disagreed with the constructivist idea that space perception is based on an inferential process that takes distance into account, and he therefore disapproved of the notion of cues that were supposed to supply that process with information about distance. From the Gibsonian vantage point, invariants in the ambient optical array suffice to provide the perceptual system with information concerning the attributes of objects and surfaces. Two such invariants were mentioned above, texture interception and the horizon ratio. Note that the invariants are seen to contain information allowing the direct pickup of size information without any need to take distance into account. Can the dual-process approach better elucidate the distinction between cues and invariants?

Both visual systems, the dorsal and the ventral, can respond to the arrangement and structure of the 3D environment, each with a somewhat different purpose and presumably using different mechanisms. The dorsal system responds to invariants in body-scaled metrics, while the ventral system to cues in relative, object-centered, metrics. Are the terms "cues" and "invariants" alternative labels for similar entities, each couched in the theories that engendered them? This is, of course, a possibility, but I should like to propose some distinctions between the two. Following the suggested congruence between the ecological approach and the dorsal system, and between the constructivist approach and the ventral system, it is suggested that the ecological invariants and the constructivist cues differ on four points: (1) they are processed by different centers in the brain; (2) the pickup of invariants is built into the system, it is hardwired, while the cues serve in algorithmic analyses of the visual environment; (3) the invariants are picked up without recourse to any additional central processing, and the cues serve an unconscious inference-like analysis of the three-dimensional space; and (4) the invariants are probably innate and the cues probably learned.

Recent research both on patients with visual form agnosia (mainly patient DF), with what would seem to be a dysfunctional ventral system and a functioning dorsal system, and on healthy subjects has begun to provide evidence on just which invariants are processed by the dorsal system and which cues are processed by the ventral system. The inherent assumption here, of course, is that these two types of information are processed in a different manner by the visual system, and that future research will spell out these differences.

Informal observations of DF indicated that she is capable of utilizing the visual information in her environment to avoid obstacles and to locomote over uneven terrain. In an attempt to experimentally examine these abilities, Patla and Goodale (1996) examined her ability to both judge and negotiate obstacles placed in her path of locomotion. She and six control subjects were asked to perform three tasks: verbally judge the height of the obstacles; raise a leg to the height of the obstacle; and walk over the obstacles. In the first two, judgmental tasks, her performance differed from that of the control subjects, with considerably poorer discrimination of the obstacle heights. In contrast, in the locomotion task she negotiated the obstacles as well as the control subjects. Presumably, her intact dorsal system is capable of picking up the information necessary for obstacle negotiation or avoidance. What invariants enable her to do this? The experimental evidence points to at least two classes of such invariants, binocular and motion invariants.[8] This evidence is based on research that has focused only on very short distances, those within the span of an arm's reach.

An early study (Milner et al. 1991) showed that DF possesses binocular-stereoscopic vision. She was able to report whether an image in a random dot stereogram was above or below the background (but could not report its shape). More recently, Marotta et al. (1997) studied the effect of the removal of binocular information on reaching and grasping movements in two patients with visual form agnosia, DF and a second patient with similar but somewhat less severe symptoms. These patients and control subjects were required to reach for and grasp blocks varying in width, placed at several distances. With binocular vision grip aperture matched block-size and was not affected by its distance for both the patients and the control subjects. With monocular vision the results for the control subjects were similar to those with binocular vision, but for the two patients grip aperture size decreased systematically with distance, indicating a breakdown of size constancy. The authors suggest that unlike the control subjects these two patients were not able to utilize pictorial cues to depth when deprived of binocular vision. These findings appear to indicate that some aspects of binocular vision, perhaps convergence or stereopsis, or both, serve as dorsal invariants. This is consistent with the neurophysiological findings indicating binocular and stereoscopic sensitivity in the parietal cortex. These invariants, it is suggested, serve in the direct pickup of object size. In contrast, some of the pictorial cues appear to be processed by the ventral system, presumably serving in an indirect algorithmic analysis of object size. Very similar results were obtained comparing DF's ability to orient her grasp to match the orientation of a block with binocular and monocular vision (Dijkerman et al. 1996).

Carey et al. (1998) carried out two further experiments comparing DF's performance to that of two control subjects. In the first experiment, two tasks were examined: the subjects' ability to reach out and grasp cubes placed at different distances, and to estimate the cube's distance without making a hand movement. Both tasks were carried out with binocular and monocular vision. In the reaching and grasping task mean peak velocity served as the dependent variable, and DF, while slightly slower than the two control subjects, yielded results that correlated with cube distance, like the control subjects in *both* the binocular and monocular conditions. As might be expected, her distance estimates were considerably poorer than the control subjects' estimates, although they improved somewhat under binocular viewing. In the second experiment, the subjects had to move a finger from a starting position to a position signified by the lighting of a LED (light emitting diode) as quickly and accurately as possible. Here, too, the task was performed with binocular and monocular viewing. The dependent variable was the mean end-point error, and DF was slightly poorer than the control subjects under binocular conditions but much poorer under monocular conditions. While the results of the second experiment are consistent with the claim that the dorsal system utilizes binocular information for picking up information about the close environment, the results of the first experiment are not. This is probably due to the different dependent measures used in this experiment. There are several components that make up prehensile movements. Amongst these are transport (or reach), which refers to the movements of the arm, and grasp, which refers to the shaping of the hand. In using mean peak velocity as their dependent variable, Carey et al. (1998) were measuring one aspect of the transport component. In contrast, the Dijkerman et al. (1996) and Marotta et al. (1997) studies measured changes in the grip, an aspect of the grasp component. The grasp component apparently relies on binocular information, while the transport component apparently makes do with monocular information.

Recently Dijkerman et al. (1999) reported a study that indicates that DF can utilize motion parallax to respond to depth information. In one task DF had to reach out and grasp a block tilted in depth. With monocular vision and a stationary head she was very poor at this task, but when allowed to make head movements her performance was comparable to that of binocular vision. This study appears to show that in addition to binocular invariants DF is capable of utilizing movement invariants.

Many of the same researchers who studied the visual agnosia patients have also carried out parallel research on healthy subjects. Servos et al. (1992) studied prehensile movements in healthy subjects with binocular and monocular vision. They found effects on both components (reach and grasp) of the movements. Monocular vision yielded longer movement times, lower peak velocities, and smaller grip apertures than binocular vision. The finding of faster binocular responses is consistent with the idea that the dorsal system, the faster of the two systems, utilizes binocular information. A second study (Servos & Goodale 1994) examined the effects of the initial view, binocular or monocular, on prehensile movements. The results of two experiments indicated that an initial binocular view is necessary for efficient prehension, but it is not needed during the movement itself. Servos and Goodale (1998) also compared binocular and monocular vision in interceptive movements, for example, catching a ball falling in a pendular motion. They measured eight kinematic parameters, including two relating to grip aperture. They found no effects of the two types of vision allowed the subjects in spite of the fact that, theoretically, binocular vision, through the stereomotion cue, should yield superior performance. This appears to imply that when motion information is available, the dorsal system can use this information for performing at a level no different from when it utilizes binocular information.

Further evidence indicating that the dorsal system utilizes binocular vision for perceiving size can be found in a recent study by Marotta et al. (1998a). They utilized the paradigm of Aglioti et al. (1995), (see sect. 3.3), where subjects were asked to either judge or grasp tokens placed on an Ebbinghaus size illusion background, but with both binocular and monocular vision. The results for binocular vision confirmed the results of the previous study, grip aperture was not influenced by the illusion; but with monocular vision the responses were influenced by the illusion. Marotta et al. suggest that when binocular information is unavailable "the visuomotor system is able to 'fall back' on the remaining monocular cues, which can cause the visuomotor system to be more susceptible to pictorial illusions." In other words, ventral system cues may serve as a backup system for apprehending size information. But Otto-de Haart et al. (1999) recently reported a study of similar nature, using the Müller-Lyer illusion rather than the Ebbinghaus illusion, that does not support these findings. The subjects either matched the size of the central shaft of the illusory figure with the gap between their finger and thumb, or grasped that shaft. Both tasks were carried both binocularly and monocularly. The matching task yielded significant effects of the illusion under both viewing conditions, but grip aperture was found not to yield significant effects under both viewing conditions. These findings do not confirm Marotta et al.'s (1998a) finding that grip aperture is affected only under monocular conditions.

If binocular information is not available can motion information serve for reaching and grasping, as was the case for DF? Marotta et al. (1998b) examined this question by having subjects reach for a sphere under both binocular and monocular viewing conditions with their heads fixed on a bite-bar, or with the bite-bar attached to a flexible spring allowing the subjects to move their heads. The measure of performance used was the number of on-line velocity corrections, which had earlier (Marotta et al. 1997) been shown to be much more prevalent in monocular vision and which was ascribed by the authors as resulting from inaccurate distance information obtained through monocular vision. A strong interaction between binocular versus monocular vision and head restrained versus unrestrained was found. With head movements, monocular vision was only slightly inferior to binocular vision. With head movements restrained, monocular vision was considerably poorer than binocular vision. Binocular performance was not affected by restraining head movements. In contrast, grip aperture was not affected by head restriction. The reason for this is not clear. The authors suggest that it might be due to the fact that only three sizes were used and the subjects learned three motor routines for grasping them.

Marotta and Goodale (1998) recently showed that monocularly guided grasping responses in a "cue-deprived test environment" (i.e., reduction conditions) could benefit from information about height in the visual field. Two types of arrays were used, a "flat" array yielding height information correlated with the object's distance, and an "angled" array not yielding such height information (objects along the line of sight). Their results showed poorer performance under monocular than binocular presentations, and, what is more important, this difference interacted with the elevation information, with only the monocular presentations suffering from lack of elevation information. Marotta and Goodale see height in the visual field as being a learned pictorial cue, but it could also be a correlate of Gibson's horizon ratio with the subjects' eye-level plane serving as the reference, which is an invariant. Their conclusion is that the visual system can learn to utilize the pictorial cue when binocular information is not available.

In sum, both the neuropsychological and psychophysical studies reviewed above appear to suggest that the two visual systems utilize different information for the pickup and perception of the near environment. The dorsal system appears to pick up binocular and motion invariants, while the ventral system appears to perceive pictorial cues. In general, it would seem that invariants are the preferred mode of picking up information about the dimensions of the ambient environment, but, when needed, the cues can serve almost as well, albeit a little more slowly and a little less accurately.

Although the nature-nurture debate is beyond the scope of the present article, it was suggested above that the invariants are probably innate mechanisms while the cues serving spatial perception are probably learned. I made a similar proposal much earlier (Norman 1983) when I speculated: "Ontogenetically, it may be suggested that the infant is innately endowed with direct perceptual capacities, but must learn to use the indirect mode of perceiving." The review of the literature above suggests that binocular and motion invariants serve in the pickup of information about our three-dimensional ambient environment, and these are further supported by a large array of other cues. If these invariants are innate they should appear very early in the infant's development. There are suggestions that this might be the case, as we find in Kellman and Arterberry's (1998) recent review of perception in infancy. At the end of their chapter on space perception, after discussing studies showing that newborns several-hours-old possess both size constancy and shape constancy (Slater et al. 1990; Slater & Morrison 1985), they write:

> These spatial abilities require some degree of metric information about space, as opposed to merely ordinal depth information, and binocular convergence is emerging as the likely source of distance information in these cases. The case for convergence is largely circumstantial, however, and more direct evidence is needed. . . . Motion-carried information about space appears to operate from the beginning, as shown, for example, by responses to kinematic information for approach. More study is needed here as well. . . . Perhaps our clearest developmental picture of the emergence of a depth-processing system is seen in stereoscopic depth perception. The rapid onset of stereoscopic acuity around 16 to 18 weeks of age, along with evidence for innate mechanisms for binocular vision in other species and knowledge of cortical maturation in humans, are all consistent with a maturational account of this important depth perception ability in humans. Arising latest, sometime in the second half-year of life, are the pictorial cues to depth. Whether they depend on maturation, learning, or some combination is unknown. (Kellman & Arterberry 1998, p.108)

5.2.2. Percept-percept couplings. In the review of the constructivist approach (sect. 2.1), it was seen that one of the central claims of the constructivists was that perception cannot be direct, as posited by Gibson, since there is a great deal of evidence indicating that percept-percept couplings occur (Epstein 1982). These were seen as evidence that one percept is influenced by a second, presumably independent, percept. Most of the examples of such perceptual interactions, or perceptual interdependencies in Rock's (1983) terms, presented in Epstein's (1982) article and in Rock's books (1983; 1997), have something in common. The manipulations used to bring about a reported change in the percept rely on changes in some aspect of the perceived spatial layout of the stimulus array. These changes in the perceived layout yield two different percepts of what appears to be a very similar visual array. Thus, perceiving a different layout yields a different percept, namely "percept-percept couplings." For example, one of the manipulations is to have the subjects use either monocular or binocular vision. Compared to binocular vision, monocular vision limits the veridical perception of the three-dimensional layout. In Gilchrist's (1977) study on perceived lightness, subjects perceived the true position of the judged patches with binocular vision, but with monocular vision they were fooled into seeing those same patches as in a perpendicular direction because of the shape of the patches. When thus fooled, the patches appeared to be illuminated with much different lighting and were judged to be very different in lightness than they had under binocular viewing. This occurs in spite of the fact that the surrounding patches in the proximal array are of the same luminance in both perceived layouts. A stimulus-bound theory, such as Wallach's (1948) "ratio principle" would lead to the prediction that the lightness of the patch should be the same in both arrays.

Another example that utilized binocular and monocular vision was Rock and Brosgole's (1964) elegant manipulation of the Gestalt principle of proximity. They arranged a board containing a matrix of lights so that the distances between those in the columns would be smaller than within the rows, yielding a perception of columns when viewed face on. They then rotated the board by 45° so that the proximal distances between the lights in the rows would now be smaller than that between the lights in a column. When this display was viewed with monocular vision subjects reported seeing the lights arranged in rows. The opposite was true when they were viewed binocularly. Presumably binocular vision allowed perception of the true spatial layout (the 45° rotation), while the monocular vision did not.

How does the dual-process approach deal with these and other examples of interactions between percepts? First of all, in all these studies subjects are required to make perceptual *judgments* leading to the involvement of the ventral system. What is more, these percept-percept couplings occur in rather restricted stimulus situations where it is relatively easy to change between two percepts by simple manipulation (e.g., binocular vs. monocular viewing) and often the viewer is looking through some "peep-hole." In other words, the situation is such where it is easy to "fool" or manipulate the perceptual system. In such situations the ventral system is called into play, and it has the "last word" in determining the conscious percept. It would prove interesting to see whether a motor response, presumably controlled by the dorsal system, would be fooled in the same manner. For example, would a false perspective cue, which would presumably cause a misperception of an object's distance, yield a dissociation between the two systems? Would a visually guided motor response, such as open loop reaching or blind walking also evidence a distance distortion? One would speculate that it would not, since the dorsal system does seem to utilize pictorial cues, and it can rely on the pickup of binocular or movement invariants.

5.2.3. Perceived versus registered variables. In the study of size perception that was mainly carried out in the constructivist tradition it was suggested that size and distance perception covary systematically. In other words, the farther away the proximal image of some given size is perceived to be, the larger the object appears. This relationship has been labeled the "size-distance invariance hypothesis." Many studies have attempted to validate this hypothesis, but quite a few of them yielded results inconsistent with it (see review in Epstein et al. 1961) and this finding has been labeled "the size-distance paradox" (Gruber 1954). Basically, this paradox refers to the fact that the reported size and the reported distance did not covary in accordance with the invariance hypothesis. Explanations of these discrepancies, both for the perception of size and for the moon illusion, have been suggested in terms of the differentiation between *perceived* distance and *registered* distance. Rock and Kaufman (1962) used these terms for a distinction that had been made earlier by Woodworth and Schlosberg (1954), between some sort of automatic apprehension of distance, registered distance, and judgments of distance, perceived distance. Much later Kaufman and Rock (1989) wrote: "we suggested that the conflict is between cues to distance having effects that are *registered* by the perceptual system (and this can occur without conscious awareness) and judgments of distance that are not based on the registered cues" (p. 199). The size-distance paradox can be reinterpreted in terms of the dual-process approach. In the studies in question *judgments* of both size and distance are required and thus the ventral system is always involved. When size judgments are requested these judgments utilize distance information picked up by the dorsal system (registered distance), but when distance judgments are requested these are furnished by the ventral system (perceived distance). This claim is strengthened by the findings briefly reviewed above (see sect. 3.3), which yield evidence of the dissociation of distance judgments in the two systems.

In sum, it is being suggested that the distinction between registered and perceived distance might be interpreted in terms of the dual-process approach, where the registered distance is that picked up by the dorsal system, and the perceived distance that perceived or reported through the functioning of the ventral system.

5.2.4. Size perception and overconstancy. In everyday life we seem capable of perceiving our three dimensional environment and the size of objects and apertures in it quite veridically. We grasp objects of different sizes, walk through apertures, and drive through narrow streets with hardly an error. In contrast with this apparent ability, studies of size perception have yielded surprising results in that size constancy has been found to be relatively inaccurate. Many of these studies have reported a phenomenon that has been labeled "overconstancy," where the general experimental result is that subjects overestimate the size of far-off objects, with this overestimation often increasing with distance (e.g., Gilinsky 1955). The finding of overconstancy is problematic. First, as the judged object is placed at greater distances, its proximal image becomes smaller, leading one to predict that underconstancy might occur but not overconstancy. Second, it is hard to think of some adaptive need for this sort of process. And, third, one does not encounter manifestations of such overconstancy in real-life situations when people are faced with tasks requiring accurate size perception.

In her study, Gilinsky had subjects judge the size of triangles outdoors at relatively great distances (up to about 975 m) by matching them to a nearby variable triangle. Her subjects were asked to adopt one of two modes of response (or "attitudes," as she called them) towards judging size. Objective instructions required that "if you measured both with a ruler they would measure exactly the same," and retinal instructions required a match with the proximal size. Of interest here were her findings with the objective instructions. She summarized them thus: "Matches of 'objective size' do not strictly follow the rule of size constancy. Instead, objects tend under this attitude to *expand* as they move further away." Her interpretation of this finding was: "Certainly in this instance objective size is being estimated inferentially and is not being immediately perceived." Gilinsky's study was followed by a series of studies that refined the exact instructions given. Carlson (1962) devised four, rather than two types of instructions, and Epstein (1963) used these four in a broader study. Of these, only two are relevant here. One was labeled "objective" and similar to Gilinsky's instructions of the same name, and the second was labeled "apparent" or "phenomenal" and asked the subjects to "adjust the variable so that it looks equal to the standard . . . whether you think it is equal in actual size or not." In Epstein's (1963) study, under the objective instructions the finding was again that of overconstancy, increasing with distance. In contrast, under the phenomenal (or apparent) instructions size constancy was found to prevail at all distances. In other words, when the subject was instructed "to tell it as she sees it" the size judgments were quite veridical.

Similar findings have been reported in other studies and they have been attributed to the idea that more "cognitive" processes are involved under the objective instructions, while the apparent instructions induce more "perceptual" processes (e.g., Gogel & Da Silva 1987). The question, of course, is just what "cognitive" and "perceptual" mean here. In the cognitive mode are the subjects making deliberate calculations or is the cognitive process something like the unconscious inference posited by Helmholtz? Deliberate calculations should take some time, while unconscious inferences should be much faster. Epstein and Broota (1975) compared response times for objective and phenomenal instructions. These response times were very quick, with those of the objective instructions being longer than those for the phenomenal instructions. The mean response time for objective instructions was only 582 msec, hardly a value to be expected if deliberate calculations are carried out. It should also be noted that while the phenomenal response times did not increase with stimulus distance, those for the objective distance did, a finding similar to the overconstancy results found in the size judgment studies.

Epstein and Broota ascribe the increase in response times with objective instructions to a postperceptual decision stage where the confidence in the veridicality of the size estimate decreases with increasing stimulus distance. While this is a possibility, an alternative explanation can be suggested, based on the two visual systems concept. It is suggested here that size information can both be picked up by the dorsal system and perceived by the ventral system. In our everyday functioning in our environment it is probably the dorsal system that we most often rely on, but when faced with experimental judgment tasks both systems can be utilized. It is posited that the objective instructions induce a greater reliance on ventral size perception while the phenomenal (apparent) instructions rely more on dorsal pickup of size information. The perception of size by the ventral system serves primarily in identification where relative size information usually suffices, while dorsal system size information pickup usually serves in one's motor interaction with one's environment. Thus, the dorsal pickup should be more veridical than the ventral perception.

Is there any evidence for the suggested greater intervention of ventral processes with objective instructions? Such evidence can be found in studies of the effects of object familiarity on judged size and distance. Use of familiar objects in studies of size and distance must, of necessity, invoke ventral system functioning, as their familiarity depends on some long-term storage system, and only the ventral system possesses such a system. There have been experimental demonstrations of the effects of familiar size on perceived size. Not very surprisingly, off-size versions of familiar objects, when presented under reduction conditions, yield nonveridical perceptions of their true size with the subjects tending to report them closer to their familiar size than they really are. Several studies have looked at the interactions between the effects of stimulus familiarity and type of size judgment instructions given. In one such study (Predebon 1992), different groups of subjects were asked to judge the sizes of familiar playing cards, of either normal size or one-and-a-half times larger, or of blank stimuli of the same two sizes. The subjects were given either objective or phenomenal instructions. If the claim that objective size judgment instructions induce more ventral system involvement in the size judgments is correct, then we would expect the familiarity variable to be more effective with the objective instructions than with the phenomenal instructions. This is exactly what Predebon (1992) found: "The influence of familiar size on direct reports of size and distance is dependent on the form of instructions, with apparent instructions minimizing, and objective instructions promoting, the effectiveness of familiar size information."

It is being suggested here that the dimension of size in the three-dimensional array that surrounds us is both picked up by the dorsal system and perceived by the ventral system. Normally we are not that conscious of the size of objects around us, we simply function in accord with their physical size. When we are consciously aware of an object's size, the size information probably accrues from both the ventral system and from its cross talk with the dorsal system. Often when we are consciously aware of the size of objects it is only of their relative size, as this is all that is needed for recognition by the ventral system. It is for this reason that we do not really perceive the true size of professional basketball players when watching a game. We only note their true size when we come close to them.

It should be noted that most of the neuropsychological and psychophysical studies reviewed above on the dissociation of the two systems dealt with the relatively near environment. For example, a majority of the studies focused on reaching behavior. Here it is being suggested that a similar dissociation subserves the perception of size at much greater distances. There is very limited empirical evidence for a similar dissociation at greater distance. One exception is the studies of the perception of the steepness of hills (Creem & Proffitt 1998; Proffitt et al. 1995) briefly described above (see sect. 3.3). In these studies a dissociation was found between the judgmental assessments and haptic motor assessments, and they were attributed to the functioning of the two systems. These are, I believe, the only studies indicating a dissociation between the two systems for distances beyond a few meters.

5.2.5. Driving. If we adopt the notion of two visual systems functioning simultaneously, one with conscious awareness, one without; one with different specializations than the other; then there are many implications of an applied nature. One field of relevance is human navigation of vehicles, such as driving a car. In this task the driver would benefit if he were able to use both systems more or less simultaneously, especially the dorsal system for navigating. This idea is not new. Many years ago, the implications of an older version of the two visual systems concept in relation to driving were pointed out by Leibowitz and Owens (1977). One of the important differences between the two systems is the relative sensitivity to peripheral vision. The dorsal system is much more attuned to the peripheral visual field than is the ventral system. Presumably, when walking or running through our environment we utilize information picked up by the periphery of our retinas for navigational purposes; for example, not bumping into objects or not stumbling over obstacles. There is good reason to believe that peripheral inputs into the dorsal system can also serve in the navigation of vehicles, and several studies have looked into this question. For example, one study (Summala et al. 1996), had novice and experienced drivers drive along a straight road using only peripheral vision for maintaining lane position while performing another task foveally. The results were different for the two foveal tasks used, but in general the experienced drivers could much better utilize their peripheral vision for lane keeping.

A recent study on driving (Higgins et al. 1998) is also of interest. It examined the effects of optical blur on driving behavior. From what we know about the two visual systems, it is the ventral system that requires high spatial frequency information while the dorsal system is much less reliant on fine details. Thus, we would expect ventral system functions to be affected by the wearing of blurring lenses while dorsal system functions would be much less so. The results of the study corroborate these expectations:

> Acuity degradation produced significant decrements in road sign recognition and road hazard avoidance as well as significant increments in total driving time. Participants' abilities to estimate whether clearances between pairs of traffic cones were sufficiently wide to permit safe passage of the vehicle and to slalom through a series of traffic cones were relatively unaffected by acuity degradation. (Higgins et al. 1998)

The two ventral tasks, the recognition of road signs and of hazards, evidenced deteriorated performance with optical degradation. Of the two tasks not seriously disturbed by optical degradation the slalom task is clearly a dorsal task, where the driver's visual inputs direct his/her actions in navigating the vehicle. The second task, that of estimating gap clearances between pairs of traffic cones, is a size perception task that can be carried out by both the dorsal and ventral systems, as was pointed out above. The fact that it did not deteriorate with blur appears to indicate that in this task it mainly depended on dorsal system input. It should be noted that it was not only the subjects' driving through the clearances or around them (when they were too narrow) that was not affected, but also the verbal estimates they gave. It was suggested above that the requirement of verbal estimates introduces ventral system participation in the perceptual task. The fact that these estimates were not affected by blur can be seen as evidence for the communication between the two systems, where the ventral system receives input from the dorsal system and utilizes that information in its estimates. Unlike psychological experiments where the subjects have had very little experience with the size judgments required of them, most adults have had a great deal of experience in estimating the clearance of gaps while driving their cars. This might be the reason why the verbal estimates appear to rely mostly on the dorsal system analysis.

6. In conclusion

The main aim of this article was to reconcile the two contrasting approaches to perception, the constructivist and the ecological. This was done by showing that they can coexist in a broader theory of perception. This broader theory is based on the accumulating research findings that point to the existence of two visual systems, the dorsal and the ventral. It was suggested that the ecological approach broadly parallels the functions of the dorsal system, and the constructivist approach broadly parallels that of the ventral system. These two visual systems deal with different aspects of perception. The dorsal system deals mainly with the utilization of visual information for the guidance of behavior in one's environment. The ventral system deals mainly with the utilization of visual information for "knowing" one's environment, that is, identifying and recognizing items previously encountered and storing new visual information for later encounters. But it should be stressed that both systems overlap in the functions they perform. Thus, it was seen that both systems are involved in the perception of size, albeit with somewhat different purposes and specializations.

Emerging from this attempt at integrating the two approaches into a single theory is a dual-process approach to visual perception. According to this approach, much of our day-to-day pickup of visual information is carried out by the dorsal-ecological system without involving much conscious awareness. In the main, the information picked up is that which allows the organism to function within its environment, that is, Gibson's affordances. The ventral-constructivist system, on the other hand, is a "higher" system that deals with the interface between the visual input and cognition, and we are normally conscious of its output. Only, it possesses a long-term memory and therefore any type of identification or recognition must transpire within it. As the dorsal system is mainly concerned with directing motor behavior in one's environment it must rely on body-centered information, in absolute units, about the environment and the objects in it. In contrast, the ventral system in its attempt to recognize objects can suffice with relative, object-centered information. It is probably for this reason that in quite a few of the studies reviewed above, dorsal system functions yielded more accurate information about the visual environment than did the ventral system. It was seen that one of the best ways to distinguish between the functioning of the two systems is to utilize very different response modes. The functioning of the dorsal system can best be tapped by requiring motor responses from the subject, while judgmental responses should call forth more ventral processing.

It was suggested that the emergent dual-process approach can shed new light on old theoretical and empirical quandaries, and a few examples were presented. A distinction was drawn between dorsal system invariants and ventral system cues, with the former being hardwired into the perceptual system and the latter serving in algorithmic processes. Evidence is only beginning to accumulate concerning these two types of visual information and their processing. Recent research appears to indicate that some type of binocular information and motion information serve as invariants picked up by the dorsal system, while the pictorial cues (or some of them) appear to be processed by the ventral system. Much more research is needed to better determine just what information is used by the two systems. One hopes that the dual-process approach will serve to engender such research and it in turn will not only demarcate the functions of the two visual systems, but will also elucidate their synergistic interactions.

In ending, a few qualifications need to be mentioned. The proposed integrative theory and its concomitant dual-process approach are clearly an oversimplified view of what transpires in visual perception. It is hoped, though, that they can serve as an initial step in the generation of more sophisticated, and presumably more complex theories that will also incorporate both theoretical approaches in a single broad theory of perception. Furthermore, there still remains the question of whether the broader approach outlined here can be generalized to other domains of perception other than the perception of space, or of the ambient environment, to use Gibsonian terminology. A contradictory example appears to exist in the realm of motion perception where Wertheim (1994) has suggested that the direct (dorsal) system utilizes relative metrics and the indirect (ventral) system absolute metrics. The current approach would, of course predict the opposite. Finally, the parallels drawn between the two approaches, the constructivist and the ecological, and the two visual systems, the ventral and the dorsal, respectively, will probably draw criticisms from the adherents of each approach, who will surely find contradictory examples. Hopefully, in spite of these, the overall approach will prove to be basically valid, and will yield new insights to the understanding of visual perception.

ACKNOWLEDGMENTS

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NOTES

1. In all the quotations in this paper the emphases (*italics*) are to be found in the original and are not my emphases.

2. Gibson was not the first to introduce the concept of invariants, he was preceded by Helmholtz and Koffka among others (see e.g., Cutting 1986, Ch. 5), but as Hochberg (1998b, note 15) notes: "Gibson's effort was new and revolutionary in that he tried assiduously to work around the need for any perceptual process other than the pickup of such invariances."

3. I was unsuccessful in finding original names for these two systems. All the psychological terms that came to mind had been used previously and were encumbered with unwanted connotations. Therefore, I decided to retain the anatomical labels originally suggested by Ungerleider and Mishkin (1982), but only as general labels and not to specify anatomical loci.

4. *Exocentric distance* refers to the distance between two objects or two locations in space. *Egocentric distance* refers to the distance between the observer and an object or location in space.

5. It should be stressed that while it is being claimed that the dorsal system does not appear to have much of a *representational* memory, it certainly passes on information to some type of procedural memory system. Without such a system perceptuo-motor learning could not transpire.

6. Very recent neuropsychological research strengthens the claim made here that the dorsal system only picks up the functional properties of objects. Hodges et al. (1999) tested two patients with bilateral temporal lobe (ventral system) atrophy, suffering from "semantic dementia." These patients were unable to name twenty familiar objects and also performed poorly when asked to demonstrate their use. In those cases where the patient did not succeed in demonstrating the objects true use, "he demonstrated a use that was incorrect but largely compatible with the object's physical properties (e.g., he carefully removed each match from the matchbox, commenting that they looked like 'little pencils' and holding them as if to write . . .)." In contrast, these two subjects performed flawlessly on a "novel tool task" in which they had to "select the appropriate one of three novel tools for lifting a wooden cylinder (with a special feature matched to the appropriate tool) out of a socket." In other words, they were able to pick up the affordance of the novel tool as appropriate for the task at hand.

7. Neisser's recent hypothesis is that the dorsal system does not categorize, not even on the basic level, but that "basic level categories are the easiest categories to learn because the dorsal system passes this kind of information on to the ventral system, which is thereby alerted to the existence of an important category" (personal communication, May 1999).

8. This is not meant to imply that all binocular and motion information is processed solely by the dorsal system. Evidence exists indicating that both motion information (e.g., Ferrera et al. 1994) and binocular information (e.g., Jannsen et al. 1999) can be also processed by the ventral system.

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Are the dorsal/ventral pathways sufficiently distinct to resolve perceptual theory?

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Abstract: The author argues that the theory of a dorsal/ventral stream for visual processing can be used to reconcile the constructivist and direct perception theories. My commentary discusses neurophysiological and psychophysical studies that run counter to the view. In addition, the central issue of debate between the constructionist and direct perception approaches regarding what is visual information is discussed.

Norman proposes that the conflicting positions of the constructionist and ecological theories can be resolved by linking these positions to the two visual pathways proposed by Milner and Goodale. In my opinion there are several counterarguments to this proposal.

One problem with this view concerns the role of motion information for perception and action and what is known about the neurophysiology of the visual system. According to the article, the dorsal stream includes regions known to be responsible for motion processing. These regions include area MT (medial temporal region) and area MST (medial superior temporal region). A number of studies have examined the sensitivity of these regions to differential invariants (divergence, curl, and deformation) that can be used to determine self-motion, heading, and structure from motion. If these pathways are distinct with regard to whether they specify information for action or judgment, then one should find sensitivity to differential invariants in accordance with whether they are associated with action or judgment. That is, one should see action based motion information (such as information for heading or self-motion) processed in the dorsal stream (areas MT and MST) and information for judgment (such as structure from motion) processed in the ventral stream. However, one does not find a nice segregation of motion information (action based or judgment based) with a particular pathway. For example, divergence information, which is processed in MSTd, can be used to specify action (e.g., heading). But the same information can be used to determine the number of overlapping transparent surfaces in a flow field (see Andersen 1989). Similarly, Orban et al. (1995) found MST cells that are sensitive to deformation. This information is only useful for specifying object shape. Thus, one does not find a clear disassociation between the dorsal and ventral pathways in specifying motion information for action or judgment.

This problem is alluded to in the target article (sect. 3.1) with the author proposing that at some level the two processing streams must be integrated. However, this explanation avoids addressing the question that, if there must be integration of information across the pathways, then are the pathways really distinct?

An additional concern regarding the target article is that it does not directly address the fundamental issue that delineates the constructivist approach and the ecological approach – the question of what is the nature of visual information. The article cites Rock and Helmholtz as examples of the constructivist approach. But I believe a better example of the constructivist approach is Marr's theory of computational vision (Marr 1982). According to the computational approach, image information is combined with constraints based on regularities of the environment to recover visual information useful to the observer. For example, in order to recover structure from motion the visual system can use constraints such as constant angular velocity or fixed axis motion. Such constraints serve as assumptions that constrain the possible 3D interpretations to the motion and can be used to recover a unique 3D interpretation. In contrast, Gibson's view is that constraints or assumptions are not needed. Rather, information is directly specified in the optic array or optic flow field in the form of invariants.

A fine example of the debate between these two approaches on this issue is the exchange of letters between Gibson and Johansson (see Gibson 1970 and Johansson 1970). In these letters, Johansson agues that "Without a priori assuming the existence of 3-D rigidity there is no specific information about space available in visual stimulus even in connection with locomotion" (p. 71). He goes on to argue that the visual system must use decoding principles to recover information about space perception. Johansson's argument not only is consistent with Helmholtz's notions about unconscious inference and Marr's ideas regarding computational vision, but it illustrates that Johansson was not an advocate of direct perception (a view expressed by many perceptual researchers including the author of the target article). Gibson's reply to Johansson is that assumptions are not needed. Specifically, he states that "I think it is a fact, a geological fact, that the surface of the earth tends to be rigid and tends to be composed of evenly distributed units" (p. 77). In short, Gibson is arguing that one does not need to invoke assumptions because that is the way the world exists. The fundamental issue of debate here is what is visual information. This debate cannot be resolved by an explanation based on separate neural pathways.

Perceptual theory has consistently and repeatedly proposed parallel processing schemes to account for the complexities of vision. The author cites the focal/ambient theory as an early construct that served as a precursor to the theory of the dorsal/ventral streams for perceptual action and judgment. The focal/ambient theory argued that ambient processing occurs in the peripheral visual field and is responsible for recovering information important for locomotion (e.g., self-motion and vection). However, psychophysical research demonstrated that self-motion/vection could occur in central vision (Andersen & Braunstein 1985), providing evidence that the visual system is not as neatly organized as was assumed by advocates of the focal/ambient theory. The author acknowledges a similar problem with the magnocellar/parvocellar theory of visual processing. Specifically, he correctly notes that the neurophysiological data does not support a neat and clean distinction between the neural pathways and visual processing. In my opinion, the dorsal/ventral theory of visual processing has the same problem as these earlier theories – namely, that the visual system is not organized in terms of independent parallel systems. As a result, it does not provide a strong theoretical foundation from which one can argue evidence for a constructivist/computational pathway and an ecological/direct perception pathway.

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When is movement controlled by the dorsal stream?

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Abstract: Our commentary focuses on the functional link between the ventral and dorsal systems implied by Norman, as they relate to overt movement. While issues relating to space perception and size constancy are the primary justification for this dual-process theory, the philosophical extensions of this approach are less consistent with examination of motor control and, in particular, motor learning.

Overall, Norman presents a compelling argument for associating the visual streams with function specific philosophies: Helmholtzian and Gibsonian. While this position is inherently attractive, on account of the demonstrated successes of each philosophy in isolation as well as in congruence with the neurophysiological evidence of Goodale and colleagues (e.g., Milner & Goodale 1992; 1995), its weakness lies in its inability to unify the viewpoints in a cohesive manner.

At the surface, there are a number of aspects of the proposed system/theory harmonization that are consistent with current approaches to the study of motor skill. Most descriptive (Fitts 1964; Gentile 1972) and theoretical (Adams 1971) accounts of skill acquisition separate the learning process into distinct stages where early stages are characterized by verbal/cognitive process and, subsequent to practice, tasks become more motor. It is also commonly demonstrated that skilled performers are able to perform with exceptional motor skill while being unable to provide a verbal report of what exactly they did or how they did it. These examples are at least superficially consistent with a dual process approach. Both examples suggest that initial unskilled attempts at performing motor actions are either verbally or cognitively dominated, supposedly using the ventral stream and constructivist

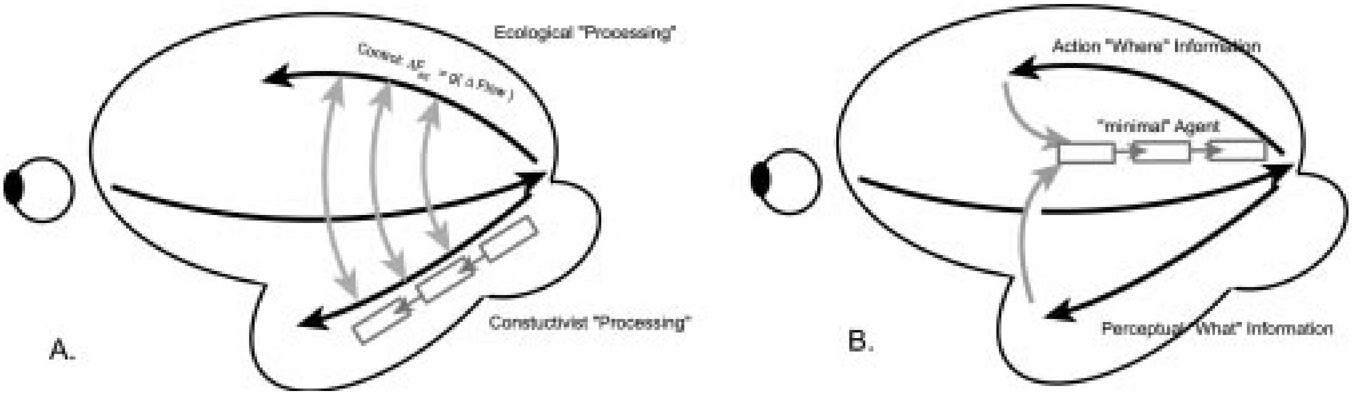

Figure 1 (Binsted & Carlton). Schematic illustrations of hypothetical sensory pathways and processes. A. Norman's 'dual-process' model depicting a ventral pathway containing information processing analyses and a dorsal pathway extracting directly perceivable variables. B. An alternative model which posits that streams perform prefilter transformations of sensory streams. The symbolic output is then evaluated by a process-limited executive.

processes. With a large amount of practice, control appears to switch to a "motor frame," consistent with dorsal stream information, and begins to arise in a more automatic fashion – nonconscious or ecological.

Norman writes:

> When the visuomotor behavior in question is complex and not yet well learned . . . many functions that are later performed solely by the dorsal system are supported by the ventral system. (sect. 5.1, para. 2)

The important feature here is how the dorsal system is able to benefit from the learning apparently accomplished within the ventral stream. According to the basic assumptions of Gibsonian psychologists (sect. 2.2), all required information must be attainable from the visual array by a process[1] of resonating toward the environmental invariants. Consider the simple "skills" of riding a bike and running. Imagine the scenario where an object "appears" immediately before the person. For a runner, this array affords jumping or stopping – presumably pre-set action-perception coupling. However, given the *identical* visual array, for the cyclist steering or braking are required. How does the ecological stream acquire this coupling without memory? The obvious answer, and the one we think would be acceptable to many movement theorists is that *ecological processes* (e.g., t, dt/dt) amount to a motor pre-filter; perhaps transforming the complex visual array into symbolic form (e.g., self-organizing maps). Moreover, if one permits the adjustment of these pre-filters in even a limited sense, either by resonance (Adaptive Resonance Theory; Grossberg 1976a; 1976b) or any unsupervised approach, the role of the ecological structure becomes more reasonable. But, without structural (or virtual) memory, whether by self-organization or supervised (executive) control, the dorsal stream is incomplete as a component of a learning system.

Similar problems arise in the expression of an ecological/ventral combined system for simple, well learned, actions if "the dorsal stream is faced with difficulties" (target article, sect. 5.1, para. 2). Although it would appear contrary to the intent of Norman, this example also appears to necessitate some agent[2] (homunculus) role – deciding the relative efficacy of the visual array. Such an executive would require real-time access to the "ecological" stream in order to make such evaluations. While it could again be argued that such an evaluation may be made without executive control (a "dis-order" invariant perhaps or resonance), this seems inconsistent with several experimental findings. In particular, Hu et al. (1999) suggest that the dorsal streams remains in control of movements for a short interval following occlusion. Moreover, according to Norman's account, the influence of spatial perception should influence motor output if, and only if, the visual array is degraded or removed during movement execution. While both of these findings have been demonstrated (e.g., Binsted & Elliott 1999a; Westwood et al. 2000), the required level of degradation/interference is equivocal.

Although neither example seems overly supportive of Norman's view, the general goal and much of the implementation of this view is valid and consistent with an earlier account of perception and action. Specifically, Kugler and Turvey (1987) suggest a "minimally intelligent agent" required to "act minimally" – this may perform exactly the combining role necessary. In Figure 1, we have summarized Norman's view (A), and an alternate view (B), where the dorsal and ventral *pathways* merely conduct information. This view is certainly contrary to Norman's but perhaps more consistent with the original conceptions of Goodale and other proponents of separated visual streams. Our contention is that, instead of two fundamentally different systems interacting (across some measure of space) and requiring the induction of a variety of new constructs, the information converges on a "Kugleresque" agent – here evaluating the efficacy of each stream, perhaps updating the perceptual and action *prefilter* to account for context or memory (Fig. 1B). What about other forms of information: proprioceptive, tactile, auditory? How are these controlled/accessed? There are strong indications that there are similar ecological information sources from within haptics but that this system is independent of contextual (perceptual) manipulation (e.g., Cooper et al. 1999). While this is consistent with a dualistic approach to information, there arises a question of the benefit of a unique *process* for any and all streams. The problem is further confounded if all "processes" are required to interconnect in the rapid and low-level manner Norman suggests.

In summary, the control of movement provides a number of challenges to Norman's view of separate theoretical approaches to thinking and doing. In the end, a single theoretical standpoint will be the most coherent explanation of the extant literature, not a concatenation of historical viewpoints.

NOTES

[1] The term "process" is attributed to both the constructivist and ecological events; while this term may be somewhat unpalatable to ecological psychologists, it is intended to reflect both the resonance/transformations of their viewpoint and the "boxes"/computations of constructivists.

[2] Here the term agent is intended to infer an "intelligent" (computational or philosophical) entity. No presumption of consciousness is to be inferred, implicitly or explicitly.

A better understanding of inference can reconcile constructivist and direct theories

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Abstract: The attempt to relate distinctions in perceptual theory to different physiological systems leads to numerous exceptions and inconsistencies. A more promising approach to the reconciliation of constructivist theory and direct perception is to recognize that perception does involve inference, as the constructivists insist, but that inference is a process in logic that does not require unconscious reasoning and need be no more thought-like than resonance.

Marr pointed out the importance of understanding visual perception at the levels of theory, algorithm, and implementation, but he wisely cautioned us to avoid "confusion about the level at which problems should be addressed" (Marr 1982, p. 25). The attempt to relate the differences between constructivist theory and direct theory to differences between ventral processing and dorsal processing appears to confuse issues of theory and issues of implementation. The target article presents a balanced view of the evidence, and it is clear that many results are inconsistent with the proposed relationship between theoretical positions and physiological systems. Inconsistencies in the classification of stimuli and responses further indicate that the dichotomies proposed in the target article are somewhat arbitrary: Raising a leg to the height of an obstacle is regarded as a judgment and is thus classified as a ventral function, whereas adjusting a tilt board with an unseen hand to indicate the slope of a hill is regarded as a dorsal function. Pictorial cues are regarded as ventral and invariants as dorsal, but the horizon-ratio, one of the best examples of an invariant, is effective as a pictorial cue.

The distinction based on the type of research conducted by proponents of the constructivist and direct views is somewhat artificial as well, applying only to extreme cases. Object recognition studies certainly fit into a cognitive framework, but many perception researchers would classify object recognition as cognition rather than perception. Some proponents of direct perception insist on active responses, but almost all use judgments as their dependent measure.

If the distinction between constructivist and direct theories is not related to the physiological implementations of the processes of interest to each type of theorist, what is the source of the distinction? It is largely due to a misunderstanding of the nature of inference in perception. Constructivists understand the necessity of inferential processes in perception, but they may regard these processes as necessarily intelligent and thought-like, involving rule-following, and even requiring an executive (Rock 1983) to oversee the whole system. Direct theorists, recognizing the pitfalls of introducing thought-like processes, and even worse, a homunculus, object to this type of inference as part of a perceptual theory, and some direct theorists reject inference altogether. But inference need not be equated with intelligent reasoning. Inference describes a form of logic, not necessarily an intelligent act, and certainly not something requiring a homunculus. It is a process that leads from premises to conclusions, but it can be described in a formal way and implemented even in a mechanistic way. It does not require explicit references to knowledge, although the inferential process can instantiate knowledge. Bennett et al. (1989; 1991) have shown elegantly how inductive inference in perception can be formally represented in a mathematical model. An inferential process can develop as part of an organism's physiology, in response to the demands of its ecological niche, in Gibson's (1979) terms. It can be instantiated as a "smart mechanism" (Runeson 1977). An inferential mechanism can be thought of as instantiating a rule rather than following a rule (Hatfield 1990b). Even inanimate devices can instantiate inferential rules. For example, one can regard the sound pressure in the vicinity of a tuning fork as a premise, and the vibration of the tuning fork as a conclusion, in an inferential analysis of resonance.

If the concept of inference is completely separated from intelligence, thought, and active use of knowledge, and is allowed to encompass smart mechanisms and resonance, there is no need for direct perception theorists to object to inference. And there is no reason for constructivists to invoke higher level processing merely because they can show that perception logically requires inference. This possible reconciliation of divergent theoretical viewpoints is probably already understood by most perceptual researchers. The theoretical distinctions represent extremes. There have long been major theoretical viewpoints in-between these extremes. Two of the most insightful perceptual theorists, Walter Gogel (e.g., Gogel 1990) and Gunnar Johansson (e.g., Johansson 1970), have occupied this middle ground. Indeed, Johansson (1970), in what appears to have been a vain attempt to persuade Gibson (see Gibson's 1970 response) that his position was too extreme, briefly describes a middle ground based on "decoding principles" which are automatically applied. The concept of decoding principles is compatible with a wide spectrum of viewpoints, including smart mechanisms (Runeson 1977), observer theory (Bennett et al. 1989; 1991), heuristics (Braunstein 1972; 1976; 1994), and rules in visual perception (Hoffman; 1998).

But can there be a middle ground with regard to percept-percept coupling? If one perception can cause another perception, then perceptual inference is thought-like and reconciliation of constructivist and direct approaches would be hampered. Epstein (1982) presents a number of cases that are consistent with percept-percept coupling. It is very difficult, however, to convincingly establish that a perception, and not some aspect of intermediate processing, causes another perception. According to Bennett et al. (1989), perception is based on a chain of inferences, with the conclusions of one inference becoming the premises of another. Consider, for example, the relationship between the perceived distance of two sides of a physically constructed wire cube and the perceived size of the sides. When there is a reversal in perceived distance, there is a corresponding change in the relative perceived sizes. Is this because the perceived distances determine the perceived sizes? That would be a percept-percept coupling explanation. Suppose, instead, that a process that infers distance provides its conclusion to a process that infers size in a completely mechanistic way. When the distance conclusion changes, the size conclusion also changes, but should that be considered percept-percept coupling? If so, then all internal processing would have to be considered perception. The inference of yellow at the LGN (lateral geniculate nucleus) from the outputs of red and green processes at the retina would be percept-percept coupling as well. Given these considerations, evidence for percept-percept coupling does not appear to be a barrier to a reconciliation of constructivist and direct perception theories, except for the most extreme direct perception position that rejects all processing.

In summary, I have two primary reasons for disagreeing with the conclusions of the target article. First, the distinctions made between constructivist theory and direct perception theory are based on extremes and do not reflect most of the research or researchers in visual perception. Second, the theoretical differences are based on the correct view that perception must include inference combined with the incorrect view that inference must involve thought-like processes. A better understanding of inference in perception should resolve the major differences between the constructivist and direct theoretical approaches.

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Two visual systems but only one theory of perception

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Abstract: The parallel drawn by Norman between the dorsal and ventral systems and direct and indirect approaches is based on two misrepresentations of the direct approach – that it is concerned only with the unconscious control of action, and that it cannot explain learning. We propose a way of understanding the visual system differences from within the direct approach.

Norman has suggested that the functioning of the dorsal system can be understood from a direct/ecological perspective and that of the ventral system from an indirect/constructivist perspective. We believe that this reconciliation falls short in two important respects. First, it is our contention that no kind of perception is best understood as indirect. In other words, perception via the ventral system is as direct as that via the dorsal system. If this is true, then the differences between the two visual systems must be a consequence of something else. Our second point then, is that these differences can be understood as a division of function within the direct approach.

Norman suggests that direct theories successfully explain only unconscious perception which guides manipulative action. We disagree. Clearly, some information pickup (in the Gibsonian sense) is unconscious, but this does not mean that all information pickup is unconscious. For example, orienting towards and tracking a swooping bird, watching it land, and then trying to identify what kind of bird it is as it sits behind swaying vegetation, are all the kinds of things Gibson had in mind when he stressed the direct and active nature of perception. This is a sequence of perceptual acts which do not involve object manipulations and which result in completely conscious percepts. In addition, constructivist approaches invariably propose that we are only aware of some of the representations we activate in a long sequence of processing (the last ones in the chain, typically), so awareness is a dubious distinction between the approaches, in any case.

The other important misrepresentation of the direct approach is the claim that information pickup is largely an unlearned process. It must be noted that far from viewing learning as a peripheral aspect of perception, the direct theorist places learning at the centre of any explanation of human behavior. For example, whether or not a gap can be jumped over depends on how big the jumper is, the strength of their legs, whether they are riding a bicycle, and various other factors. Obviously, the perception of what the gap affords must be learned, if only because that changes as the perceiver grows.

In a footnote, Norman grants the dorsal system procedural memory, but no "representational memory." This is fine for the direct theorist, but only because she considers all memories as procedural rather than representational (a position which echoes the proceduralist stand of memory theorists, such as Crowder 1993). The direct approach to learning is to suggest that it involves "tuning in to" properties of the world – acquiring sensitivities to situations or relationships in the world. There are brain changes associated with these new sensitivities, but these changes don't "represent" the external situations, they merely give the perceiver a new sensitivity. This is less liable to be misinterpreted if we use a simpler, nonneural, example. Lifting lots of heavy weights causes changes to muscular tissue. These changes give the weight-lifter new abilities – they can now lift previously unliftable weights. They stand in a new relationship to certain (heavy) objects in the world (they have a new "sensitivity," in one sense). Importantly, there is no sense in which the changes in the musculature "encode" the events that lead to them. Note the similarity between this example and the learning of connectionist models; changes in muscle fibres and connection weights are, to our minds, the same general kind of change. One hallmark of this view is that it is clear what is happening when learning occurs, unlike the state of affairs following the "encoding of a new representation."

We are in general agreement with Milner and Goodale (1995) (and, to some extent, with Norman) that the major difference between the ventral and dorsal visual pathways is one of function. However, we state that functional distinction in slightly different terms. According to Norman, the representation-less dorsal stream is implicated in reaching behavior, whereas the ventral stream mediates object recognition through long-term representations. Presumably, this distinction is based on the belief that an object's size is a property of the object (which can therefore be detected directly), but its identity is a relational property (it is a member of some class of objects). Of course, in order to grasp an object, its size must be known relative to the observer, and so this is a relational property too, but it is a different kind of relationship. Since both these kinds of relationships are properties of the world, a direct theory has no problem proposing that the recognition and grasping of objects are both a function of sensitivity to relational properties. The dorsal stream appears to be sensitive to person-relative properties of the world (egocentric relationships), and the ventral stream appears to be sensitive to object-relative properties of the world (allocentric relationships). For example, even if it could be unequivocally shown that when a person reaches for a disc embedded in an illusory context they do so accurately, but when judgements are made of the disc using the ventral system they are influenced by the illusion, this would be perfectly explicable from an entirely direct perspective. Such an explanation would only need to propose that the dorsal system is sensitive to the "absolute" diameter of the disc but the ventral system is only sensitive to its relative diameter. It is sensitivity to relative size that generates the illusion. In our view, understanding of the functioning of the ventral stream will come from more careful consideration of the allocentric relationships in the world to which human perceivers become sensitive. That understanding, however, will be best considered in terms of the direct relationship between the human perceiver and the world which is perceived.

Conceptual space as a connection between the constructivist and the ecological approaches in a robot vision system

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Abstract: The *conceptual space* (Gärdenfors 2000) is discussed as a representation structure that connects the *constructivist* and the *ecological* vision subsystems in an operating autonomous robot based on computer vision.

The two vision subsystems discussed by Norman, based on the *constructivist* and the *ecological* approaches, have an immediate counterpart in the design of robotic architectures based on computer vision. On the one side, the *ecological* approach is adopted to design robot behaviors that reactively connect the information acquired by cameras and other sensors to robot actions, as in the case of obstacle avoidance, path following, and orienting the robot towards a goal (see Arkin 1998).

On the other side, the *constructivist* approach is adopted to design the object recognition system of the robot, that is, the high-level vision algorithms that let the robot identify and recognize the objects on which it needs to act in its working environment. In general, a robot object recognition system generates 2D/3D observer-independent reconstruction of the objects in the perceived scene. It comes out from information-processing tasks that receive raw and low structured information (the data acquired by cameras) as input, and give as outputs highly structured data, which are then employed for identification and recognition (see, e.g., Ullman 1996 and Edelman 1999). Several proposals are described in the literature, about the connections of the two subsystems in operating robots (see Kortenkamp et al. 1998 for a review).

The autonomous robot operating at the Robotics Laboratory of the University of Palermo (a RWI B21 equipped with stereo head) connects the two subsystems by adopting a theoretically motivated approach based on the *conceptual space* (CS – Gärdenfors 2000). A CS is a metric space whose dimensions are related to the quantities processed by the robot sensors. Examples of dimensions could be colour, pitch, volume, spatial coordinates. In any case, dimensions do not depend on a specific linguistic description: A generic conceptual space comes before any symbolic-propositional characterization of cognitive phenomena.

A *knoxel* is a point in the conceptual space and it represents the epistemologically primitive element at the considered level of analysis. In the implemented vision system (Chella et al. 1997), in the case of static scenes a knoxel corresponds to a *geon*-like 3D geometric primitive, that is, a superquadric (Pentland 1986). It should be noted that the robot itself is a knoxel in its conceptual space. Therefore, the perceived objects, like the robot itself, other robots, and the surrounding obstacles, are all reconstructed by means of superquadrics and they correspond to suitable sets of knoxels in the robot's CS. Some dimensions of the CS are related to the knoxel's shape (the length of the axes and the shape factors), that comes out from the robot's *constructivist* subsystem, while other dimensions are related to the displacement in space of the knoxel (the position of the center and the orientation of the axes) and they comes out from the robot's *ecological* subsystem, in Norman's terms. The conceptual space is therefore a result of the connection of the two subsystems and it contains all the information needed for the robot to describe the represented objects in symbolic terms, and to act in its contemporary environment (Chella et al. 1998).

To account for dynamic scenes, the robot CS is generalized to represent moving and interacting entities (Chella et al. 2000). In this case, an intrinsically *dynamic* conceptual space is adopted. Simple perceived motions are categorized in their wholeness, and not as sequences of static frames. In other words, simple motions of superquadrics are the perceptual primitives for motion perception. According to this hypothesis, every knoxel corresponds to a simple motion of a superquadric, expressed by adding suitable dimensions in CS that describe the variation in time of the knoxel. For example, considering the knoxel describing a rolling ball, the robot's dynamic conceptual space takes into account not only the shape and position of the ball, but also its *speed* and *acceleration* as added dimensions (Marr & Vaina 1982). So, when the robot chases the rolling ball, it represents this action in its dynamic CS as a set of two knoxels, corresponding to the moving ball and the chasing robot itself. In this case also, the dynamic conceptual space is a result of the connection of the two subsystems proposed by Norman.

This new conceptual space allows the robot to represent and recognize dynamic scenes; in particular, the scenes in which the robot moves itself in a dynamic environment. In this case, the behaviors of the *ecological* subsystem receive feedback and control from the CS during their own operations. The feedback is employed to monitor the operations of the behaviors to obtain satisfactory performances. This is another example of the connections between the two subsystems described by Norman.

The dynamic CS representation lets the robot *anticipate* possible future interactions with the objects in the environment (Gärdenfors 1997). In fact, the interaction between the robot and a generic object (e.g., the ball previously decribed) is represented as a sequence of sets of knoxels in CS. This sequence can be imagined and *simulated* in the robot's CS before the interaction actually happens in the real world. In the implemented robot system, the imagined sequence of knoxel sets is recalled by a recurrent neural network (Elman 1990) receiving as input the knoxels describing the robot in the current environment. For example, when the robot perceives the quiet ball, it can imagine bumping it, or, when the robot perceives the ball that rolls, it can imagine stopping it. Therefore, the CS may represent simple forms of objects *affordances.*

Moreover, the rolling ball may disappear from the robot field of view because of an occluding obstacle. In this case, the robot represents the ball's trajectory in its CS by the associative mechanism previously outlined, and it anticipates the ball's future positions. Also in this case, the CS representation is usefully employed to suitably drive the behaviors of the *ecological* subsystem of the robot to catch the ball. In this sense, the CS allows for the description of some forms of high-level, *conceptual affordances* that allow the robot to represent immediate action plans.

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Perception, learning, and judgment in ecological psychology: Who needs a constructivist ventral system?

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Abstract: Norman's identification of a ventral system embodying a constructivist theory of perception is rejected in favor of an ecological theory of perception and perceptual learning. We summarize research showing that a key motivation for the ventral-constructivist connection, percept-percept coupling, confuses perceptual and post-perceptual processes.

Milner and Goodale (1995) presented solid evidence that the anatomically-defined ventral and dorsal cortical streams may be necessary to perform certain functions. In his target article, Norman goes a step further and uses these streams as a basis for suggesting the existence of two theoretical entities, dorsal and ventral *systems.* The functions of these two hypothetical systems parallel the ecological and constructivist theories of perception. It is our position that this stance is unjustified (see also, Michaels 2000; Michaels et al. 2001). We maintain that ecological psychology provides an approach to all perceptual phenomena, including identification and recognition, and that the constructivist theory of perception is, quite simply, not a theory of perception at all but a confusion of perceptual and postperceptual processes. In our commentary, we briefly outline our view on three phenomena that motivated Norman to relegate ecological psychology to the dorsal system and to advocate a constructivist theory of perception in the ventral system. These phenomena are learning, judgment, and (apparent) percept-percept coupling.

Norman suggests that because ecological psychology lacks memory representation, it cannot account for the changes in perception that accompany experience. In ecological psychology, learning is not conceived as storage, but as the education of attention to variables that specify a to-be-perceived environmental property. We grant that much research by ecological psychologists has emphasized expert perception, but the education of attention is a key concept both in development (for a recent summary, see Gibson & Pick 2000) and the study of adults' performance in novel situations. For example, perceivers who are asked to judge kinetic properties specified in optical displays (relative mass of colliding balls or the force with which a human pulls) often do not start by detecting a variable that specifies the to-be-reported property (Jacobs et al. 2000; Michaels & de Vries 1998; Runeson et al. 2000). Instead, they appear to rely on lower-order variables and to converge on the more informative variables only when given practice with feedback. That is, they educate their attention to optical variables that specify mass ratio or pulling force. Identifying a mass ratio of 2:1 or a pull of 250 N means that one has learned to pick up information that specifies mass ratio or pulling force. The education of attention is obviously learning, so it is only memory-as-stored-representation to which we object. We do not believe that one needs a qualitatively different theory to explain the education of attention to information that specifies that a bird is a chickadee or that a person is Fred. Identifying a chickadee or recognizing Fred is evidence that the education of attention has occurred; it is only spooky assumptions about time that entail the storage metaphor (cf. Michaels & Carello 1981).

We cited the above experiments in part because they involve making judgments, a putative ventral stream function in Norman's scheme. However, we differ from Norman by claiming that judgment is based on perception, not part of it. What is observable to the scientist is behavior – the participant can act on an environmental configuration or report what was perceived. Both types of observables are assumed to be a consequence of perception. In his section 5.2.2, Norman provides evidence of percept-percept coupling that favors the constructivist-ventral connection, but he and many of the authors he cites do not make a distinction between judging and perceiving. Equating reports of perception (i.e., judgments) with perception itself is an error that has led many investigators to conclude that percept-percept couplings occur. Traditionally, covariation, correlation and partial correlation have been used to investigate percept-percept coupling. These methods provide only a weak test of perceptual independence for two reasons: (1) they make no formal distinction between perceptual and judgmental process; and (2) while two independent variables are not correlated, uncorrelated variables need not be independent. Recent multivariate extensions of signal detection theory have addressed these limitations and established a method that makes a clear distinction between perception and judgment; and they provide a rigorous statistical definition of independence (Ashby & Townsend 1986).

The paradigmatic case of percept-percept coupling is the size-distance invariance hypothesis; it claims that a given visual angle determines a unique ratio of perceived size to perceived distance (Kilpatrick & Ittelson 1953). Cooper (1999) used the statistical advances of Ashby and Townsend (1986) to test the validity of the size-distance invariance hypothesis. In his experiment, observers viewed spheres (1.3, 1.5, and 1.7 cm diameter) placed at 3 viewing distances (780, 805, and 830 cm) under either monocular or binocular viewing conditions. Observers verbally reported both size and distance on each trial. While the analysis is too complex to describe here, the results clearly showed that size and distance are perceptually independent. This means that the perception of one dimension was in no way contingent upon or interacted with the perception of the other. These results suggest an alternative interpretation of the research Norman cites as evidence for the coupling of perceived size and distance in the ventral system. Namely, the apparent percept-percept coupling is due to post-perceptual decisional process, and not to an (unconscious) inferential process as Norman and the constructivists would argue.

We hope that, together, our comments suggest: (1) that perceptual learning, under the heading of the education of attention, is needed and expected in ecological psychology, and does not entail representations or comparisons; (2) that perception may or may not be faithfully preserved in the post-perceptual process of judgment, without impugning the perception on which judgment or inference is based; and (3) that the perceptual independence of size and distance refutes some of the evidence that Norman claims as support for a constructivist mode of perception in the ventral system.

Invariants and cues

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Abstract: The concepts of invariants and cues are useful, as are those of dorsal and ventral streams, but Norman overgeneralizes when interweaving them. Cues are not confined to identification tasks, invariants not to action, and both can be learned.

Norman has organized disparate metatheoretical views – one centrally represented by Gibson, the other by selected views of Helmholtz and of Rock – in terms of emergent binary schemes in neurophysiology over the last thirty years. From the latter, Norman has chosen the anatomical terms of dorsal and ventral, tried to make them functional, and provided an overview of several classes of data. The attempt at synthesis is laudable, but overgeneralization is its danger, and inconsistency a major cost. I note, in passing, that the perceptual evidence for a dorsal/ventral distinction is not always clear (Vishton et al. 1999), but I shall consider Norman's suggestion about dorsal invariants and ventral cues for the perception of space, or layout.

Gibson's goal was to understand and explore the general trustworthiness of perception. Railing against the centuries of claims about perception's faultiness – often couched as the *argument from illusion* – Gibson suggested that organisms surviving in real environments – what can be called the *argument from evolution* (e.g., Cutting & Vishton 1995) – made it extremely unlikely that perception is systematically faulty. He embraced invariants as the reason for perception's trustworthiness, but without a lot of evidence.

Two of Gibson's invariants that Norman mentions are texture interception and the horizon ratio. Unfortunately, these are invariant only under very restricted circumstances. Gillam (1995) noted that the texture interception is not invariant for any vertical objects occluding texture lying on or near a ground plane. Consider an example. Any two identical objects that are one eye-height tall and at different distances will have tops that intercept the horizon and will cover different amounts of ground texture, in part because their bases occlude different amounts of texture nearest the observer. Gibson's rule is true only for flat-lying objects occluding flat-lying texture. The horizon ratio is similarly constrained. That is, given two identical objects at different depths intercepting a true horizon, the ratio of the proportion above to that below is the same (invariant) for both. But, as Sedgwick (1986) noted, this is true only for objects that are small relative to their distance, that are identical in height, and for object bases and an observer's feet that are coplanar. The stringency of these conditions suggests that neither invariant is likely to get one very far, dorsally speaking. Although a number of perceptual invariants have indeed been found – and some are about identifying objects (e.g., a rigid, toppling ladder; Cutting 1986), not about action – it seems unlikely that they govern all of perception coupled to action (Cutting 1993).

Norman suggests that invariants and cues engage different neural systems, invariants invoking hardwired, innate processes and cues demanding algorithmic, inferential processing. Perhaps. But consider research on the perception of one's heading (direction of locomotion), a domain where yet another Gibson invariant – the focus of expansion – has little currency, except at high speed (Cutting 1986; 2000). Consider further, an invariant and a cue (Best et al. 2002; Cutting et al. 2000; Cutting & Wang 2000; Wang & Cutting 1999). During locomotion, the convergence in the field of view of any two stationary objects at different distances specifies that one's heading is to the outside of the nearer object. The accelerating divergence of any two such objects, however, specifies nothing – 69% of the time heading is to the outside of the farther object, 22% of the time between them, and 9% to the outside of the nearer object. From computer simulations of travel through modestly cluttered environments, it is clear that observers can use both of these sources of information for heading judgments, although they spend more time looking at members of an invariant pair, when available, than at those of a cue pair. Are completely different neural mechanisms used in the two cases? – innate for the convergence invariant and algorithmic for accelerating divergence cue? I think not. Following E. J. Gibson (1969), I suggest these information sources are experienced by the young traveler – one found very trustworthy (convergence), the other less so (accelerating divergence) – and differentiated through that experience. Why hardwire either from birth?

In summary, cues are not just "ventral," and invariants not just "dorsal." Moreover, invariants are probably too rare to govern all of action, and some are likely to be learned.

Ecological and constructivist approaches and the influence of illusions

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Abstract: Norman tries to link the ecological and constructivist approaches to the dorsal and ventral pathways of the visual system. Such a link implies that the distinction is not only one of approach, but that different issues are studied. Norman identifies these issues as perception and action. The influence of contextual illusions is critical for Norman's arguments. We point out that fast (dorsal) actions can be fooled by contextual illusions while (ventral) perceptual judgements can be insensitive to them. We conclude that both approaches can, in principle, be used to study visual information processing in both pathways.

The visual system has two main pathways for processing visual information: the ventral and the dorsal. Color, texture, and shape are primarily analyzed in the ventral pathway, while motion and egocentric position are analyzed in the dorsal pathway (Mishkin et al. 1983). More important for Norman's distinction, the ventral pathway is believed to consider contextual information, while the dorsal pathway is believed not to do so. This difference in processing contextual information is what distinguishes ecologists' invariants from constructivists' cues and constancies. Thus, the influence of illusions, which often arise from misinterpreting the context, can be considered critical for this debate. Many studies have compared information processing in the dorsal and ventral pathways by comparing the influence of illusions in perceptual and motor tasks. In perceptual tasks, assumed to be processed by the ventral system, illusions obviously show an influence on the measured variables (otherwise, they would not be illusions). In motor tasks, assumed to be processed by the dorsal system, often no influence is found.

However, although they have received less attention, many experiments show that motor tasks can be influenced by illusions. When hitting a moving target with one's hand, a moving background can lead to changes in the hand's speed (Smeets & Brenner 1995a) and in the hand's direction (Smeets & Brenner 1995b). Bridgeman et al. (1997) studied pointing movements towards a target within a frame. For half their subjects, an offset of the frame from the subject's objective median plane caused a bias in the pointing movements in the opposite direction. Brenner and Smeets (1996) demonstrated that the force exerted to lift an object is influenced by the Ponzo illusion. Jackson and Shaw (2000) found the same for grip force. Yamagishi et al. (2001) showed that pointing movements towards a small window with a moving grating displayed behind it were biased in the direction of the grating's motion. These experiments show that when performing motor tasks, as used by the ecologists to study invariants, subjects can be fooled by illusions.

Individual illusions also do not influence performance in all perceptual tasks. Smeets and Brenner (1995a) showed that background motion influences the perceived motion of a target, but not the perceived position. Similarly, the Müller-Lyer illusion influences perceived size but not the perceived positions of the endpoints (Gillam & Chambers 1985). Vishton et al. (1999) showed that the horizontal-vertical illusion is reduced considerably if the perceptual judgement is an absolute judgement of a single element of the display instead of a relative judgement of two elements. Similar results were obtained for the Ebbinghaus illusion

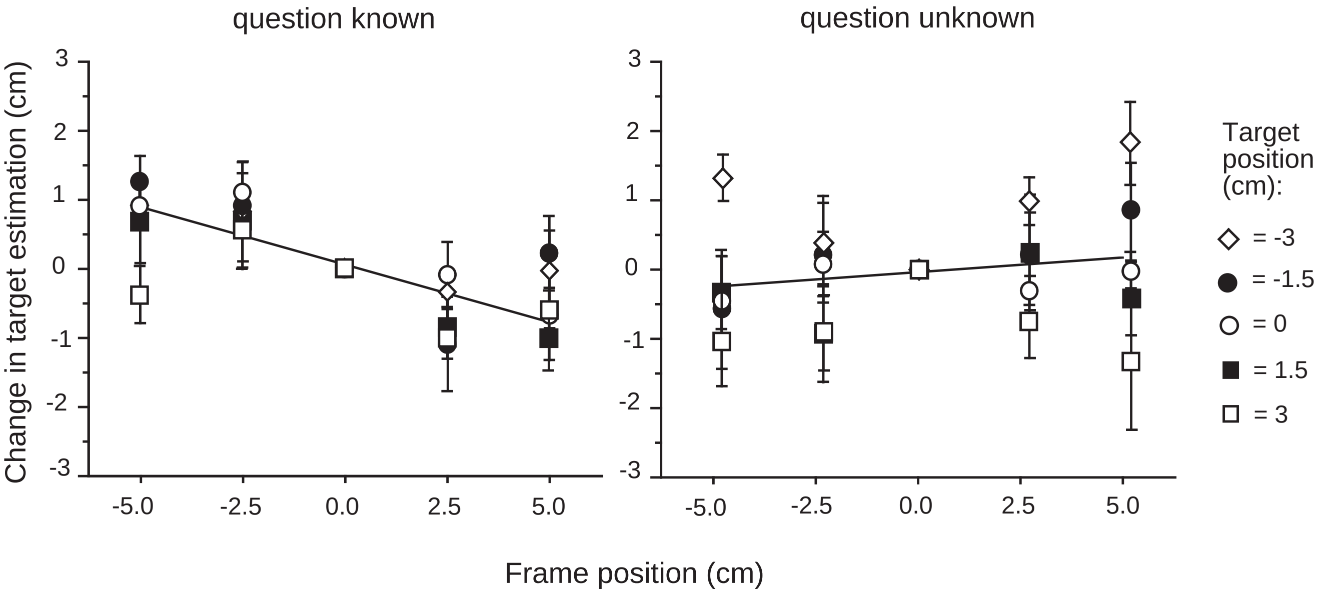

Figure 1 (de Grave et al.). Target estimation when the question was or was not known in advance. The values show the difference between the estimated position with the frame straight ahead and the estimated target position when the frame was shifted 2.5 or 5 cm to the left (negative) or right (positive). Error bars represent standard errors between subjects.

(Pavani et al. 1999) and the Müller-Lyer illusion (Franz et al. 2001). These experiments show that when performing perceptual tasks, as used by constructivists to study cues, the effect of the illusion can be absent.

Hence, whether an illusion affects a task does not depend on whether the task is an ecologist's motor task or a constructivist's perceptual task, but largely on the question asked or variable studied. However, the influence of illusions is not even fixed within a single experimental paradigm for a single question. We showed this recently using a constructivist's paradigm based on the induced Roelofs effect (de Grave et al. 2002). Subjects were presented a target within a frame in complete darkness. Target and frame could both be shifted to the left or right of the objective straight ahead. Subjects gave verbal estimates about the position of either the target or the frame. In one condition, subjects knew prior to stimulus presentation that they would be questioned about the position of the target. In another condition, they had no prior knowledge whether the question would be to respond to the position of the target or to the position of the frame. In the "question known" condition the perceived position of the target followed the misjudgement of the eccentricity of the frame (the induced Roelofs effect). But in the "question unknown" condition, the illusory effect was not present (Fig. 1).

We argue that the illusory influences on both perception and action depend on the aspect of the task that is studied and on the circumstances under which this is done. Since contextual illusions are generally linked to the ventral stream, the ecological and the constructivist approach cannot correspond with the dorsal and the ventral pathway, respectively.

Evolutionary and intellectual antecedents of primate visual processing streams

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Abstract: The main function of vision in many animals is to control movement. In rodents, some visuomotor acts require the construction of models of the external world while others rely on Gibsonian invariants. Such findings support Norman's dual processing approach but it is not clear that the two types of processing rely on homologs of visual processing streams described in primates.

Norman attempts to draw the sweep of phylogeny into his view of perception and action by mentioning some seminal findings from old experiments involving nonprimates. It is accurate to characterize Schneider's hamsters and Ingle's frogs as the *intellectual* predecessors to Milner and Goodale's DF, as both of these sets of studies were strong influences on the "two cortical visual systems" idea. One of my interests has been in asking whether there is an *evolutionary* relationship as well as an intellectual one between the organization of cortical visual streams in primates and the simpler visual systems of other animals. Is there anything like a ventral stream in a rat and, if so, what is it for? This is a question that has troubled me for some time, as most descriptions of the primate ventral stream are steeped in discussions of awareness and viewpoint-independent object recognition. In contrast, my own experiments, like those of many others, have suggested that the main function of vision in rodents is to control action directly, rather than to produce abstract representations of the external world (Ellard 1998; Goodale & Carey 1990). When I read Norman's paper, though, it organized some inchoate thoughts that had been sloshing around in my mind about some of my own experiments involving the visual control of running and jumping in the Mongolian gerbil (Ellard & Shankar 2001).

Gerbils can be trained to jump long distances with great accuracy (Ellard et al. 1984), and one of the most potent sources of information that gerbils use to estimate such distances is retinal image size (RIS) (Goodale et al. 1990). In order to use RIS, gerbils need to learn a calibration between the size of the proximal image and its distance. Not only do gerbils appear to learn such calibrations very quickly, but they can learn to keep a kind of catalog of such things for multiple objects and they can rapidly and effectively update the catalog in light of feedback (see Ellard & Goodale 1991; Ellard & Shankar 2001 for reviews). These findings suggest that gerbils in these tasks are constructing a model of the external world on the basis of the outcomes of visuomotor interactions with that world. Not only is the model liable to modifications depending on the success with which it is applied, but it is applied in slightly different ways depending on prevailing conditions. For example, when RIS is rendered less reliable by making object size more variable, it may still be used to compute distance but it will make a smaller contribution than when object size is stable. Gerbils are constructing a modest model of the external world.

Gerbils can be trained to run towards a visual target and to brake effectively so as to avoid hitting the target. As was first suggested in Goodale's lab (Sun et al. 1992) and confirmed in later experiments in my lab (Shankar & Ellard 2000), they are probably using a Gibsonian invariant called time-to-collision (TTC) to time braking in this task. TTC relies on the ratio between the proximal size of a target and its instantaneous rate of change as it is approached (Lee 1976), and so can be used even when the distal properties of the target are unknown. Unlike the case for jumping, the information that is used to compute braking time in the running task is not prone to the influence of experience. For example, presenting misleading TTC information (by changing target size as the animal runs towards it) does not influence the performance of gerbils on subsequent trials (Ellard & Blais, in preparation). These experiments are compatible with the ecological view of perception.

I think it is possible to imagine how both ways of using visual information (constructing models of the world and using Gibsonian invariants) can contribute to an animal's ability to navigate through space. Animals may move from place to place largely under open loop control, relying on path integration and a set of invariants like TTC. Between such movements they may update their locations by taking "fixes" that rely in part on stored information about allocentric space.

It would be nice if I could conclude my commentary by saying that there was also an anatomical correspondence between the dorsal and ventral streams in gerbil cortex and the running and jumping abilities that I have described, but, alas, I cannot. Large lesions of temporal cortex have no effect at all on RIS in gerbils, and lesions of parietal cortex can be shown to produce effects that mirror object recognition deficits (Ellard & Sharma 1996). We know little about the neural substrates involved in computation of TTC in the gerbil, but it appears as though a small "dorsal" cortical area may play an integral role (Shankar & Ellard 2000). This may only mean that our knowledge of rodent cortex is not advanced enough to make the proper comparisons, but my hunch is that the differences are simply too great to make much of a case for a parallel between rodent and primate visual cortical streams.

What impact does this have on Norman's hypothesis? For one thing, if there is no real homolog to the ventral stream in rodents it means that the evolutionary antecedents for different modes of perception preceded the anatomical parcellation. On the other hand, if gerbils are constructing allocentric models of the world using a procrustean version of the ventral stream that has yet to be identified, it might help to point us in the right direction to find it. It might also help us to understand ventral stream function in a way that can be characterized without reference to consciousness and rumination. I would find it satisfying if the evolutionary roots of both streams were to be related to the kinds of problems for which vision first arose – moving one's body through space.

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A fast ventral stream or early dorsal-ventral interactions?

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Abstract: Several lines of evidence indicate that rapid target-aiming movements, involving both the eyes and hand, can be biased by the visual context in which the movements are performed. Some of these contextual influences carry-over from trial to trial. This research indicates that dissociation between the dorsal and ventral systems based on speed, conscious awareness, and frame of reference is far from clear.

Norman should be applauded for his attempt to reconcile two very different theoretical approaches to visual perception by drawing on recent neurophysiological and behavioural evidence delineating the functional characteristics of the dorsal and ventral visual pathways. Although we agree that a compromise between ecological and constructivist thinking may be needed to explain the variety of visual-motor and visual-cognitive behaviours performed by humans, we are less convinced that the dorsal-ventral dichotomy is the best theoretical vehicle for achieving this compromise.

One of the most compelling lines of evidence for dissociation between the dorsal and ventral stream in the intact brain is the work on the relative susceptibility of the human performer to visual illusions in different response or decision-making contexts. Although it appears that people are less influenced by characteristics of the visual surround when reaching toward an object (i.e., body-scaled egocentric frame of reference) than when making a more cognitive judgment about the size of an object (i.e., allocentric frame of reference), it has been demonstrated that the very rapid eye movements are susceptible to the visual context in which a target object is placed. For example, it is known that eye movements between the vertices of the Müller-Lyer illusion undershoot the outward-pointing form of the illusion (<—>) and overshoot the inward pointing form (>—<) (see Coren 1986 for a review). This occurred even when the participants were explicitly instructed to hit the vertices with their eyes (Binsted & Elliott 1999b) and regardless of whether the eye movements were made to visible or remembered Müller-Lyer figures (Binsted & Elliott 1999a; see also Binsted et al. 2001). This bias was evident in both the initial saccade to the target and the end position of the eyes after one or more "corrective" saccades. Like a number of studies reviewed by Norman, hand movements made to the same Müller-Lyer targets were only biased when aiming depended on the remembered target position. Thus, unlike hand movements, eye movements exhibited a pattern of movement more associated with an allocentric frame of reference (i.e., ventral stream), than a body-scaled frame of reference (i.e., dorsal stream). This was in spite of the fact that the eye movements were much faster than the hand movements and presumably more automatic (i.e., less conscious control). It is our view that the dissociation between the susceptibility of eye and hand movements to illusions has more to do with the type of closed-loop control available to the latter but not the former effector system, rather than a ventral-dorsal dissociation per se. Perhaps performing accurate hand movements to an illusory target position requires rapid switching between ventral (i.e., allocentric) and dorsal (i.e., egocentric) processing (see Glover & Dixon 2001).

Although hand movements have been shown to be less susceptible to visual context than cognitive judgments, there are situations in which the environmental context in which a movement unfolds affects movement accuracy. In two recent experiments, Proteau and Masson (1997) have demonstrated that the on-line control of rapid aiming movements can also be influenced by the visual background against which the movements are performed (see also Masson et al. 1995). In a computer-aiming task, participants applied pressure to a force transducer with their fingers in order to move a cursor from a home position to a target on a computer screen. Upon movement onset, the contextual features in the visual background began to move in either the direction opposite to the cursor or in the same direction. In the former case, this "contextual flow" gave the impression of the cursor moving at a higher speed than it really was. Compared to control trials, in which there was no moving background, participants terminated their movements earlier. This resulted in target undershooting. When the background was moving in the same direction as the cursor the opposite result was obtained on the first few illusory trials. If one argues that the indirect nature of the aiming task required ventral stream involvement, it follows that the ventral stream can operate very rapidly.

One apparent distinction between the dorsal and ventral stream is that dorsal processing depends on the direct pickup of visual information from the environment while the ventral stream processing can be memory-driven. With this in mind, it is interesting that both rapid eye (Binsted & Elliott 1999b) and hand movements (Elliott & Lee 1995) are susceptible to "range" or "context" effects. That is, when participants are asked to make target-aiming movements of several different amplitudes within the same block of trials they undershoot the target with movements to far targets and overshoot the target when moving to near targets. This type of context effect is thought to be the result of perceptual and/or motor averaging (i.e., regression toward the mean) within a set of perceptual-motor events that vary in magnitude (Pepper & Herman 1970). At some level, this averaging requires a memory for past events that lasts at least several minutes. For both the eyes (Binsted & Elliott 1999b) and the hand (Elliott & Lee 1995), these contextual biases in aiming are independent of the bias induced by the Müller-Lyer illusion. Thus automatic/unconscious perceptual-motor performance appears to be affected by both the environmental context within a specific movement attempt (i.e., the Müller-Lyer findings and moving background), and the context across a number of movement trials (i.e., the range effect).

In terms of the two visual systems dichotomy, it would appear that either the dorsal stream function is not strictly egocentric and can be influenced by environmental context, or that the ventral stream has a role to play in the on-line control of aiming movements. Certainly, memory appears to play a role in even very rapid, unconscious perceptual-motor behaviours. In any event, recent empirical work indicates that issues such as speed of processing, frame of reference and consciousness need to be revisited in Norman's two visual systems, Helmholtzian-Gibsonian theoretical framework.

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Constructivist and ecological approaches in tactual perception

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Abstract: Constructivist and ecological approaches are also observed in tactile perception studies. The question is whether identification and localization are dissociated in the tactile modality as well, and whether Norman's conception may be generalized to the field of touch. An analogue to blindsight was evidenced in passive touch, but no such dissociation was observed in active touch. A study is in progress in this domain.

In the target article, Norman's tentative reconciliation between the constructivist and the ecological approaches in perceptual studies concerns visual perception only. His thesis is mainly based on the fact that the two distinct visual systems, the ventral and the dorsal, have different functions which are compatible with the psychological distinction between a constructivist and an ecological approach of visual perception.

But these two major theoretical conceptions of perceptual processes are not limited to vision in the literature. The same dichotomy between constructivist and ecological approaches appears in touch studies too (for reviews, see Hatwell et al. 2000; Heller 2000; Lederman et al. 1996). A constructivist approach is found, for example, in Gentaz and Hatwell (1996; 1999), Heller et al. (1997), Klatzky (1999), Lederman and Klatzky (1987), Luyat et al. (2001), Millar (1994), and Streri (1993). In some of these studies, exploratory movements of the hand are intentionally produced in order to acquire information about objects in an environment. This tactual-kinesthetic perception (labeled haptic) is described as relying in a large part on inferential processing, that is, on information beyond that found in the sensory stimulation (mainly in visual imagery and memory). On the other hand, and as is noted by Norman, the ecological approach in the research on touch is essentially represented by Turvey and his associates (for a review, see Turvey 1996). In these studies, blindfolded subjects were able to tell much about an object's shape or length by wielding it (Carello et al. 1992; Pagano & Turvey 1992). According to Turvey (1996), the abilities of "dynamic touch" would result from the sensitivity of the body tissues to certain quantities of rotational dynamics about a fixed point that do not vary with changes in the rotational forces and motions. Thus, the muscles are viewed as measuring instruments which are able to detect physical invariants, without having recourse to inferential process.

The question, therefore, is whether Norman's thesis will hold when tactual perception is considered instead of vision. An affirmative answer to this question requires the demonstration of the existence of two distinct tactual systems having specific functions similar to those described by Norman for visual perception.

In passive touch, where no movements are executed by the observer who is passively stimulated, some observations showed that a double dissociation analogous to blindsight may occur (see Rossetti 1998 for a review). A patient with a left parietal thalamo-subcortical lesion was studied in search for residual processing in the somesthetic modalities (Rossetti et al. 1995). The patient was unaware of any kind of cutaneous stimuli applied to his arm, and failed to demonstrate any significant performance in a verbal forced-choice paradigm. However, he demonstrated a successful performance when pointing at the cutaneous stimulus location on the numb arm. A similar observation was reported by Paillard et al. (1983), who described a tactual equivalent of blindsight. Rossetti et al. (1995) questioned whether the residual ability of the patient was linked to the mode of response (motor vs. verbal) or to the representation subservient to these responses (motor vs. symbolic). Thus, the patient failed to point correctly to the stimulus location on an arm picture. This dissociation indicates that only a representation of the stimulus linked to the body scheme was preserved, whereas more elaborate representations of the stimulus had disappeared. Moreover, the patient was unable to localize verbally where his right index finger was passively positioned in a horizontal plane, but was successful in pointing to this finger with the left hand. This result shows that "blind-touch" can also be generalized to proprioception. The residual abilities reported in this patient suggest that only a specific sensori-motor pathway was left intact following the stroke. This direct sensori-motor pathway would specifically allow for movements made toward a stimulus, but is inefficient as soon as a symbolic representation of the stimulus is required and/or produced.

To our knowledge, no such dissociation has ever been observed in active touch (haptics), and we wonder whether it could be evidenced in it. It may be impossible to observe this dissociation in haptics because the hand is both the motor system by which reaching and grasping are carried out, and the exploratory perceptual system through which spatial information is picked from the environment. We are nevertheless currently studying this question by investigating the effect of a haptic illusion on grasping. Our aim is to know whether haptic perception is more sensitive to the illusion than the "haptic-motor" grasping behavior, as it is the case for visual illusions (Aglioti et al. 1995; Gentilucci et al. 1996). In the Müller-Lyer illusion, a line ended by two arrows oriented either toward its center or away from its center is judged shorter or longer (respectively) than it is really. This illusion is a good candidate for our study because it exists also when blindfolded sighted or congenital blind participants explore haptically a 3D stimulus (Patterson & Deffenbacher 1972; Rudel & Teuber 1963; Suzuki & Arashida 1992) and it is sensible to the same factors in vision and haptics (learning, angles of the arows, etc; for a recent review see Gentaz & Hatwell, 2002).

Therefore, after a haptic exploration (with the index) of this figure, we are asking blindfolded sighted participants to grasp the 3D-line between the thumb and the index finger and we measure their grip size during reaching (before contact). If the grip size is determined by the true size of the line to be grasped and not by its illusory perceptual size, this would mean that the classical dissociation between localization and identification exists in haptics too. In this case, Norman's tentative reconciliation between the constructivist and the ecological approaches in visual studies could be extended to tactual studies. By contrast, if such dissociation cannot be evidenced in active touch, the reconciliation of the two major theoretical conceptions will seem difficult to propose in tactual studies. Our further results will help to answer these questions.

Recognising actions

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Abstract: The ability to recognise the actions of conspecifics from displays of biological motion is an essential perceptual capacity. Physiological and psychological evidence suggest that the visual processing of biological motion involves close interaction between the dorsal and ventral systems. Norman's strong emphasis on the functional differences between these systems may impede understanding of their interactions.

Norman follows Milner and Goodale's (1995) theory of two visual systems in making a strong distinction between two functions of vision: the immediate control of action, and a broader category that includes recognition and conscious awareness. It is interesting that he refers to a three-way classification (Neisser 1994) that adds a third function of visual perception, the "interpersonal perception," involved in our "immediate social interaction" with others. We believe that existing knowledge of basic visual processes involved in interpersonal perception requires the theory of two visual systems to be developed in a quite different way from that proposed by Norman. In particular, it points to the importance of analysing how the dorsal and ventral systems interact with one another.

It is well known that one basic component in social perception is the processing of biological motion. Research on the perception of point-light displays and other forms of animation has demonstrated a remarkable sensitivity to spatial and temporal properties of human movement, which is based on mechanisms specialised for its analysis (e.g., Neri et al. 1998). In terms of the theory of two visual systems, where are these mechanisms located? On the one hand, the processing of complex patterns of image motion is usually regarded as a function of the dorsal pathway. On the other, the perceptual judgements supported by biological motion stimuli presumably arise from ventral processes. For example, point-light displays of biological motion can support recognition of affect (Pollick et al. 2001) or of individual people, and this recognition can be enhanced by exaggerating temporal parameters of the display, in the same way as "caricaturing" of faces (Hill & Pollick 2000).

Specialised processing of biological motion could be explained within Milner and Goodale's theory by allowing complex motion processing in the ventral as well as the dorsal pathway. However, physiological evidence suggests instead that it arises from interaction between the two systems. Cells in the superior temporal polysensory area of macaques are selective for complex properties of biological motion depicted in full or in point-light displays (Oram & Perrett 1994), and this area has connections to others in both the dorsal and ventral pathways (Baizer et al. 1991). Neuroimaging data suggest an area in human superior temporal sulcus (STS) that is selectively activated by biological motion displays (Grossman et al. 2000). This is accompanied by activation in other cortical areas, in patterns that are influenced by the nature of the task in which perception of motion is embedded. Greater activation is observed in dorsal areas if the observer's task is to imitate a movement, but in ventral areas if the task is recognition (Decety & Grezes 1999). These differences suggest that different sets of areas are recruited to different tasks, following initial processing of biological motion through an early interaction between dorsal and ventral areas.

There is also psychophysical evidence consistent with interaction between dorsal and ventral systems in the processing of biological motion. Knowledge of body kinematics can influence observers' judgements of shape and identity from motion displays. Viviani and Stucchi (1989) used displays in which a point of light traced out an elliptical path, with a velocity profile matching that of a person's hand making a natural drawing movement (i.e., following the 1/3 power law relating velocity and radius of curvature). Observers' judgements of shape were more strongly influenced by the velocity profile of the moving dot than by the shape of the path that it traced out, implying that perception of geometric form is influenced by implicit knowledge of arm and hand kinematics. Alternatively, work by Pollick and Sapiro (1997) has revealed common computational properties between the visual representation of planar shape and the 1/3 power law of drawing that could form a common basis for perceiving and drawing planar shapes.

Knowledge of more specific kinematic patterns can also influence recognition of shape. Observers are able to discriminate their own drawing actions from those of other people, when depicted in a kinematic display (Knoblich & Prinz 2001), and to use the kinematic properties of handwriting movements to predict which letter will follow one displayed (Orliaguet et al. 1997). These findings imply that processes underlying recognition of shapes, supposedly in the ventral system, have access to knowledge of kinematic properties of body movements. The same is also true of knowledge of the anatomical structure of the body (Shiffrar & Freyd 1993). If observers are shown successive images of an actor holding an arm in two different positions then, provided that the interval between the onsets of the frames is longer than 500–600 msec, they perceive the arm to move in a curved but anatomically possible path. While ventral processing for visual awareness or recognition might draw on its own, special-purpose representations of body structure and movement, it is more parsimonious to suggest that it has access to those used in the planning of motor actions in the dorsal system.

The evidence from research on the perception of biological motion is consistent with Milner and Goodale's (1995) theory of two visual systems, but raises new questions about the nature of the interactions between them. Are motion processing and the representation of body structure and kinematics really duplicated in the dorsal and ventral pathways? If not, what interactions between them are involved in the perception of biological motion? Norman's arguments for identifying the cognitive and ecological approaches to perception with the ventral and dorsal systems, respectively, seem very likely to hinder progress on these important problems.

Direct information on the cutting room floor

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Abstract: Norman's assigning of the constructivist percept-percept coupling approach and the ecological affordances approach to the ventral and dorsal visual systems, respectively, makes a more workable metatheory than each taken separately, but brings both under closer inspection.

Discussing them mainly in terms of perceived size and distance, Joel Norman thoughtfully reconciles the "classical" Helmholtzian inference-based constructivist theory of visual perception, and the more recent Gibson-derived invariant-based ecological viewpoint, by attributing them to the ventral and dorsal visual systems, respectively. If we yearn to retain those approaches, his attempt is probably as good as we can get. But I think that it, and an earlier attempt by Neisser (1989; 1994), invites more attention to how both theories deal with the behavior of visual inquiry.

In Helmholtz's theory, all perceived distal properties, like an object's perceived size and distance, start out as sets of pointwise independent sensory responses to proximal (e.g., retinal, oculomotor) stimulation, which are then subject to unconscious inferences based on specific contingencies learned from the world and from the details that can be estimated about the sensory organs themselves (e.g., interocular separation, retinal acuity). Relevant physical structure is thereby internalized, visual attributes (retinal size, binocular distance) are derived or perceived, and other perceived attributes are inferred from these. All perceived distal relationships, and most illusions, seemed quantitatively predictable, within this unified enterprise, making it an admirable first start at a theory of perception.

Unfortunately, that theory has been untenable for more than fifty years. First (as Hering and Mach had argued a century ago; see Hochberg 1998a), receptors and higher cortical structures act in patterns, not modular points, so that what needs to be inferred (rather than responded to directly), and how, remains undetermined. Second, perceptions do not in fact follow the presumably-internalized geometries – thus, perceived size does not necessarily vary with perceived distance (Haber & Levin 2001), perceived shape does not depend on perceived slant (Pizlo & Salach-Golyska 1995), and most fundamentally, viewers turn out to be incapable of perceiving metric structure as such (Todd & Norman, submitted).

Most important of all, we now know (Lennie 1998) that, as in

Hebb's (1949) persuasive phase sequence speculation, any neural input's effect in the visual cortex depends not only on that input, but on re-entrant input from ongoing activity elsewhere in the brain – that is, on what the viewer brings to the visual question being asked.

We need a different constructionist approach, and we have one, essentially starting in its modern form with Neisser (1967). In this view, perceptual inquiry is an attentive act, essentially the testing of the perceiver's schematic expectations about the environment. This approach has considered how attended things are perceived and unattended things are not perceived (Neisser 1964; Neisser & Becklen 1975), as in issues now called "popout" and "inattentional blindness," rather than the geometry of objects and space. I return to that after briefly considering the ecological viewpoint.

Gibson's ecological approach, which Norman (and Neisser) assign to the dorsal system, rejects any Helmholtzian "perception-causes-perception" inferences. Viewers directly pick up those mathematical invariances in the physical optical array which specify affordances for their potential purposive behaviors – that is, means-end-readinesses like graspability, point-of-approach, and so on. Perceived size varies with perceived distance simply because information in the optic array specifies both. (This is considerably closer to what I originally meant by perceptual coupling, in 1974, than the way today's Helmholtzians are using the term; see Hochberg 1974.) In this theory, stimulus analysis by the visual system starts at an ecologically relevant level of stimulus information (gradients of texture, patterns of motion parallax).

Two problems in this approach are important here: First, its ideosyncratic avoidance of neuroscience, which Norman's article should help end. Second, despite its emphasis on perceptual behavior, it neglects the most pervasive of perceptually-guided human behaviors: active saccadic looking. Such eye movements, made to change the proximal optic array (or retinal image), cannot rationally be neglected by any attempt to understand visual perception. Although such behaviors may indeed seek information about distal spatial structure (e.g., the path of the wayfarer's motion; Cutting et al. 2000), saccadic glances are predirected and attention-driven (Hoffman & Subramaniam 1995) with their goal being to bring to the eye's fovea that part of the presaccadic field of view that promises to afford the detailed information the viewer is seeking. Saccadic behavior is ecologically consequential, since it supports our massive visual environment of text, pictures, movies, and interactive graphic displays

In most laboratory research, simple objects are often presented within one brief glance, usually terminated by some form of mask (and it seems relatively easy to relate such input to measurable brain events). However, such single unanticipated, brief views are rare outside of the laboratory; when they do occur, almost certainly unmasked, they usually provide, first, a fast undetailed component, from the magno cells over the larger field of view, and then, a slower detailed component from the parvo cells near the fovea. (Indeed, masking may chiefly result from what Enns & Di Lollo [2,000] term object substitution: re-entrant contributions from higher and lower cortical responses competing for notice.) When presented as multiple brief views (as in the 0.10–0.50 msec of visual displays), morphing and short-range apparent motion results, resistant to the outcome's meaning and determined by where the successive contours fall.

Outside the laboratory, looking at the world or at media presentations of it within the range of voluntary saccadic glances (e.g., 200–2,000 msec), the viewer's attention and expectations largely determine whether and where another glance will be taken. The information provided by the visual environment may indeed be rich and detailed, mathematically highly constrained, and relatively easy to specify and study. However, what we encode and carry from one view to the next, whether of the size of the field that is open to be visually explored (Intraub 1997; Intraub et al. 1998), the layout and content of successive overlapping glimpses of some scene (Hochberg & Brooks 1996a; 1996b; O'Regan & Noë), or even the construction of some simple object examined with different foci of attention (Hochberg, in press), depends on the viewer's schemas and choices. Without active participation by the viewer, some unknown proportion of the potentially-direct information in the field of view clearly falls between the glances.

We need to study successful and unsuccessful viewing (and/or film editing) over multiple glances, just as we need research on effects of stimulus information within a single presentation.

A wider view of the spatial mode of vision

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Abstract: The two modes of visual processing "localizing" versus "identifying" as expressed by four authors in 1967 are more encompassing than the "two visual systems" dichotomies posed by later theorists. Norman's view of parietal cortex functions of vision seems much too narrow.

Norman carefully distinguishes the psychophysical properties of object *perception* and those underlying the visual guidance of *localizing* movements, and he plausibly relates these two kinds of visual information processing to known functions of temporal and parietal cortices in primates. However, in presenting a short history of the "two visual systems" concept, Norman gets much of it wrong. And he ignores important properties of the "spatial" system that makes his view of parietal cortex functions regrettably narrow.

One part of his short history is irrelevant to the main debate on parietal versus temporal visual mechanisms: Schneider's distinction between *orienting* functions of the hamster's tectum and *discrimination* functions of the visual cortex. His is the best-remembered statement from our initial proposals (Held 1968; Ingle 1967; Schneider 1967; Trevarthen 1969) but it is the least relevant to Norman's discussion. Schneider assessed the function of the hamster's visual cortex by a stripe orientation discrimination, hardly a test of "*identification*" and not dependent on temporal cortex. While Norman ignores my 1967 paper on dissociable visual mechanisms in goldfish, he implies that a later paper, "Two visual systems in the frog" (Ingle 1973) supports Schneider's dichotomy; whereas, in fact, it distinguished two different subcortical orienting systems: tectum and pretectum.

In my 1967 paper, I distinguished *shape* identification versus *spatial* processes underlying two modes of interocular transfer of mirror-image shape discriminations in goldfish. Both processes occur in an animal without any neocortex, although the same contrasting modes of interhemispheric transfer were also seen in split-chiasm monkeys (Noble 1968). I found that mirror-image shapes (e.g., leftward vs. rightward sideward T's) transfer from eye-to-eye "veridically" in goldfish (when they are small and seen as single objects), while the discrimination values of stimuli are reversed when these shapes are larger. I proposed that for larger patterns the two line segments are seen in different locations: for example, one pattern is coded as "vertical-line-in-front" and the second shape as "horizontal-line-in-front." That kind of selective attention results in the equivalence of mirror image shapes, when they are later seen in the opposite visual field. In those transfer tests a fish that had been reinforced for avoiding "vertical-in-front" via one eye, still avoids "vertical-in-front" seen via the other eye. This "spatial coding" hypotheses was confirmed by Campbell (1975) who found that fish trained to discriminate mirror-image pairs of larger shapes, generalized the responses to presentation of only the front half of either training shape. Further examples of the "*spatial mode*" of shape discrimination in fish are reviewed by Ingle (1978).

My idea that larger mirror-image pairs can be discriminated by various animals using a "spatial coding" mode was supported by the finding that a discrimination by monkeys between mirror-image shapes (or between the same shapes rotated by 90 degrees) is *not* impaired by inferotemporal cortex lesions, as are discriminations based on identification of a particular shape (Gross 1978). The implication is that the differing locations of some feature of the two shapes (a point or a line segment) are registered in another visual area, such as parietal cortex. The attempt of Eacott and Gaffon (1991) to test that idea led to ambiguous results: All three monkeys with parietal lesions found rotated shapes harder to discriminate than different shapes, but two animals did not show an absolute deficit with rotations, compared to unoperated controls. I would have suggested using *larger* shapes as a better test of the hypothesis that the parietal cortex contributes heavily to discrimination of different orientations of the same shape.

Studies with parietal lesioned monkeys have not yet been undertaken to test for possible deficits in registering spatial relationships of separated contours: for example, whether two edges are aligned across a gap, whether they are parallel to one another, or whether one is higher than the other. As Milner and Goodale (1995) point out, deficits in ocular scanning might create difficulties in interpreting such comparisons – but monkeys can learn to make judgments when pairs of stimuli are presented tachictoscopically. The spatial comparison tests used by Trevarthen and Sperry (1973) with split brain patients – high/low, near/far, approach/recede – did not require eye scanning and could be adapted for monkeys.

However, several early studies of patients with parietal damage demonstrated deficits in reporting both location and orientation of simple objects (e.g., Newcombe & Ratcliff 1989). More recently, Von Cramon and Kerkhoff (1993) reported that patients with anterior parietal lesions (confirmed by MRI) showed impaired perception of angles and axes, while perception of position and distance was more impaired by posterior lesions. One cannot explain this dissociation by saying that defective eye scanning accounts for all "spatial" deficits. Norman has underestimated the role of parietal cortex in *conscious* perceptions, which can operate independently of visuomotor actions

Held (1968) analyzed conscious and relativistic spatial percepts following "adaptation" of subjects to horizontal prismatic displacement of objects seen in the upper visual field. After removal of the prisms, objects objectively aligned on a vertical axis directly below now appeared to fall along a diagonal axis. Yet, while the perceived *relative* locations of two (or more) objects were modified via prismatic adaptation, a single vertical line was still seen as continuous as it crossed the horizontal meridian. Held argued that perception of a line's orientation and its continuity is rigidly fixed by the *identification* system, while some spatial relations between separate objects can be "relearned." A dramatic example from Kohler (1964) extends Held's argument. Following long term wearing of left-right reversing prisms, Kohler's "adapted" subjects reported that the *location* of cars on the street appeared no longer to be reversed, but that the letters on the license plates were still reversed. Neither Held nor Kohler had evidence to postulate cortical loci for these "what versus where" percepts, but they appear to correspond to temporal versus parietal visual functions. And here it is the parietal mode of spatial processing which is susceptible to perceptual learning, while the identification mode proves to be rigidly fixed.

Norman fails to note that Trevarthen (1968) had mentioned perceptual functions of the parietal cortex and was careful to say that the relative contributions of cortical and subcortical visual areas to spatial vision had not been worked out. Trevarthen discussed the role of "ambient vision" (self-produced motion) in defining the relative locations of objects *and* in sharpening perception of one's own direction of motion (drawing here on Gibson's views), but he did not emphasize spatial vision as a framework for reaching. Ironically, he proposed that the geniculostriate system – with its emphasis on analyzing details of foveated objects – is used for *praxis.* Norman ignores the question as to whether functions of temporal or parietal cortex are critical to identification of small objects by stereognosis (during finger grasping).

Trevarthen and Sperry (1973) further illumined the "*what versus where*" dichotomy by asking split-brain subjects to compare visual stimuli in opposite hemifields. While no subject with the callosum severed could compare shapes or colors seen on opposite sides of the fovea, they could describe *spatial* features of stimuli seen simultaneously in peripheries of opposite fields. They could say which object was "higher or lower," "approaching or receding," and whether a given hemifield contained one or two moving objects. These authors argued that while shapes and colors were processed via striate cortex and hence "disconnected" via callosal transection, the *relative* locations and motion directions were transmitted to the opposite brain half via subcortical visual commissures and relayed via the thalamus to the extra-striate cortex via motion-sensitive channels. We suppose that this stream of visual information (which crosses the midline subcortically) is perceived after it reaches the parietal cortex. We don't yet know how parietal lesions in man would affect these tests of *relative* localization.

Trevarthen and Sperry described *relative* location and *relative* direction as salient features of this spatial mode of vision; as before them Held and Kohler had each described adaptations to spatial rearrangements in terms of changes in perception of *relative* direction. Furthermore, the test that Ungerleider and Mishkin (1982) used as diagnostic for parietal function in monkeys (the "landmark" test) involves a judgment of *relative* distances between objects. Yet, Norman assigns relativistic judgments to the inferotemporal cortex and absolute ego-centric directions to the domain of parietal cortex. Since the investigators that I have reviewed stress *conscious* percepts as representative of the spatial mode of vision, Norman's stress on *unconscious* localization processes reflects a narrow view of parietal functions.

While Norman argues that the spatial representations of the parietal cortex are egocentric, my previous report in this journal (Ingle 1990) showed that the excellent ability of humans to remember visual locations after self rotation (in real-world coordinates) was eliminated by a small parietal lesion in a split-brain subject. That lesion did not affect visuomotor skills since this subject could draw well with either hand. Following body rotations of only 60 degrees in either direction, he randomly guessed at the locations of targets seen 10 seconds earlier in the hemifield opposite his parietal lesion. Yet, even with larger rotations, he pointed accurately at targets seen earlier in the field ipsilateral to the lesion. He did not "neglect" the contralateral field and could point to objects seen there if he remained stationary for 10 seconds. In summary, I conclude that a part of the parietal cortex plays a role in maintaining memory of real-world spatial locations during self-motion.

My remarks do not deny the major contribution of the parietal cortex to egocentric (and often unconscious) processes involved in grasping and manipulating objects. I appreciate the impact of Milner and Goodale (1995) in stressing the kind of "object vision" mediated by the parietal cortex, but I reaffirm the arguments of Ungerleider and Mishkin (1982) that the functional dichotomy between temporal and parietal cortices is not to be defined simply as *perception versus motor guidance.* Students of parietal function have scarcely begun to analyze mechanisms for registration of spatial relations *between* small objects or *within* large objects. And they have tended to ignore the problem of the spatial constancy of the remembered world during translations and rotations of their own bodies (but see Berthoz 1997).

A further stage of theory-building remains: relating the use of eye, hand, and body movement to the very ontogenesis of spatial perception. Even the relationships which normal humans can perceive during a fixed gaze, may be "constructed" through early experience with reafferent visual changes during eye and body movements. I do not have time here to discuss the evidence of Hein and Diamond (1983) showing that eye movements are required for kittens to learn visuomotor skills but not for adult cats to perform them. The exciting new report of Sereno et al. (2001) that the "spatial map" in one area of the human parietal cortex is not a passive two-dimensional screen (as in striate cortex) but is a map of directions for eye-movements, also suggests a role for eye movements in the calibration of spatial representations in infancy. Finally, in order to understand how the various modules within the parietal cortex are coordinated through experience, we may have to consider still higher executive functions, such as those attributed to the prefrontal cortex (Miller & Cohen 2001).

On invariant-sensitive graspers and cue-sensitive perceivers

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Abstract: I argue that Norman's thesis works in sofaras one is prepared to be lax about the alleged congruency between "ecological" and "dorsal," and unconcerned by the results of recent studies showing that grasping movements are subject to the same geometric distortion illusions as perceptual judgements.

In what, for many, is the manifesto of the computational approach to vision, David Marr wrote in *Vision* that Gibson was not the first to be misled by the apparent simplicity of the art of seeing. Indeed, he "seriously underestimated the complexity of the information-processing problems involved in vision and the consequent subtlety that is necessary in approaching them" (Marr 1982, p. 29). For Marr, the detection of the "invariants" in the optic array that Gibson rightly understood to be so rich a source of information about spatial relationships was *ipso facto* the stuff of computational vision. Implicit in Marr's thinking was the idea that all of vision is both ecological and constructivist. That is, all perception is mediated by brain mechanisms that have inbuilt knowledge about the visual world, whether in the form of orientation selectivity in V1 simple cells, the mechanism that implements the gestalt law of common fate,[1] or the assembly of neurones that enables you to recognise your grandmother. In his carefully considered and highly thought-provoking article, Norman however prefers to reconcile rather than abandon the dichotomy between ecological and constructivist, by giving each its own place in the anatomical-functional division between Milner and Goodale's (1995) "dorsal-for-action" and "ventral-for-perception" streams. Just how far Norman succeeds depends on whether one is prepared to be lax about the alleged congruency of the ecological and the dorsal, and not too worried about certain problematic pieces of evidence that have surfaced in recent studies of geometric illusions.

Regarding the alleged congruency between the dorsal and the ecological, Norman's thesis works providing one *defines* ecological perception as that concerned exclusively with visual signals that control motor activity. However, for Gibson at least, invariants apparently unrelated to motor activity were also involved in ecological perception, for example, the texture-interception and horizon ratios used for size constancy. Since we are able to make reasonably accurate perceptual decisions about the relative sizes of distant objects, Norman is forced to relocate such object-centered invariants to the ventral-constructivist pathway and change their name from invariants to cues (see sect. 5.2.1). In so doing, he gives the ventral pathway an important role in ecological perception. That's fine with me, but it seems at odds with his thesis.

There are in fact many examples in vision where both invariants and cues are exploited for a common purpose, and which are therefore likely detected in the same pathway. One example is lightness constancy. By taking the ratio of an object's luminance to that of its surround – an invariant – we achieve lightness constancy with respect to the ambient level of illumination. But we also need to discount the effects of spatially varying illumination such as shadows and shading, and these must first be identified from signature figural properties such as X junctions, that is, cues. These invariants and cues are probably detected by mechanisms at different *stages* in the visual pathway (Kingdom 1997; in press), but probably not within *different* pathways.

These considerations raise a related and thorny question: When is a visual signal for "action," and when for "perception"? It is not always so easy to tell. Consider the study by Proffitt et al. (1995) described by Norman. They showed that haptic (manual) judgements of the perceived slant of a distant hillside were more veridical than perceptual (e.g., verbal) judgements, and Norman interprets this result as consistent with the dorsal-action versus ventral-constructivist distinction. Notwithstanding the inherent difficulties of comparing such radically different methods of estimating slant, one must question whether a manual estimate of the perceived slant of a distant object is any more relevant to motor control activity, or any less relevant to perception, than a verbal one.

Turning now to the problematic evidence, I refer here to the recent findings of Franz et al. (2000; 2001) with the Ebbinghaus illusion, and Vishton et al. (1999) with the horizontal-vertical illusion. These studies, in my view, have convincingly demonstrated that once the task requirements are made commensurate, grasping actions and perceptual decisions are affected equally by geometric distortions. This surely casts doubt on the strong form of Milner and Goodale's hypothesis, which begins from the standpoint that the visual signals utilized for motor control have fundamentally different origins and are subject to radically different transformations than those used for making perceptual decisions. There is, however, a weaker form of the hypothesis that is in keeping with the results of the above studies, and which Norman himself seems to veer towards on more than one occasion in his article. It is that a considerable amount of computational processing has already taken place prior to the divergence of signals into the dorsal and ventral streams, including the computations of size that give rise to illusions such as the Ebbinghaus. Under this weaker form of the hypothesis, visual form agnosics such as D.F., while being able to recruit the outputs of these relatively low-level computations for motor activity, are unable to utilize *the same signals* for higher-order processing, such as for identifying objects and faces, or for storage in a representational form (verbal or iconic) that allows those signals to be used for relatively simple perceptual tasks such as judging orientation. If so, "perception-for-motor-activity" versus "perception-for-recognition" might be better acronyms for the functional differences between the dorsal and ventral pathways.[2] This alternative view has the added benefit of not requiring that invariants be segregated into those for motor control and those for perceptual decision making, since both activities would access a common early representation of those invariants. Indeed, if Gregory (1963) is correct in his original suggestion that illusions such as the Ebbinghaus are a result of misapplied size constancy, then it makes perfect sense that invariant-sensitive graspers and cue-sensitive perceivers should be affected by such illusions, alike.

NOTES

[1] **1.** Elements moving in the same direction tend to be seen as part of the same object.

[2] **2.** I prefer "motor-activity" to "action" because the ultimate purpose of all perception is to enable the user to act upon his or her physical, biological, and social environment.

The primacy of ecological realism

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Abstract: Whether or not the correspondence of dorsal stream functions to Gibsonian ecological psychology and the ventral stream functions to "constructivism" hold up, the overall goal of capturing a pragmatic realism should not be forgotten.

The fundamental perceptual question for ecological psychology, as some of us understand the field, is "How does an animal perceive its environment?" (Shaw et al. 1982). In proximal-distal language (which Gibson rejected as incoherent), the question would be, "Why does an animal *ever* experience the distal?" We have argued that the primary emphasis needs to be on the word "environment," and that the priority to be emphasized is the problem of reference in perception. Thus, the question of how an animal perceives its environment is to contrast "the environment"[1] with other possible objects of perception. "How is it that the animal perceives its environment, and not something else?" This is the question of realism, and it is a question that can guide empirical research.

The realist emphasis is one that does not come through in Norman's description of ecological psychology in the target article. He understands correctly that Gibson argued for a far richer view of optical structure than most other researchers, and that the concept of an invariant is important, but does not mention that the point of richer notions is to converge on "the environment," to make specificity possible. I do not think Norman objects to the realist position, but his focus is elsewhere. The persistent problem of reference (Shaw 2001) is rarely acknowledged by psychologists and neuroscientists, including adherents of Gibson, yet we've always taken it to be where Gibson's insights have contributed the most. In brief, Gibson's answer to why it is the external world that an animal perceives and not something else, is that the information (optical structure, for vision) specific to the environment is *different from* the information specific to anything else. Gibson's enterprise of ecological optics, and the consequences of its alternative descriptions, is devoted to finding formulations that are more and more adequate to this scientific goal of principled, specific description of environmental information. We take it that questions of processing and questions of directness and indirectness are *subordinate to* the question of whether or not the environment is indeed what is perceived. We have maintained that indirect perception of the environment will necessarily be parasitic on direct perception and would be impossible without direct perception.

Gibson's position is that information (optical for vision) is indefinitely rich in its specificity. A real world is distinct from a surrogate world by virtue of its nested structure at all scales. When one looks closely at the skin of a person, one ultimately gets to cells. When one gets closer to a painting of the same face, one gets to the paint and grain of canvas, not cells of skin tissue. For a digitized photo of the painting, one gets to pixels, not paint and canvas. The scrutiny of the world at a variety of levels, which exist simultaneously, is critical for clarifying what one is perceiving. The convergence of perception on the "real world" in light of indefinitely rich, specific information, is crucial to how Gibson thought about perceiving and its foundations. In pattern recognition, there is an unknown pattern and the task is to make it explicit, to come up with an answer to the question of what it is (Marr 1982). In Gibsonian perceiving, there are no right or wrong answers, but degrees of clarity and sufficiency for the tasks at hand. Perceiving is pragmatic. There is always more to be perceived in any real situation, and obtaining additional information is a criterion of reality. The specific cases of texture gradients, horizon ratios, optic flow, and tau, stimulated by ecological research, need to be thought of as way stations toward increasing understanding of optical information (for vision), and not as ultimate destinations. They represent progress over what came before, and they illustrate what Gibson meant by "higher order invariant," but they are far from sufficient to specify fully the concrete world that animals (of any kind) live in. They do not, in themselves, capture the nesting type of organization crucial to Gibson. If we were to stop with the inventory we have, we would have "higher order invariants," but we would still be far short of specificity. Our scientific characterizations have to get richer and deeper, just as Gibson said that perceiving over time does (learning). Scientists need some kind of vision to act as a guide for future work. What I've sketched is what I take to be a guiding Gibsonian vision.

Regardless of whether one calls what either the dorsal or the ventral system does "perceiving" or "information pickup," the question I want to highlight is whether the object of the system, for Norman, is the environment. If recognition and identification are carried out primarily by the ventral system, using long-term memory, what is it that is recognized and identified? If I see someone from a distance, without my glasses, and finally "recognize" the person as my acquaintance, John, what did the ventral system do besides come up with a name? It is one thing to try to identify some relevant brain events, but it is quite another to explain how they refer back to John, the unique person in the world.

Without an account of reference, I do not see how an indirect theory can succeed, and I can't see that associating the constructivist approach with the ventral system helps. The problems that constructivist approaches fail to address are still not addressed when one associates them with the ventral system.

What is to be said about the data reviewed by Norman? How are we to understand the two streams? There is much to be understood and he is persuasive that the labor is worthwhile. The development of Gibson's ideas toward more traditional "cognitive" topics was started by Gibson himself (see Mace 1986) and is being pursued seriously by Robert Shaw (Shaw 2001) by careful examination of intentionality and choice. As these efforts mature, I'm guessing that alternative interpretations of the functions of the ventral system will emerge and that we can fruitfully discuss and debate these alternatives with Norman.

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I thank Joel Norman for his patience and seriousness of purpose. It's a pleasure to know that "we're all in this together."

NOTE

1. The word "environment" is used broadly here to include the self, in accord with Gibson's stipulation that "to perceive is to be aware of the surfaces of the environment and of oneself in it . . . . These are existing surfaces; they are specified at some points of observation . . . The full awareness of surfaces includes their layout, their substances, their events and their affordances" (Gibson 1979/1986, p. 255).

The dual route hypothesis in visual cognition: Why a developmental approach is necessary

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Abstract: Norman presents intriguing arguments in support of a mapping between ecological and constructivist visual cognition, on the one hand, onto the dorsal ventral dual route processing hypothesis, on the other hand. Unfortunately, his account is incompatible with developmental data on the functional emergence of the dorsal and ventral routes. We argue that it is essential for theories of adult visual cognition to take constraints from development seriously.

Norman reviews an impressive amount of evidence in support of his claim that dual route visual processing reflects the distinction between constructivist and ecological approaches to visual cognition. Unfortunately, while he makes a convincing case, he fails to address an important part of the literature: the developmental literature. Although from the onset Norman declares that a true developmental approach is beyond the scope of the target article, his proposal rests on the assumption that the dorsal route functions are innate or largely mature very early in infancy, whereas the ventral constructivist functions are largely developed later in life. Norman cites a paragraph from Kellman and Artberry (1998) in support of this claim (see target article, sect. 5.2.1).

However, a wide range of behavioural and electrophysiological data does not support this assumption. The dual route visual processing paradigm is playing an increasingly important role in the study of infant perceptual and cognitive development (e.g., Atkinson 1998; Berthental 1996; Mareschal et al. 1999). One of the questions raised in developmental circles is whether the dorsal or the ventral route functions develop first during infancy. The general conclusion to this question is that if there are differences between the developmental rates of the ventral and dorsal routes, then the dorsal route is likely to be developmentally delayed with respect to the ventral route (e.g., Atkinson 2000).

We list here just a few pieces of evidence in support of this claim (a full review can be found in Johnson et al. 2001). Studies measuring Evoked Response Potentials (ERPs) to face images indicate that the ventral pathway can be activated at 6 months (albeit with some further specialization to take place; De Hann et al., in press). In contrast, ERP evidence suggests the dorsal pathway is still not influencing eye movement control at that age (Csibra et al. 1998). This ERP evidence is interpreted as suggesting that at least this aspect of dorsal pathway function is somewhat slower to develop than the ventral pathway. Other evidence arises from behavioural infant studies. While babies show sophisticated facial discrimination abilities (a canonically ventral function) from a very early age (De Hann & Halit 2001), body-centred spatial representations that guide eye movements develop gradually over the first year of life (Gilmore & Johnson 1997).

Within the context of this commentary we wish to remain agnostic with regards to (i) whether the two routes do actually develop at different rates, or (ii) whether they both develop at comparable rates during infancy. The important point is that neither of these interpretations of the data are inconsistent with Norman's proposals.

So, where does this leave us with regard to Norman's hypothesis? We do not have a problem with the mapping that Norman is trying to make between the dorsal/ventral dual route hypothesis and the ecological/constructivist debate in perception. In fact, we are generally sympathetic to many of his arguments. What we do wish to do, however, is to argue that any theoretical account of perception and cognition must take developmental constraints seriously (this was well understood by Gibson 1969). It is no use coming up with a theory of adult performance that is incommensurate with developmental evidence. Otherwise, one is stuck with the unwelcome task of explaining how one behavioural system is magically transformed into another at an unspecified point in development.

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One visual system with two interacting visual streams

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Abstract: Norman's aim to reconcile two longstanding and seemingly opposed philosophies of perception, the constructivist and the ecological, by casting them as approaches to complementary subsystems within the visual brain is laudable. Unfortunately, Norman overreaches in attempting to equate direct perception with dorsal/unconscious visual processing and indirect perception with ventral/conscious visual processing. Even a cursory review suggests that the functional and neural segregation of direct and indirect perception is not as clear as the target article would suggest.

Norman's argument falls short in part by failing to provide evidence that direct perception is generally unconscious. A number of the studies cited as demonstrations of direct perception are in fact demonstrations of direct *conscious* perception. Gibson's discovery of texture interception and the horizon ratio as invariants allowing size constancy was spurred by a study involving verbal reports of perceived size. Norman's (1980) own experiments required participants to report with a button press which of two objects appeared larger. Clearly – indeed, almost by definition – responses in these studies were based on observers' conscious percepts. While it may be true, then, "that certain invariant ratios were picked up unawares by the observers and the size of the retinal image went unnoticed" (Gibson 1979, p. 160; 1986), the result was nonetheless *conscious* perception of distal size. Unless we redefine the notion of conscious perception to include only instances in which an observer is capable of verbalizing the information and processes leading to the percept – by which definition almost all perception, including the unconscious inferential processes like those posited by Rock (1983), would be excluded – these studies do not buttress the claim that direct perception is largely unconscious. Norman wishes to circumvent this objection by suggesting that directly but unconsciously perceived information "can enter consciousness via the ventral system after the event" (target article, sect. 5.1). He gives no compelling reason, though, for us to reject the more parsimonious alternative that conscious perception may itself be direct.

Norman likewise gives very little evidence that dorsal function cannot be constructivist, often appearing instead to simply term the information used for visuomotor control as "invariant" with no independent evidence to justify the label. Nor is his argument rescued by an appeal to differential susceptibility of conscious perception and visual-motor behaviour to illusions. Assuming that illusions are evidence of a constructivist process, Norman suggests that action should be largely impervious to visual illusion, other than when behaviour must be guided by a remembered stimulus representation from a ventral (conscious/constructivist) store. A multitude of studies examining the effects of the Mueller-Lyer illusion on eye movements have consistently found an influence of illusive length on saccade programming: saccades to the endpoint of the subjectively longer end overshoot their target, and saccades to the endpoint of the subjectively shorter end undershoot it (e.g., Delabarre 1897). Importantly, these effects are obtained even when the stimulus remains visible throughout saccade preparation and execution; hence, the influence of the illusion on the movement was not produced by a memory-guided ventral stream representation. In studies of eye movements and the M-L illusion, subjects have generally been asked to saccade from end to end of the figure; movements have therefore been strictly voluntary. Our own data (DiGirolamo et al. 2001) suggest that there are differences in the influence of illusions based on the type of saccade required. Voluntary saccades are as influenced by the illusion as the conscious perception, while reflexive saccades (saccades to a flashed cue at the endpoint of a M-L segment) show modest effects of the illusion. These data are consistent with the notion that multiple spatial representations exist within the brain, which are differentially influenced by illusions. Note, however, that both of these movements are prejudiced by the illusion, arguing against a strictly direct representation used by the dorsal stream for guiding action.

To the extent that the Müller-Lyer illusion is a consequence of constructivist processing, then, dorsal function does not appear to be strictly direct. While our own data support the view of different representations and computations guiding action and conscious perception, it appears that most representations are susceptible to the constructivist process of the illusion. Separating direct and indirect perception within the brain, it seems, will be more difficult than Norman suggests.

Where does perception end and when does action start?

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Abstract: Currently there is considerable interest in the notion that dorsal and ventral visual systems might differ in their specializations for thought and action. Behavior invariably involves multiple processes such as perception, judgment, and response execution. It is not clear that characteristics of the dorsal and ventral processing streams, as described by Norman, are entirely of a perceptual nature.

Norman defines perception broadly as the effects of sensory stimulation on behavior. This all-encompassing definition is contrasted with a definition of perception as the conscious awareness of objects and events. Norman then suggests that there are two modes of visual perception: a dorsal stream associated with motor responses, and a ventral stream associated with verbal judgments. One problem with this approach is that it is difficult to distinguish between characteristics of these two streams of visual perception and characteristics of the manual and verbal motor systems that they are closely linked to.

The dorsal visual stream is said to be located in the main in the posterior parietal cortex and adjacent areas. The posterior parietal cortex may not be exclusively devoted to visual processes, however. Andersen et al. (1997) have reviewed evidence that the posterior parietal cortex, including areas associated with manual reaching, are areas of convergence of auditory, visual, vestibular, and proprioceptive information. These multimodal sensory signals are combined with efference signals from motor structures to produce multiple spatial frames of reference. Although there is a tendency to label all areas associated with visual information processing as belonging to the visual system, some of these, such as posterior parietal cortex, may actually be higher-order association areas that are not appropriately described as part of the visual system.

One proposed method of examining the differences in visual processing between dorsal and ventral streams is to contrast judgment responses with manual responses. An example is the study of Castiello et al. (1991) that showed differences in response latency for verbal and grasping responses to the sudden displacement of a visual object. It is important to keep in mind that any study that contrasts verbal and grasping responses necessarily involves both perceptual and motor processes. Differences in reaction time may be a function of differences between these two response systems that are independent of the nature of the stimulus. The problem is to find a method for differentiating the role of sensory and motor processes. One solution to this problem is to use stimuli from different sensory modalities to compare the characteristics of verbal judgments with those of manual responses such as grasping. If it is the motor systems that differ, then haptic or auditory cues should produce similar differences in reaction time between verbal judgments and grasping. If, on the other hand, visually guided grasping reflects some unique characteristic of a dorsal visual processing stream, then this should not be reproduced by cueing with other stimulus modalities.

Norman summarizes a number of differences between the two visual systems. These include temporal and spatial sensitivities, as well as the extent to which the two are memory based. The association of the dorsal stream with sensitivity to high temporal frequencies and the ventral stream with sensitivity to high spatial frequencies may be appropriate. However, the differential involvement of memory in the functioning of these two systems may be another matter. It is probably not correct to rule out the use of memory for behaviors such as visually guided reaching. Rather, it may be more appropriate to describe the calibration of visual-motor transformations as involving procedural memory. In contrast, semantic and working memory systems may be more involved in verbal judgments. Again, separating the role of memory in perception and action by behavioral criteria requires manipulation of the stimulus modality.

It is not clear how purely behavioral criteria can be used to dissect these two visual systems. The dorsal and ventral visual systems are defined anatomically. Thus, data that explicitly deal with anatomical localization are required, such as functional imaging and lesion studies. The data on optic ataxia support the distinction between the two visual systems. However, what ultimately is required here is a demonstration of localized damage correlated with modality-specific deficits. Concluding that some area, such as the posterior parietal cortex, is associated with visual guidance of movement requires demonstrating that patients do not have deficits with auditory or haptic directed movements. If the behavioral deficit is not restricted to visually guided movement, then the deficit should be characterized as being associative or motor in nature. The literature on optic ataxia does not always demonstrate dissociation in terms of sensory modality. For example, Buxbaum and Coslett (1997) note that optic ataxia by definition involves preserved ability to reach with auditory or tactile defined targets. However, no mention of this appears with their first case and they describe their second case as being impaired at directing gaze to auditory and visual targets. In addition, both cases had damage in both parietal and frontal lobes. The study of Milner et al. (1999), discussed by Norman, makes use of a subject with parietal lobe damage extending to premotor cortex on the right side. Again, testing was only in the visual modality. Careful experimentation is thus required to establish that ataxia in any particular patient is indeed "optic."

A definition of perception that includes all effects of sensory stimulation on behavior is too broad in scope. It follows from this broad definition that all psychological processes are perceptual processes. Defining perception as the conscious awareness of objects and events is consistent with our subjective notion of perception. However, this approach is not useful for generating testable hypotheses amendable to an experimental approach. A definition of perception should be restricted to those aspects of sensory stimulation that are modality-specific. With this approach it is possible to ask specific questions about the nature of visual perception, or sub-categories of visual perception. Visual perceptual processes can then be compared and contrasted with perception in other sensory modalities. The proper way to do such experimentation is to vary parameters associated with the stimulus while minimizing the variance associated with the response modality (McFarland & Cacace 1995). In contrast, Norman suggests holding the stimulus constant and varying the response modality. This strategy is likely to provide information that is useful for characterizing different response modalities, but may tell us very little about the nature of perception.

The dorsal system and the ecological self

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Abstract: Perception, as Gibson described it – picking up information that specifies the real local situation – includes not only perceiving affordances and controlling small movements, but also seeing the large-scale environmental layout and the position/movement of the "ecological self." If the dorsal cortical system is also responsible for that very significant achievement, its activity must be at least partly conscious.

For over twenty years, James J. Gibson's (1979/1986) elegant ecological account of vision has stood in sharp contrast with standard information-processing models. Gibson (1966) insisted that vision is a *perceptual system,* which actively seeks information rather than merely processing input. That information, available in the *optic array,* specifies not only the possibilities for action that he called *affordances,* but also the *layout* of the local *environment* and – especially important for my argument here – the position and movement of the *self.* This contrast between the views of Gibson and his critics is not just a matter of terminology, nor does it stem only from his consistent rejection of all processing models. At its most basic level it is an epistemological contrast. For Gibson, the information available to active observers uniquely specifies the real layout. Perceivers do not need to make guesses or draw inferences about the local situation, because they can see it (and their own position in it) *directly.* Adherents of the standard cognitive science model find this preposterous: for them, perception is always a matter of indirect inference.

Gibson and his critics both believed that their opposing views of perception were irreconcilable. Regarded as accounts of a single mechanism, they certainly are: picking up invariants is one thing, while making probabilistic inferences is quite another. But could there possibly be two separate systems, one of which picks up invariants, while the other makes inferences? The concept of two functionally distinct visual systems was only beginning to develop in Gibson's time, so we do not know what he would have thought of it. The first compelling evidence for distinct cortical systems in vision was presented only three years after his death. Ungergleider and Mishkin (1982) noted that the *dorsal* and *ventral* systems, which they had successfully isolated in monkeys, could be loosely described as focused on "where" and "what" respectively.

Later in the 1980s, it occurred to me that the "where" and "what" systems of Ungergleider and Mishkin could be neatly mapped onto the views of Gibson and his opponents, and I gave several talks expounding this hypothesis (e.g., Neisser 1989). Nevertheless, I did not proceed to publish it, mostly because my grasp of the underlying neuroscience was a bit shaky. I'm delighted that Joel Norman has now (quite independently) developed a similar view, and indeed carried the analysis much further. I agree substantially with that analysis, except perhaps for Norman's conclusion that the dorsal system is the faster of the two. Sometimes this may be so, but a system that integrates movement-produced information over time cannot always give an instantaneous response. Moreover, the rapid word-recognition capabilities of the ventral system – as exhibited in fast reading – are impressive indeed. Because there is no space to pursue that issue here, I will confine these comments to a single point which Norman has somewhat neglected: the perception of the self.

Much of the recent evidence for an independent dorsal system is based on the control of relatively small movements: pushing a card through a slanted slot, picking up an object between thumb and forefinger, and so on. Gibson was certainly interested in such tasks, and often cited graspability as an example of an affordance (1979, p. 133). Goodale and Milner's (1992) ingenious studies of their patient DF have further focused interest on small movements of this kind, and led them to describe the dorsal system as focused on "how" rather than "where."

As Norman points out, the information that the dorsal system uses in the control of such movements probably includes motion-produced optic flow as well as binocular disparity. But those forms of information – and related forms such as occlusion/disocclusion – specify more than just the dimensions and affordances of small objects. They also specify the layout of the large-scale surrounding environment itself, together with the perceiver's position within that environment. There is no room here to describe those forms of information in detail; I have done so elsewhere in defining what I call the *ecological self* (Neisser 1988; 1995). Thanks to optic flow and similar forms of information, perceivers can simply *see* where they are and where they are headed. More fundamentally, they see that they are *in* an environment: it exists independently of them, but they can act on its affordances. This is the basic condition of terrestrial animal life, the situation within which everything else occurs, the "horizon" that has so often been discussed by phenomenologists. The remarkable visual system that enables us to perceive that situation itself is surely one of the great achievements of evolution.

It is not entirely clear whether the dorsal system as we presently know it carries out this crucial function. In principle there might be *two* Gibsonian perceptual mechanisms, the dorsal system for controlling small-scale actions and another, yet to be discovered, for using similar information to locate the self in the larger environment. Nevertheless, it seems more parsimonious to posit just one.

If indeed there is only one – that is, if the dorsal system discussed here is also responsible for people's awareness of being selves located in a larger environment – then the question of *consciousness* will have to be reconsidered. Norman describes the activity of the dorsal system as outside consciousness, largely because subjects often cannot report the very aspects of the environment to which that system is evidently tuned. But whatever the merits of this argument with respect to small-motor control, it does not apply to the layout of the environment and the ecological self. We are almost always aware of our own situation in the environment, even though it rarely becomes focal. Some aspects of the activity of the dorsal system, then, are by no means unconscious.

Two theories of perception: Internal consistency, separability and interaction between processing modes

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Abstract: Comparisons are drawn between two theories of visual perception and two modes of information processing. Characteristics delineating dorsal and ventral visual systems lack internal consistency, probably because they are not completely separable. Mechanism is inherent when distinguishing these systems, and becomes more apparent with different processing domains. What is lacking is a more explicit means of linking these theories.

Norman seeks a potential rapprochement of constructivist and ecological schools of perception by identifying these schools with the features of ventral and dorsal visual systems, and conscious and unconscious modes of information processing. Within cognitive psychology, such two process models of information processing gained acceptance because the concept of automaticity was felt to be a valuable explanandum. Nevertheless, critics felt that such models only had worth if the characteristics delineating these processing modes were internally consistent.

For Norman, internal consistency requires that ventral/constructivist processing always involves identification, processing time, and is conscious, while dorsal/ecological processing always involves performance, is immediate, and is unconscious. In Norman's case, this does not seem to hold. There are already some confusing inconsistencies observed when people interact with illusory figures.

In addition, Norman implies a close identity between the conscious ("conscious awareness") and cognition ("the cognitive system"), whereas the terms are different and distinct. The term "conscious awareness" implies attention, which may be the result of deliberate, intentional selection (mediated ventrally), but attention can also result from a more "primitive" dorsal process, as when a flashing light in the peripheral visual field causes reorientation in that direction. Cognition is rather an outcome (or purpose) of sensory processes. Hence, clarification is required of processes that are described as sensory, perceptual, conscious, or cognitive. For example, spatial self-awareness can be demonstrated in visual or auditory modes; however, the concept itself is cognitive, and it may or may not be conscious. Hence, while it might be appropriate to associate consciousness with the ventral system, it is not an appropriate distinction for cognition.

Nevertheless, even if there are internal inconsistencies in a presentation of two processing modes, this need not be a problem. A requirement of internal consistency of processing modes also implicitly requires the separability of processing modes. For instance, the two visual systems are potentially dissociable under poor visual conditions during driving (e.g., darkness, bad weather), with the conscious system often having an inadequate appreciation of the limitations of the other system. However, it is doubtful that the two processing modes can occur in isolation, since each mode is likely to provide products, and context, to influence the other. For example, any selectivity of awareness implied by the dorsal/ventral distinction suggests a paradoxical awareness by the dorsal system of the material to be excluded. Instead, the dorsal/unconscious system is likely to provide the context within which a ventral/conscious system operates, that is, to provide sensations for perception. Indeed, drawing from our knowledge of reflexes, it is likely that one system would be modulated within the context of the other (see Prochazka et al. 2000). For instance, saccadic eye movements serve to maintain the visual focus of moving objects.

Instead, a consideration of separability of two visual processing systems leads to questions typically not considered by ecological theorists, that is, questions of mechanism. Ecological accounts focus upon competence, while constructivist accounts dwell upon performance and mechanism. For such reasons, constructivist accounts tend to be stronger when seeking to understand damaged or disordered processing and offering intervention. In contrast, ecological accounts tend to dismiss damage or insult (Latash & Anson 1996), but in so doing, potentially ignore important phenomena.

There are a number of distinct problems of object recognition, space perception, distance processing, and perceptual tuning, that emerge as a function of neuropathology (e.g., agnosia, hemineglect, Parkinson's disease, cerebellar disorder). We have observed similarities between some of these conditions, and the problems healthy individuals have when interacting with tools (namely, graphical user interfaces) (Phillips & Triggs 2001). Under less than optimal conditions, a variety of mechanisms appear to contribute to performance. Instead of an integrated whole, the apparent fusion of mechanism is reduced and we begin to notice contributing parts. It would appear that when simple, direct, sure solutions to performance are lost or unavailable, humans rely upon more flexible processing systems. Indeed, some of these systems are likely to be the phylogenetically important systems that make us uniquely human. Regrettably, by identifying the dorsal and ventral visual systems with the two major perceptual schools, there is an unfortunate but seductive tendency to overlook other systems and mechanisms. For example, driving involves considerable head movement, and the vestibulo-occular reflex serves to maintain focal vision during such head turns. We would thus expect the vestibular system and the cerebellum to interact with the dorsal system, and the frontal lobes to interact with the ventral system.

The identification of the potential neurophysiological substrates underpinning the two schools of perception is of interest. However, the difference between the schools of thought is paradigmatic, and reflects the types of tasks the perceptual system is required to undertake. Thus, there does not appear to be the same sorts of theoretical imperatives that were required for the acceptance of controlled and automatic modes of information processing modes in cognitive psychology. Indeed, what is lacking is theory that ties these modes of processing together. However, a focus upon the underlying neurophysiological substrates is liable to generate a better understanding of mechanism, with a greater potential for future intervention and rehabilitation.

The ventral stream offers more affordance and the dorsal stream more memory than believed

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Abstract: Opposed to Norman's proposal, processing of affordance is likely to occur not solely in the dorsal stream but also in the ventral stream. Moreover, the dorsal stream might do more than just serve an important role in motor actions. It supports egocentric location coding as well. As such, it would possess a form of representational memory, contrary to Norman's proposal.

Norman's target paper must be applauded for bringing together two historical approaches to visual perception – the ecological-constructivist and the dorsal-ventral division – and providing a basic scheme to look at the organization of the visual system. At first sight, the ecological-constructivist division seems to profit most from this attempt at reconciliation, as competing theories of visual perception are integrated into a more general perspective. Nevertheless, it should be mentioned that proponents of the ecological view had indicated that a marriage between the ecological and constructivist approach would never work, as their viewpoints are too different and they do not even agree on the object studied. For instance, the radio metaphor has been used to illustrate direct perception: to stress that the neural system detects information that is already present in the environment; whereas the computer metaphor has been used by proponents of the constructivist approach to describe the way in which the neural system construes the outer world on the basis of its projections on the retina. The main criterion by which the value of the target article is to be judged concerns what we may learn from the attempted integration, that is, from knowing that the ventral stream works in a constructivist fashion, and that Gibson's concept of direct perception rests upon the dorsal stream.

Let us focus here on the concept of affordance. According to Norman, it offers the observer invariant, unequivocal information about what he or she can do with part of the visual world. As such, it seems obvious to link it to the dorsal system. Now, what we know of this system is that it is responsible for direct motor actions. D.F. is able to pick up and point towards things, which she cannot describe or consciously perceive. The implication of direct motor actions for the direct perception view is that the detection of affordances may in some way directly invoke appropriate motor activity. That is, whenever we perceive something to step on or something to grasp, "picking up" the affordance would already elicit premotor activity, even if there is no intention to actually perform the action. Interestingly, a recent PET study by Grezes and

Decety (2002) revealed that, irrespective of the task, visual presentation of objects indeed induced activation in motor and parietal areas, suggesting an automatic apprehension of the action afforded by the object. So far so good for Norman's proposal.

However, if the concept of affordance solely maps on the dorsal stream, then dorsal stream patients should not be able to make "affordance" judgements about objects they can describe perfectly well. Vice versa, ventral stream patients should be able to decide upon possible affordances of otherwise unrecognizable objects. That at least the latter is not always so, seems implied by the difficulty D.F. has in visually selecting the correct part of the object to grasp, when the object was presented in an unusual orientation (Carey et al. 1996). Carey et al. argued that D.F. does not directly access functional knowledge of an object. In other words, she does not perceive the affordance of the object. Apparently, some form of affordance coding takes place in the ventral stream. In a recent paper Michaels (2000) – strangely enough, not discussed by Norman – points to the fact that affordances may be both viewer-dependent (egocentric) and viewer-independent (allocentric) affordances, which indeed suggests that mapping direct perception on the dorsal route does not cover the concept of direct perception. In addition, there may be higher-order affordances at an abstract level (for instance, standing on a platform may also have the affordance of becoming famous) which are far away from any spatial representation or motoric activity.

A second concern related to Norman's proposal is the claim that both the dorsal stream and "direct perception" have no memory. However, at least during the acquirement of affordances memory seems to play an important role, as most affordances are assumed not to be present at birth but to develop during a lifetime. In addition, the idea is that the dorsal stream, supporting foremost direct motor activity, builds on coding space towards one's body. Given that our body continuously changes position in absolute space it would not be profitable to use egocentric codes after too much time. There might be situations, however, which ask for longer-lasting egocentric codes. Holdstock et al. (2000) tested a hippocampal patient on an egocentric memory task: to recall the position of a light point in the complete dark, and on a comparable task, but now offering allocentric cues. In the former task, one necessarily has to code the locations in body co-ordinates. The patient showed poor performance on the allocentric memory task, while being within the normal range on the egocentric task, suggesting an egocentric memory system outside the ventral-hippocampal circuitry. Of course, one can debate where in the human brain egocentric (memory) judgements take place: dorsally or more ventrally? It has been argued that there is a qualitative difference between ego- and allocentric judgements of the same location (Sterken et al. 1999), possibly resembling the dorsal-ventral distinction. In line with this, Gallati et al. (2000) asked subjects to judge the position of a vertical bar superposed on a horizontal bar either with respect to their body midline (egocentrically), or with respect to the midline of the horizontal bar (allocentrically). The egocentric condition showed fronto-parietal fMRI activity. Interestingly, the allocentric condition activated a subset of these regions and, exclusively, the hippocampal formation.

Elaborating on the foregoing results, (verbal) judgements of egocentric position and delayed conscious recall of egocentric position might engage the dorsal stream – suggesting that dorsal functions exceed that of direct visuomotor action – whereas comparable decisions on allocentric position depend on the ventral stream (including the hippocampal formation). Holdstock et al. (2000) showed that egocentric location memory lasts for at least 60 seconds. While this still could be within the dorsal stream's "resonance capacity," the type of memory assessed here should be considered as representational. The latter clearly does not agree with the supposed characteristics of the ecological view.

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Berkeley, Helmholtz, the moon illusion, and two visual systems

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Abstract: Berkeley and Helmholtz proposed different indirect mechanisms for size perception: Berkeley, that size was conditioned to various cues, independently of perceived distance; Helmholtz, that it was unconsciously calculated from angular size and perceived distance. The geometrical approach cannot explain size-distance paradoxes (e.g., moon illusion). The dorsal/ventral solution is dubious for close displays and untestable for far displays.

Norman seeks to reconcile the Gibsonian and Helmholtzian approaches by identifying them with the dorsal and ventral visual systems. However, he misplaces Berkeley as being in the same camp as Helmholtz on the question of size perception. He quotes Gibson (target article, sect. 4.1) as denying that the "affordance" of a visual size can only be learned through touch. He adds the rider that this is "a gibe at Berkeleyan empiricism, one of the forerunners of Helmholtzian constructivism." It is true that Berkeley and Helmholtz both believed that spatial perception was indirect (multi-stage) and was influenced by experience; but it is debatable whether Berkeley was a constructivist in the same sense as Helmholtz (Schwartz 1994).

Helmholtz believed in unconscious inferences which were quasi-logical, whereas Berkeley believed in an almost accidental association of ideas through learning. In the case of size and distance, Helmholtz believed that perceived size (linear) was unconsciously calculated in a geometrical manner from image size (angular size) and perceived distance. Most size illusions were therefore caused by perceptual mistakes about distance. He was following a long line of authors in the geometrical-optical tradition, such as Ptolemy, Cleomedes (see Ross 2000), Alhazen, and Descartes. All of these authors describe perceived size as being like a geometrical inference. Berkeley (1709/1948, Section 53) rebelled against this tradition, which he ascribed to the "optic writers." Instead he propounded a "New Theory of Vision," in which we learn by experience to associate certain properties. He was against the idea that we first have to take account of distance in order to calculate size: instead, size and distance judgments were separately conditioned to various cues. His size judgments therefore had one less stage than those of Helmholtz, and that stage was less cognitive. It could be argued that Berkeley's views on size were quite similar to those of Gibson, insofar as Gibson denied that it was necessary to take account of distance (target article, sect. 2.1). Boring (1942, pp. 223 and 298) must take some of the blame for promulgating the idea that Berkeley took a geometrical approach to size perception.

Some size illusions are paradoxical in that cues normally associated with increased distance give rise to perceptions both of increased size and of decreased distance. A prime example is the moon illusion, but neither Berkeley nor Helmholtz saw this illusion as paradoxical. For Berkeley (1709/1948, Sections 68–73), the moon appeared larger on the horizon than in the zenith because size enlargement was conditioned to certain visual and bodily cues, such as aerial perspective and an upright head and body orientation: perceived distance was irrelevant. For Helmholtz (1910/1962, pp. 290–291), the moon appeared *further away* on the horizon (owing to learned distance cues such as aerial perspective), and the size enlargement was caused by our experience of size-distance relationships. However, other authors pointed out that the moon appears nearer rather than further on the horizon, and that this contradicts the geometrical model. In the nineteenth and twentieth centuries attempts were made to salvage the perceived distance account of several paradoxical size illusions, by making a distinction between "registered" and "perceived" distance. The terminology dates from Rock and Kaufman (1962), but the idea was credited to various nineteenth century authors by James (1890/1931, p. 235) and Sanford (1898). The argument usually goes that the horizon moon is (unconsciously) registered as far; it is then enlarged in (linear) size in accordance with size-distance invariance; the enlarged size is then taken as a distance cue (like angular size), and the distance is consciously judged or perceived as near. There are obvious difficulties with the argument, such as the sequential nature of the stages, the implicit shift from linear to angular size, and the uncertain grounds for distinguishing between conscious and unconscious processes (Ross, in press).

Norman suggests (sects. 5.2.3 and 5.2.4) that size-distance paradoxes such as the moon illusion can be "reinterpreted" as a dual process: Size judgments make use of the distance information from the dorsal system (registered distance, which is not "cognitive"), but verbal distance judgments are influenced by the ventral system (and are thus "cognitive"). He cites evidence from many experiments that the two types of distance judgments can conflict. The argument may sound neat, but it is a redescription rather than an explanation of the paradox. Empirical tests are needed to distinguish between "cognitive" and "noncognitive" judgments – otherwise it is too easy to say that any paradoxical perceptions must be contaminated by cognitive factors. The usual distinction is that verbal judgments are cognitive whereas motor reaching is noncognitive. Unfortunately for this rule, the size-distance paradox may sometimes occur when distance is adjusted manually (e.g., Wood et al. 1968; Zinkus & Mountjoy 1969). And of course, motor distance judgments cannot be used at all for very distant objects such as the moon. "Dorsal" and "ventral" measures of perceived size for geometrical illusions at close distances also fail to show a clear discrepancy (sect. 3.3). We cannot at this stage use the dorsal/ventral distinction to explain the moon illusion (or indeed normal size constancy) in a way that unites the approaches of Berkeley, Helmholtz, and Gibson (Ross & Plug 2002). The Helmholtzian constructivist approach remains paradoxical.

Integrating constructivist and ecological approaches

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Abstract: Norman relates two theoretical approaches, the constructivist and ecological, to two cortical visual streams, the ventral and dorsal systems, respectively. This commentary reviews a similar approach in order to increase our understanding of complex skill development and to advance Norman's goal of stimulating and guiding research on the two theoretical approaches and the two visual systems.

Norman advocates a broad definition of "perception" according to which it encompasses "both conscious and unconscious effects on sensory stimulation of behavior," (target article, sect. 1). I advocate more restrictive definitions to foster distinct labels for qualitatively different mechanisms for processing sensory information. I agree with Milner and Goodale (1995) who "see perception as subserving the recognition and identification of objects and events and their spatial and temporal relations" (p. 2). Their use of "perception" includes one's phenomenological experience, in that it enables the development and storage of models of the world. They have explicitly excluded from this definition not only reflexive uses of sensory information, but also some sensory guided actions that they have investigated for skills such as grasping and obstacle avoidance during walking. They highlight the distinction between processes underlying perception and those underlying action. Norman and I endorse this distinction. Norman, however, views his broader definition of "perception" as "more commensurate with [his] attempt . . . to include both approaches, constructivist and ecological, under a common framework" (target article, sect. 1.).

Shebilske et al. (1999) have shown that this goal could be achieved without the broader definition. We have argued for interactive modularity of processes described by the constructivist and ecological theories, but related the theories to explicit controlled and implicit automatic processes, respectively, in a heuristic framework of Explicit and Implicit Learning Ensembles (EILEEN). The explicit controlled processes have serial or hierarchical architectures, and the implicit automatic processes have parallel distributed architectures. This starting point enables researchers to use existing models. The explicit control processes integrate models of attention by Kahneman (1973), ability-motivation interactions by Kanfer and Ackerman (1989), and motor control by Schmidt (1975). The implicit automatic processes expand Shebilske's model of sensory-guided action (Shebilske 1991; Shebilske & Peters 1996), which is based on Pribram's (1991) holonomic brain theory.

Automatic and controlled processes have been operationally defined and dissociated by procedures that measure distinct characteristics, such as automatic processes requiring fewer cognitive processing resources and being less flexible (e.g., Jacoby 1991; Jacoby et al. 1993; Lindsay & Jacoby 1994; Toth et al. 1994). Norman follows Milner and Goodale (1995) in describing many automatic dorsal processes and many controlled ventral processes. The processing systems and cortical systems should not be equated, however, until future research increases our understanding of the interplay within and between both.

Shebilske et al. (1999) presented two null hypotheses to guide this research. The first is that sensations and perceptions mediate explicit controlled processes, but they do not mediate implicit automatic processes, which receive input directly from the proximal pattern, the interface between the physical world and the nervous system. Ecologically integrated stimulus-response contingency rules of implicit automatic processes engage and disengage depending on the presence of a match between the input structure and the system's internal structure. These rules coordinate sensory inputs and effector responses without internal representations of space. Such sensory guided actions have been instantiated by active vision architectures in robotics (Aloimonos 1993). The first null hypothesis proposes that such autonomous sensorimotor modules are contained within a larger framework that includes perceptual representations. Rejecting this null hypothesis would require evidence of perceptual influences that cannot be accounted for by explicit controlled processes.

The second null hypothesis is that interactions between explicit controlled processes and implicit automatic processes are mediated only by their shared influence on effectors. This hypothesis entails three assumptions. The first, common to all neural networks, is that the output of the implicit automatic processes is completely determined by its internal structure and the structure of the inputs. The second and third assumptions are that each kind of process has (a) no direct input to the other, and (b) no direct influence on the other's internal structure. Even with these highly restrictive assumptions, explicit controlled processes would affect the input via effectors, and thus the output, of implicit automatic processes; automatic controlled processes would affect demands on controlled processes. Shebilske et al. (1999) extended theories that emphasize ways in which automatic processes reduce demands on controlled processes (e.g., Ackerman 1987; Anderson 1983; Fitts & Posner 1967; Rasmussen 1986; Schneider & Shiffrin 1977; Shiffrin & Schneider 1977). The extension emphasized ways in which automatic processes increase the importance and complexity of controlled processes (e.g., reasoning and strategic attention control) through expanded potentiality when automatic processes take over lower functions, and demands for backup when automatic processes fail. Shebilske et al. (2000) illustrated these expanded responsibilities in aviation. These expansions entail a vital synergy between automatic and controlled processes, which can occur within the constraints of the second null hypothesis. Rejecting this hypothesis would require evidence of interactions that go beyond shared influences on effectors.

Some of Norman's interpretations of perceptual phenomena rely on the assumptions that the dorsal system shares unconscious information with the ventral system, which makes it conscious, and that experience alters such cross talk. The second null hypothesis would exclude similar assumptions in the EILEEN framework, which could account for the same phenomena by the more parsimonious assumptions that automatic and controlled systems have overlapping inputs, weight the inputs differently, and change the weightings in response to experience.

In EILEEN, sensory guided actions including motor skills, can be mediated or unmediated. Mediation in explicit controlled processes is by perceptions, which are indirect in that they are mediated by top-down processes of expectations based on mental models. The constructivist approach provides a foundation for understanding mediated perceptions and actions. Implicit automatic processes are not mediated. The ecological approach provides a foundation for understanding the rules of engagement of implicit automatic processes in sensory guided actions. The maturity of both approaches provides empirical and theoretical foundations for analyzing the vital interplay between automatic and controlled processes. Norman's framework for investigating this interplay has the advantages of incorporating research on cortical processes and of expanding the range of possibilities for designing and interpreting psychophysical research. Shebilske et al.'s (1999) framework has the advantages of incorporating research on skill development and of specifying controls that will be required in future research.

Two visual systems must still perceive events

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Abstract: Perception of (and during) events is a necessary feature of any perceptual theory. Norman's dual-process approach cannot account for the perception of events without substantial interactions between the dorsal and ventral systems. These interactions, as outlined by Norman, are highly problematical. The necessity for interactions between the two systems makes the distinction useless.

In this target article, Norman makes an attempt to identify two visual pathways with two approaches to perception, ecological and constructivist. We argue that the attempt fails and must fail. The failure is revealed when the dual-process approach requires interactions between the two systems. If the distinction between two visual systems is worth making, then the descriptions of what each does independently had better hold up for most cases. In fact, Norman admits "that often they interact synergistically in . . . perceptual activities." We find that the necessity for interaction of the proposed systems is the norm, and that the described interactions cannot account for normal perceptual function.

Another problem appears in Norman's account of ecological psychology and its applicability to the problems of recognition and identification. If we want to take Gibson's formulation for an ecological psychology seriously, then we must remember that events are primary for perception. This means that perception normally occurs in the course of self-motion or motion of objects in the environment, both of which are events. At no point in the article does Norman address the perception of events, aside from saying that the ventral system recognizes them. The idea that events are primary has two very important implications for any account of recognition. First, objects can and must be recognized in the course of events. Second, events themselves can and must be recognized. We find that a simple analysis of the constraints on event perception reveals many inadequacies in the proposed approach.

Norman provides many instances where the two systems would not act in isolation, but leaves the impression that this is the exception and not the rule. Upon examination it can be seen that interactions between the systems must be the rule and not the exception. The case of event perception makes this very clear. Humans are very adept at recognizing patch-light events. We can even identify individual people using only this motion information. When the motion stops, the display appears merely as a random-dot display. According to the dorsal-ventral distinction, motion is processed primarily by the dorsal system, which includes areas MT and MST. The only way for the dual-process approach to account for our ability to recognize events and objects in patch-light displays is through an interaction of the two systems. Information must be transmitted from the dorsal system to the ventral system to allow for recognition and identification. This raises several questions about the proposed interactions of two separate systems. How can the dorsal system transmit information to the ventral system if they use different information, that is, if the ventral system uses constructivist cues and the dorsal system uses ecological information? If the dorsal system has no stored representations, then what does it give to the ventral system? Furthermore, if the dorsal system uses an egocentric framework and the ventral system uses an allocentric framework, how is the information translated between the frameworks? If we go on to assume a coupling between the systems that provides an adequate translation, then why should we assume that this coupling is ever inactive, given that our surroundings are typically populated with recognizable events? Clearly, dissociation between these two systems cannot be the norm.

There are two cases in which objects are recognized in the course of events. Both are problematic for the dual-process approach. In the first case, objects must be recognized while they are in motion relative to a static observer. Consider an example of an everyday task: search. If I am waiting to get my luggage at the baggage claim after a flight, what is required of my visual system?

First, I must recognize my suitcase. As luggage streams out of the hatch, I perceive many different suitcases sliding down onto the conveyor belt. This is the perception of events. I do not recognize suitcases and then detect motion. I perceive "suitcases sliding onto the conveyor belt." What information available in these events specifies a suitcase? There is a wealth of literature that demonstrates our ability to perceive shape or structure from motion. This is similar to patch-light recognition, in that perception can be based on motion only. The dual-process approach cannot adequately account for this essential ability.

There is an additional problem for recognition in the course of an event. As my suitcase is carried towards me, I must recognize it while it moves, and must therefore track it with my eyes. What information enables me to track (an inherently spatial task) the suitcase? It must be information within an egocentric framework, as tracking may involve moving not only the eyes, but the head, body, or other objects as well. According to Norman, this egocentric framework is subserved by the dorsal system. Then again, tracking means keeping an object foveated. As a stream of suitcases moves past me in unison, I track one at a time. How do I track a given suitcase without recognizing that it is a suitcase? If recognition is accomplished via the ventral system, then that system must also drive the recognition task. At some level I must recognize which events qualify for visual tracking in the first place. After all, I do not bother examining the trash can by the wall or the man walking past me to see if he is my suitcase. According to Norman, this task is accomplished by the ventral system in an allocentric framework. There is nothing gained by the suggestion that recognition is the domain of the ventral system.

Recognition must also occur during self-motion. Self-motion is by all accounts representative of perceptual functioning, as Gibson made clear. Self-motion is also an event, during which recognition must occur. As I walk to the baggage claim past stationary people, I do not need to foveate them individually to recognize that they are people. Yet, they are projecting optic flow structure into my eye. As I approach my suitcase I am able to recognize it. The event of approaching a suitcase is very complicated visually.

As I approach, the image expands and translates on the retina. It is well known that self-motion enhances the perception of space, but how does this work for an allocentric ventral system? To perceive size based on an expanding image there must be some information about the approach; however, this calls for an egocentric framework.

Norman's dual-process approach fails to adequately account for the recognition of events. We can and must recognize events to interact successfully with the world. If I am waiting for a second piece of luggage, I will notice if someone else takes my first suitcase. I could recognize this event when detected in the periphery. People walking past me and picking up other objects will be recognized as that, but when my suitcase is taken it will be recognized as such and will yield rapid action to interfere with the event and to retrieve the suitcase. This ability cannot be adequately explained with Norman's dual-process approach, except by such thoroughgoing and complete interactions that the distinctions between the two systems are irrelevant.

Of course, Norman does suggest that the two visual pathways often act together, or even mimic one another. The problem is that this explanation is an attempt to repair a fundamental flaw in the idea. The abilities and qualities that Norman wants to segregate in different regions of the visual system cannot be easily separated. Additionally, Norman never addresses an ecological account of recognition and identification (of events and objects [in events]) which requires the detection and use of information.

Norman's dual model in a broader context

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Abstract: This commentary suggests how Norman's dual control model of vision can be fitted into a broader general model of the control of behaviour by direct (on-line) and indirect (off-line) processes. Some general principles of behavioural organization, development, and competition are described and their specific application to vision is noted.

I welcome Norman's valuable contribution. This commentary is an appeal for integration and, as such, sets Norman's argument into context and looks at its broader relevance. I developed a model of the control of behaviour that has close parallels with that of Norman (Toates 1998) and related it to vision (Toates 2001). However, sometimes we cannot see what is before our eyes and, prior to reading Norman, I was still left wondering how on earth vision could work the way that Gibson described. Norman shows how a Gibsonian process can coexist with a more top-down "off-line" system.

There is an analogous argument to that of Norman in the area of rat learning. The old Hull/Tolman controversy concerned whether rats learn fixed stimulus-response connections or cognitions, and was a debate on *theoretical perspective.* It now seems that these perspectives can be reconciled on the assumption that animals tend to learn according to both principles in parallel, reflecting two types of *process* (Hirsh 1974; Mishkin et al. 1984; Toates 1998; White 1989).

In fact, Norman's kind of dual control model has an application to such diverse areas as attention (Rafal & Henik 1994), reasoning (Epstein 1994; Sloman 1996), attitudes (Wilson et al. 2000), moral judgments (Bolender 2001), and emotion (LeDoux 1995), and is revealed in the acquisition of fears and phobias (Jacobs & Nadel 1985) and stress (Toates 1995). This suggests a strong and broad selection pressure to favour dual control. Nature organizes a quick stimulus-driven solution where such a solution can be specified but has off-line controls that modulate its sensitivity and monitor its outcome. These can take over some additional responsibility when on-line control cannot solve the problem. Sometimes the quick on-line solution can only exert control after the off-line system has solved the task for some time.

Based largely upon a consideration of broad principles of behavioural control, I would like to suggest a number of aspects of dual control that are worth exploring, as follows.

Goal-setting. Clearly it would soon land us in trouble if, equipped with an array of Gibsonian processes, we were to go around responding rapidly to everything that offers an affordance! Thus, one would speculate that high-level goals, a feature of off-line processing, and probably involving the prefrontal cortex and ventral stream, need to modulate the sensitivity of links between visual stimuli and behaviour (involving the dorsal stream) such that, at each moment, behaviour fits goals (cf. Houghton & Tipper 1995; Milner & Goodale 1995). Goals involve semantic processing and would "permit" appropriate Gibsonian mechanisms to operate.

In addition to cortical processing, subcortical processes mature early and provide a rapid means of triggering certain actions based upon the physical properties of stimuli (Bronson 1974; Schiller 1985). By means of the cortex, evolution builds on, refines, and modulates the control exerted by brainstem and basal ganglia processes.

Memory. Norman suggests that the ventral system is "the memory based system" whereas the "dorsal system appears not to have a long-term storage of information." I suggest that the dorsal system acquires *implicit* memories, encoded in gradually modifiable links between stimulus situations and the appropriate responses.

Cooperation and competition. Under some conditions, could there be competition between dorsal and ventral streams? Some hints suggest this, one, if only analogous, example being the Stroop effect (Stroop 1935). Yantis (1998, p. 251) marshals evidence for dual factors in showing that attention is controlled by "an interaction between the observer's intentions and the properties of the image." Whilst engaged in a task such as reading, both attention and eye movement control can be captured by the introduction of a new stimulus into the visual field (Theeuwes et al. 1998).

Phylogeny. As a general principle, off-line processing is believed to be a development of the evolutionarily older on-line processing, a principle that applies to the visual system (Milner & Goodale 1995, p. 65).

Development. With development, there is some shift of weight from on-line to off-line processes. This corresponds to a shift from subcortical to cortical controls (Bronson 1974). In the control of eye movements, cortical systems acquire an increased ability to modulate the control organized at the level of the superior colliculus (Braddick et al. 1996). By analogy, *within* the cortex, it seems likely that the ventral stream acquires a greater influence relative to the dorsal.

On-line processes seem to play a crucial role in the development of off-line processes. Thus, subcortical controls of eye movements bring the neonatal fovea into alignment with salient stimuli, even though the cortical system is only in a rudimentary state of development as far as the ability to process this information is concerned (Bronson 1974).

Adults are able voluntarily to direct attention to less salient features of an image, whereas new-borns are more strongly under the control of visual stimuli (Holtzman 1984; Johnson 1995). The adult ability involves exerting inhibition on the tendency for the most salient feature to capture attention, for example, by eye movements (Bronson 1974; Johnson 1995). Bronson (1974, p. 879) suggests:

> all neonatal reactions can satisfactorily be explained by reference to automatic mechanisms, hence it is not necessary to assume volitional control in the new-born infant.

This gain of some autonomy from the control by physical attributes of the world suggests an increase in weight of processing by the ventral stream relative to the dorsal stream and subcortical mechanisms.

Pathology. Damage to regions of the frontal cortex in adult humans is followed by a difficulty in suppressing automatic saccades and initiating eye movements directed to a goal that is not yet visible (Guitton et al. 1985). Unlocking of gaze can be difficult.

On the development of the two visual systems

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Abstract: Norman's reconciliation of the two theories of perception is challenged because it directly leads to the nature-nurture dichotomy in the development of the two visual systems. In contrast, the proposition of a separate development of the two visual systems may be better understood as involving different types of information that follow a distinct temporal sequence.

Like Norman, we interpret the empirical evidence as being supportive for the existence of two distinct visual systems differing in their functions. The ventral system's primary function is perception of objects and self, and appears, as convincingly shown by Norman, particularly sensitive to object-centered, monocular pictorial information. The dorsal system, on the other hand, is mainly involved in the visual control of goal-directed movements, and appears specialized in the detection of body-centered, binocular and monocular motion information (Milner & Goodale 1995). Considering the development of the two visual systems, a logical proposition would be that the dissociation is present from birth, and that the two visual systems follow different developmental trajectories (Van der Kamp & Savelsbergh 2000). So far this proposition has not been systematically tested.

Norman, however, makes the strong claim that the development of the two visual systems follows different principles. He argues that the use of visual information in the perception of objects has to be learned, whereas the use of visual information to control goal-directed movements is claimed to be innate. This nature-nurture distinction is of course a thorny issue in developmental psychology. Coupling nature and nurture issues to the supposed separate developmental trajectories of the two visual systems may therefore do more harm than good. Spelke (1998) defines the nature-nurture debate as a continuum between two positions: at the one extreme (nature), the infant's object perception emerges entirely by virtue of intrinsic processes of growth, independently of any specific encounter with the object. At the opposite extreme (nurture), perception is entirely shaped by children's encounters with the object.

It was Fantz (1961) who pioneered preference looking techniques as a way to study the development of the perception of, among other things, the human face. Although Fantz himself concluded that only three-month-olds could discriminate a human face from a scrambled one, later studies using moving face-like figures have shown that even infants who averaged 9 minutes from birth distinguished between a human and a scrambled face (Goren et al. 1975). These and other studies demonstrate that a newborn baby perceives human faces, but possibly also motion, size, and shape at its very first (experimental!) encounters with objects (Kellman & Arterberry 1998).

Whereas the use of information for perception is not *by definition* learned, the use of information in the control of goal-directed movement is not *by definition* innate. Kayed and Van der Meer (2000) have recently examined the visual information that 5- to 7-month-olds use to time defensive eye-blinking. Infants were presented with a looming image that approached under different constant velocities and constant accelerations. The youngest infants timed their blinks at a threshold visual angle. This timing strategy worked well in the case of constant velocities but resulted in late blinking under the fastest accelerative approach condition. The older infants, in contrast, geared the timing of their blinks to the inverse of the relative rate of change of the visual angle (i.e., tau). Hence, the older infants had learned, perhaps by a process of selection based on previous encounters with similar events, to use information specific to the time to collision that made it possible to cope with all approach conditions.

These counter-examples do not disprove Norman's claim, just as demonstrating that some aspects of perception are indeed learned cannot prove his claim. It does underline, however, that in pursuing the proposition that the two visual systems develop separately, the nature-nurture dichotomy is not a very fruitful avenue to follow. Further, questioning the nature-nurture distinction directly challenges the profitability of the dual-process approach from which Norman derived his claim in the first place. That is, the dorsal system functioning according to ecological processes, and the ventral system functioning according to constructivist processes.

If not from a reconciliation of two theories of perception, how then should the putative dissociation of the two visual systems in development be assessed? Framed within the ecological approach, the development of the two visual systems can be understood as following different trajectories because it may involve different types of information in different temporal sequences. Consider, for instance, infants dealing with information about direction of motion for perception and for action, respectively. Wattam-Bell (1996) shows that the earliest perception of direction of motion at 10 weeks of age is exclusively based on information about the relative motion of an object in relation to its background. That is, early perception is exclusively based on object-centered information and thus is probably supported by the ventral system. It was found that only several weeks later infants' perception of direction of motion was affected by body-centered information about the object's absolute direction of motion in the absence of a background. In contrast, von Hofsten (1983, p. 84) has argued that in the development of the control of early reaching ". . . the infant reaches in reference to a coordinate system fixed to the moving object instead of to a static background." That is, early reaching is primarily geared to body-centered information and thus probably uniquely supported by the dorsal system. These studies demonstrate that insight into the proposition that the two visual systems develop separately can be gained by assessing the differential involvement of body- versus object-centered information in early action and perception (or by assessing the differential involvement of binocular and monocular motion information and monocular pictorial information). At the same time, these studies also illustrate that the information-based distinction is not an absolute one. At a particular phase in development both types of information may merge (Milner & Goodale 1995). We are convinced that such merging of information in the development of action and perception can be understood more parsimoniously by the relative contribution of different types of information, than by the interaction of processes that operate according to contrasting ecological and constructivists principles, as Norman would have it.

Author's Response

Adequacy and utility of the dual-process approach to perception: Time (and research) will tell

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Abstract: My response and reactions to the quite diverse commentaries are presented. Among the topics covered are a response to holders of the ecological viewpoint; memory and learning in the two perceptual systems; development of the two systems; biological motion; size and distance perception; illusion and the two systems; and several others. It is suggested that the dual-process approach is a viable working theory of space perception and, perhaps, of other types of perception as well. Hopefully, future research will enhance it with added refinements and variations on the original theme.

The commentaries cover a wide range of diverse topics, many of them quite specific, with relatively little overlap. I will begin (sect. R1) by responding to three commentaries that offer a general critique of the proposed dual-process approach. This will be followed (sect. R2) by my responses/reactions to specific topics addressed by individual commentators. More general responses on common topics that several commentators dealt with will follow (sect. R3). Finally, a short summary statement will be presented (R4).

R1. General critiques of the dual process approach

Braunstein offers an alternative means of reconciliation between the two theoretical approaches, the constructivist and the ecological, instead of the dual-process approach. He suggests that Observer Theory (Bennett et al. 1989; 1991) can serve as a means of bridging the two approaches. This theory is based on quite difficult mathematical concepts that even mathematically trained psychologists find difficult to follow (see Laming 1991). That is, perhaps, the reason it has not yet had a major impact on research and theories of perception. From Braunstein's commentary we learn that Observer Theory consists of a formal theory of perception that is based on inductive inference. Braunstein notes that these inferential processes need not worry the proponents of the ecological view, as it is "completely separated from intelligence, thought, and active use of knowledge, and is allowed to encompass smart mechanisms and resonance, there is no need for direct perception theorists to object to inference." He goes on to suggest that percept-percept couplings can be interpreted in the light of Observer Theory as "perception is based on a chain of inferences, with the conclusions of one inference becoming the premises of another." These concepts may or may not be palatable to the ecologically oriented, but even if they are, does the alternative approach suggested by Braunstein have any advantage over the dual-process approach discussed in the target article? I argue just the opposite; it has less to offer than the dual-process approach.

Using a common formal mathematical definition of inductive inference might serve as a bridge between both theoretical approaches, but the ultimate question is whether or not it will prove fruitful. The ultimate goal of the target article was not simply to find a means of reconciling the two theoretical approaches; rather, this was a vehicle for finding a broader, more encompassing theory of perception, and specifically, space perception. In section 5.2 of the target article, several examples of the usefulness of the dual-process approach in explaining visual perception phenomena were outlined. In contrast, what is the usefulness of formalizing both kinds of theoretical approaches with the aid of Observer Theory? Showing that both types of contrasting theories can come under a singular broad theory is certainly an important exercise, but to what extent this helps explain phenomena or predict new outcomes is not that clear.

Many adherents of the ecological approach believe that it can explain all perceptual and cognitive phenomena, and that the constructivist approach is simply misplaced dualism. Thus, it is understandable that holders of this view are troubled by the legitimacy bestowed on the constructivist approach by the dual-process approach.

Several commentators take a strong ecological stand (**Burke & Hayward**, **Cooper & Michaels**, **Mace**, and **Shull & Bingham**). Two of these, Mace and Michaels, are prominent spokespersons for this approach, and although their commentaries are quite different in emphases, both deal with the dual-process approach in its entirety. Therefore, I will respond to them here. The other two commentaries focus on specific points and will be responded to in the appropriate sections below (see sects. R3.1 and R3.4).

Cooper & Michaels' commentary is totally committed to an extreme version of ecological psychology lacking any tolerance for any deviations from the "orthodox" view. To their mind, the constructivist approach is "not a theory of perception at all but a confusion of perceptual and post-perceptual processes." Sadly, this commentary is completely oblivious to an important interchange between Michaels and myself on this issue (Michaels 2000; Norman 2001; Michaels et al. 2001; Norman 2002). For example, as in the above quote, Cooper & Michaels several times intimate that what the constructivists call "perception" is not really perception at all. While it might be said that the ventral system is more "cognitive" than the dorsal system, the labels given the various processes can vary with an author's whims. It is somewhat surprising that these commentators chose to ignore the quote from Gibson' *Purple Perils* (1976) entitled "What is it to perceive?" that I included in my response to Michaels (2000). Gibson wrote:

> The verb *to perceive* has two meanings, one being that of ordinary usage and the other coming from a puzzle in philosophy and psychology:
>
> 1. To perceive X means simply to be in touch with it, to be able to cope with it, or to be aware of it in the environment.
> 2. To perceive X means to have an *experience* corresponding to X, or *percept* of it, or a content of awareness, or of consciousness. This implies that there is a *mental* X besides the actual X. The second meaning is troublesome.
>
> The two meanings need to be kept separate in the investigation of perceiving. The act of a perceiver and the content of his mind should not be confused.

Gibson was more interested in the first of the two meanings and may have felt that the second "is troublesome," but he clearly indicated that he was cognizant of the second meaning and unlike Cooper & Michaels he was willing to call it "perception"!

Cooper & Michaels also assert that I was motivated "to *relegate* ecological psychology to the dorsal system" (my emphasis). This was the tone in Michaels (2000) and I tried to alleviate her fears in my response – as the "Not to worry!" in my title indicated – but was obviously unsuccessful. That response explained that the pickup of visual information by the dorsal system is the brunt of perceptual activity in both humans and animals. What is more, I had pointed out that Michaels and her colleagues had researched topics that were all from the dorsal domain.

There is an apparent contradiction in **Cooper & Michaels**' commentary here: On the one hand they want to relegate many aspects of constructivist/ventral perception to realms other than perception, yet they see the ascribing of ecological perception to the dorsal system as a "relegation." This contradiction results, perhaps, from the belief of some (if not all) members of the ecological camp that they have an alternative explanation for the ventral phenomena of recognition and identification. This belief is expressed in the last sentence of the commentary by **Shull & Bingham**, who write, "Additionally, Norman never addresses an ecological account of recognition and identification (of events and objects [in events]) which requires the detection and use of information." Unfortunately, they did not elaborate on this sentence and it is difficult to understand just what is meant by it. Certainly detection and use of information are involved in recognition and identification, but just how are they involved? From Shull & Bingham's remarks it is not clear just what it is that is detected, or that detection is the underlying process. When I see my wife across the street I recognize and identify her, but this is quite different from my detecting some movement across the street. Recognizing my wife entails some process that lets me know that this person is someone familiar, indeed, none other than my wife, and this process is more than mere detection. (For more on this topic, see Norman 2002). On this point **Cooper & Michaels** write:

> it is only memory-as-stored-representation to which we object. We do not believe that one needs a qualitatively different theory to explain the education of attention to information that specifies that a bird is a chickadee or that a person is Fred. Identifying a chickadee or recognizing Fred is evidence that the education of attention has occurred; it is only spooky assumptions about time that entail the storage metaphor.

Perhaps it is the type of "representation" posited in constructivist theories, say, some pictorial simulacrum, that is bothersome for them. In which case I see no problem in positing a less "mentalistic" type of storage such as that posited in connectionist models. However, their total disapproval of "the storage metaphor" and their suggested "education of attention" as the means of perceptual learning (see discussion in sect. R3.1), indicate that theirs is a somewhat irrational fear of all processes which seem to entail some semblance of mental processing. My hope that the dual-process approach would allow members of the ecological camp to live with the specter of "mental processes" has, at least in this case, not been realized.

The central point of **Mace**'s commentary is that I do not do justice to the ecological endeavor, that of creating a psychology true to the tenets of pragmatic realism. He correctly assesses my positive attitude to the realist position, but feels that my outline of the ecological position and my attempts at reconciling it with the constructivist approach miss out on the central issues that drive ecological thinkers. Not being trained in philosophy, I have trouble with some of the goals the realist ethic sets. As I understand it, I am fully in agreement with the realist proposal that the environment exists "out there" with or without our perception of it. In other words, things exist without the need of their being in one's "mind." But their existing without being in the mind does not, to my understanding, necessarily imply that we do not have a "mind" that responds to, and processes, those objects or events in the environment. In other words, I find it difficult to completely eliminate "mental processes" from a cognitive psychology. In the specific case of "perception," one's interpretations depend, to a great extent, on one's definition or understanding of that term. In his chapter on Gibson's ecological approach, Mace (1986) writes, among other things, that "to identify correctly based on perceiving is not perceiving." Here he is limiting perception to dorsal system perception, as outlined in the target article. He also writes, "to perceive is not to experience something occasioned by a stimulus." If we look back at the quote from Gibson's *Purple Perils* in the response to **Cooper & Michaels** above, it is obvious that Gibson was willing to accept a broader definition of perception, although he noted that the second type of perception "is troublesome." It is indeed troublesome if one insists on maintaining a strict pragmatic realist position.

My feeling is that some of the problems crop up especially when ecologically oriented theorists attempt to deal with certain "more cognitive" aspects of perception. Take, for example, the problem of recognition and identification. One could say that this is not perception, as in the quote above, but where does that lead us? – probably not very far. Giving things another label is not much of a panacea, as the process with the new name has to be explained as well. What is more, it is hard to accept the suggestion that recognition and identification are not perception, even if we adopt an ecological definition of perception such as the one given by Bruce et al. (1996):

> In order for its movement to be regulated by the environment, an animal must be able to detect structures and events in its surroundings. We call this ability *perception*, and it in turn requires that an animal be sensitive to at least one form of energy that can provide information about the environment. (p. 3)

Take **Mace**'s example of recognizing his acquaintance, John, where he suggests that recognition/identification is nothing more than coming up with a name. It would seem that he is underplaying the situation. First, recognition is not always accompanied with the recall of a name; it might simply be that that person is familiar; for example, he is a teacher in my son's school. What is more, in terms of the definition above, the human animal might recognize the human body not far away as the runaway prisoner from the local jail who is known to be armed and dangerous, and this will regulate his movement in a direction away from that body. Thus, there is more to recognition than coming up with a name. Also, there is a great deal of evidence that points to the fact that stimulus familiarity influences visual perception quite early in the processing of visual information. Familiarity has been shown to enhance performance in a wide variety of speeded visual tasks, such as visual search (e.g., Shen & Reingold 2001), perceptual grouping (e.g., Kimchi & Hadad 2002), and mental rotation (e.g., Koriat & Norman 1985). These familiarity effects must be based on some kind of storage of previously encountered stimuli, and the fact that they occur very early in the perceptual process appears to argue against the claim that they are postperceptual.

All in all, while one cannot but admire the ecologically oriented theorists striving to rid psychology in general, and perception in particular, from "mentalistic excess baggage," the current state of the art leads one to conclude that one still needs to posit some sort of mind-like activity to explain processes like recognition and identification. It is difficult at present to find a tenable alternative to some sort of storage of information to explain these ventral processes. However, that storage need not be a representational replica; a connectionist type of alternative such as some distribution of weights and activations that change with experience will serve the purpose just as well.

R2. Responses/reactions to some topics raised by individual commentators

Neisser made a proposal similar to that suggested here many years ago (Neisser 1989) so that it is not surprising that he substantially agrees with the ideas voiced in the target article. He takes exception to two claims made in the target article and points to a topic that was neglected. He disagrees with the suggestion that the dorsal system is the faster of the two and notes that humans are capable of very fast reading speeds; and, of course, reading is carried out in the main by the ventral system. While the ventral system is capable of speedy processing of well-learned materials as in reading, it still seems that the dorsal system is the faster of the two. There are two main reasons for saying this: first, we know that the transmission speed of the magnocellular pathway that feeds the dorsal system is faster than that of the parvocellular pathway, which is a very important component in ventral system processes. Second, there are several studies that have compared the speed of a motor response to that of a judgmental response and found the former to be faster (see target article, sect. 3.3). Also in those situations that require very fast actions, such as when one quickly raises an arm to protect one's head from a ball batted directly at it, that action is totally under dorsal control. Perhaps, the answer lies in the possibility that ventral responses can only be very fast when they are highly overtrained, whereas when a very fast response is required in a novel situation it must be processed by the dorsal system.

Neisser also disagrees with my contention that the dorsal system functions unconsciously in the main. This is dealt with in the section on consciousness below (see sect. R3.6). He also notes that I neglected the topic of the perception of the self. This was simply because I found it beyond the bounds of the topics that I could handle in the target article. **Mace** notes, as well, that according to Gibson "To perceive is to be aware of the surfaces in the environment and of oneself in it" (1979/1986, p. 255). Clearly, relating and tying in the perception of the environment with the perception of the self will further increase our understanding of perception in general, but this is beyond the scope of the current exposition.

Hochberg, the doyen of present day perceptual theorists, finds the dual-process approach "a more workable metatheory than each taken separately," referring, of course, to the constructivist and ecological approaches, but expresses his disappointment with both. This disappointment stems first from the inadequacy of the constructivist approach, both in its breaking down of perception into small independent units and in its positing of unconscious inference processes that "rebuild" the distal stimulus. Hochberg reminds us that perception is of patterns, that it depends on ongoing activities in the brain, and that most of the couplings suggested by the constructivist approach (size-distance, shape-slant, etc.) cannot be validated empirically. His disappointment with the ecological approach stems from its "idiosyncratic avoidance of neuroscience" and, more importantly from his viewpoint, its neglect of "active saccadic looking." He then spells out the importance of the understanding of saccadic eye-movements for a more complete theory of perception.

Hochberg's approach does not fit either of the two approaches outlined in the target article. On the one hand, his title suggests that he would prefer a direct theory, but one including spatial exploration with the aid of eye-movements. On the other hand, he writes, "the construction of some simple object examined with different foci of attention . . . depends on the viewer's schemas and choices." Those "schemas and choices" would certainly not go well with the orthodox ecologists. As for the dual-process approach being suggested here, the incorporation of the idea of saccadic spatial exploration and information intake is not well developed, and there is a need to incorporate it as well.

Toates' commentary does us the important service of showing how the dual-process approach outlined here is but one of many such dual-control models that serve in the explanation of a wide variety of behaviors. His is an impressively broad foray into very many diverse areas where he briefly illustrates such models. As the points he makes are all quite brief it does not make any sense to paraphrase them, and I can only recommend his commentary to those readers who might have skipped it. For me it was exciting to realize that ideas similar to those of the dual-process approach to space perception have been shown to have explanatory value in so many other domains of psychology. One domain that he did not mention, that of social psychology, as I discovered, also has its "dual-process approaches" (see sect. R3.9). The first example he lists is the very old Hull/Tolman debate on how rat learning transpires, through S-R connections or cognitive maps, informing us that there are other current approaches that incorporate both approaches and indicating that both types of learning occur in parallel. Another dual process model of learning and memory has been proposed by McClelland et al. (1995). This model is noteworthy as it combines connectionist models of learning with physiological studies of brain functions. Briefly, it posits two brain systems for learning, a fast learning hippocampal system and a slower neocortical system.

In another enlightening commentary, **Chella** points to the parallels between the two perceptual systems in the dual-process approach, and the two vision systems that are necessary for the building of intelligent robots. One robot vision system picks up information from cameras enabling navigation and obstacle avoidance. It parallels the dorsal system suggested here. The second system, the object recognition system, utilizes high-level algorithms to identify and recognize objects of interest in the robot's environment. It parallels the ventral system. Chella describes some of the interactions between the two robot vision systems in both static and dynamic scene analysis. His analysis is based on conceptual spaces (Gärdenfors 2000), where the information from the two vision systems is combined. We have little understanding of how such combination occurs in biological visual systems and Chella's commentary reminds us that there is room for more cross-fertilization between those studying artificial vision systems and those studying biological systems. Interestingly, Chella notes that when the robot chases a rolling ball it must have information ("knoxels") both about the moving ball and about itself. This is reminiscent of **Neisser**'s (see above) call for the perception of the self in the dual-process approach. Finally, toward the end of the commentary Chella mentions "high-level conceptual affordances." This use of "affordances" differs from that consistent with dorsal system function, as will be elaborated on below (see sect. R3.8).

In his commentary, **Ellard** discusses some parallels between the dual-process approach and some of his findings on Mongolian gerbils. He notes that while dorsal system functions such as action control are of primary importance in the vision of rodents, his research has also indicated a ventral system-like function in the gerbils. This occurs when gerbils learn to scale the sizes of objects to allow them to jump long distances. This appears to be a variant on what has been called the familiar size cue in human perception, and is a ventral system cue as it depends on the past experience with given objects. He also notes that in other tasks, such as braking during a running task, Gibsonian invariants most probably play a role. Thus, we have dual processes in the gerbil as well. He also notes that the relevant brain areas in the gerbil are not homologous to those of the primate, leading him to conjecture that "the evolutionary antecedents for different modes of perception preceded the anatomical parcellation." It is interesting, but not surprising, to learn that rodents like gerbils possess two visual systems. My hunch is that this is true of all animals with a cortex, but one wonders about lower animals. **Toates** writes in his commentary that, as a general principle, the dorsal system is the older evolutionarily, and this makes sense. An interesting question is, how far down the phylogenetic scale must we go to find organisms that possess only a dorsal system?

Gentaz et al. look for parallels to the dual-process approach in haptic or tactile perception. They note that a constructivist-ecological dichotomy also exists in theorizing about touch (see the brief quotes from Heller 1997, an expert on touch, at the end of target article sect. 2.3), and they wonder whether it can be reconciled in a manner similar to that of vision. They note that in passive touch one finds a double dissociation similar to blindsight (labeled "numb-sense" or "blind-touch"), and this would indicate that there are two touch systems, as in vision and as in the recent reports about two auditory systems (e.g., Alain et al. 2001; Romanski et al. 1999).

Gentaz et al. describe their current research attempts at finding a similar dissociation for active touch (haptics). They are looking for a dissociation in a tactile version of the Müller-Lyer illusion, a somewhat difficult task since all the responses involved are motor ones. I hope they will obtain results suggesting that the dual processes are common to all the modes of perception.

Ingle was one of the first suggest the existence of two visual systems (Ingle 1967). In his commentary, he corrects and adds details to my description of the early work on that idea. I am very grateful to him for this. He goes on to point out various functions of the parietal cortex, and suggests that I underestimated the role of the parietal cortex in conscious perceptions. Two comments in response here: first, the dual-process approach is not a physiological theory, but rather, an attempt to order disparate perceptual findings and theoretical approaches, and, thus, no claim is made as to all the possible functions of the parietal cortex. Most probably, not all of the parts of the parietal cortex are part of the dorsal system. Second, as is pointed out in section R3.6, the question of the locus of consciousness is complex as there is continuous cross-talk between the dorsal and ventral systems, and the fact that subjects are capable of reporting a dorsal function does not necessarily imply that it occurred consciously. Ingle also briefly describes work on adaptation by Held and Kohler. As Neisser (1989) pointed out, studies of adaptation give further credence to the two perceptual systems idea, where participants report adaptation of dorsal functions after much experience with the distorting optical system, but this occurs before adaptation of ventral functions. Thus, for example, participants having a lot of practice with their left and right visual fields reversed are capable of cycling among cars but report that the letters on the license plates remain reversed. Studies of perceptual adaptation have for some strange reason "dropped out of fashion," with a few exceptions (e.g., Redding & Wallace 1997), but there is room to return to the many older studies of adaptation and look at them in the light of the dual-process approach.

Ingle also raises the question of whether stereognosis, the recognition of objects by touch, is a dorsal or ventral system function. Stereognosis is known to depend on parietal cortical function, as patients with damage there appear to lack it. But as it also includes recognition the ventral system is involved as well. Recent research (Amedi et al. 2001) appears to strengthen the idea that both systems are involved. Amedi et al.'s subjects' task was to identify objects both visually and haptically while fMRI imaging was carried out. One of their findings was a robust and consistent somatosensory activation in the occipito-temporal junction, a nonretinotopic region in the ventral visual pathway. They raise the possibility that this area is human homolog of the monkey's inferotemporal cortex. Ingle also describes the research of Trevarthen and Sperry (1973) on the ability of split-brain subjects to compare items presented to opposite visual fields. Interestingly, these subjects could not compare colors or shapes, ventral system functions, but "could say which object was 'higher or lower,' 'approaching or receding,' and whether a given hemifield contained one or two moving objects" (**Ingle**, this issue). The attributes that could be reported are all dorsal system functions, appearing to indicate that the dorsal system can transfer information between hemispheres via its subcortical structures, and that information becomes available to the ventral system for report.

Shebilske's commentary calls attention to parallels between the dual-process approach to perception and the broader topic of complex skill acquisition. Shebilske et al. (1999) proposed a hybrid model called Explicit and Implicit Learning Ensembles (EILEEN), which contains both explicit controlled processes and implicit automatic processes. The commentary undertakes the difficult task of very briefly summarizing some of the ideas in that paper. There are, of course, clear parallels between the explicit processes and the ventral system, and between the implicit processes and the dorsal system. Shebilske et al. also suggest that the explicit controlled processes are serial and that the implicit automatic processes are parallel. This distinction has become more blurred over the years, and it is doubtful whether one would want to say that ventral processing is serial, or that dorsal processing is parallel, so categorically – although it would make sense to suggest that dorsal processing is more parallel than ventral processing. Two of four "null hypotheses" in the paper are briefly mentioned in the commentary. One is that inputs to implicit processes are not mediated by sensations and perceptions, in contrast to explicit processes where they play an important role. Shebilske notes that he does not accept my "broad definition" of perception, but one might assume that the term "sensations" covers a much broader range of inputs, and thus it is difficult to envisage automatic implicit control processes where sensations do not play a role. That these are labeled "null hypotheses" might imply that they are meant to be rejected, but reading Shebilske et al. (1999) one understands that this is not the case. They write,

> The ecologically integrated stimulus-response contingency rules automatically engage when there is a match between the input structure and the internal structure, and automatically disengage when a match no longer exists. . . . The process of engaging these rules is consistent with Gibson's ecological optics (1979/1986), but is not consistent with his direct perception because the system operates without perceptions. Sensory inputs and effector responses are coordinated without internal representations of space.

If, when writing "the system operates without perceptions" they are referring to "internal representations of space," Gibson and all ecological oriented thinkers would most probably agree, as they do not accept the notion of internal representations. However, the idea of stimulus inputs that do not elicit sensations or perceptions, especially when those inputs are supraliminal, is difficult to accept. It is not clear whether the problem here is one of semantics alone, or one of truly different conceptualizations of the underlying processes. The dual-process approach would simply suggest that in the case of implicit processing the perceptual processes transpire in the dorsal system, which functions without representations but certainly includes an input stage – label it sensation, pickup, or perception, as you like.

Shebilske's second null hypothesis deals with the interactions between the two types of control processes, suggesting that such interactions are "mediated only by their shared influence on effectors." As the dual-process approach deals with perception that does not necessarily entail a response by some effector (this is also true of dorsal perception!), it is difficult to draw a parallel between this claim and the dual-process approach. However, let me look at the three assumptions that this hypothesis entails in the light of the dual-process approach. According to Shebilske, the first is that "the output of the implicit automatic processes is completely determined by its internal structure and the structure of the inputs." This can be converted to: "the perceptions of the dorsal system are determined by its structure and its inputs"; and this is not problematic, except that it should be added that the dorsal system's structure changes with experience (see sect. R3.1). However, the other two assumptions that Shebilske notes would appear to be more troublesome for the dual-process approach. The second claims that there is no direct input from one process to the other, and the third that each system has no influence on the other's structure. A viable possibility is that repetitive perception by the dorsal system of some similar object or event might influence ventral perception. Thus, for example, taking one of the examples of evident dissociation between the two systems in interactions with visual illusions (see sect. R3.7), one might ask whether a great deal of practice with the dorsal perception and the concomitant motor response would not affect the ventral perceptual judgments of the visual characteristics of the illusory figure? Another question is whether the symmetrical relation exists at all where much experience with ventral perception influences dorsal processing. As the dorsal processing is mainly implicit and hardwired, one would guess that this is not the case, but I know of no research on this topic.

McFarland's title asks "Where does perception end and when does action start?" – clearly a rhetorical question. However, he goes on to state in his Abstract that the characteristics of the two systems indicate that they are not entirely of a perceptual nature. Here, as in the responses to other commentaries, it must be noted that the exact demarcation of what are, and what are not, perceptual processes depends on one's exact definitions and theoretical proclivities. McFarland is unhappy with some of my definitions, saying of my definition of perception that "it follows from this broad definition that all psychological processes are perceptual processes. . . . A definition of perception should be restricted to those aspects of sensory stimulation that are modality-specific." I, of course, disagree with both those claims. The ecologically oriented might accept the first part, but would surely balk at the second. Much of McFarland's commentary deals with the question of experimental methods to differentiate between perceptual and motor processes. This is an interesting question in its own right, but somewhat tangential to the thesis of the target article, where quite a few examples favoring the dual-process approach did not include a motor component at all. Further, McFarland writes, "The dorsal and ventral system are defined anatomically." This is incorrect! As I wrote in Note 3 of the target article, I used the anatomical names for want of a better alternative, but only "as general labels and not to specify anatomical loci."

As its title indicates the **Phillips et al**. commentary touches upon a rather broad range of topics relating to the two perceptual systems as envisaged in the target article. None of these are dealt with in any depth and hence it is difficult to respond to the points they raise. Among the topics raised is the relation of the concept of attention to the two perceptual systems. Phillips et al. note that the ventral system is the system that possesses "conscious awareness," and that this term implies attention. This leads them to correctly point out that attention can be under dorsal control, such as in the case of a flashing light. The problem here lies with the multifarious usages of the term "attention" and not with the two systems notion. However, these commentators suggest that this and other examples imply that the dorsal system is in a way "cognitive." I have no trouble with this suggestion as I feel that the demarcation between perception and cognition is also quite fuzzy, although I would add that in the very few instances that I appended the term "cognitive" to the ventral system, I was using it in relative terms, to imply that it is more cognitive than the dorsal system. Another point these commentators raise is regarding the separability of the two systems, where they rightly point out that they cannot normally function in isolation. They suggest that "it is likely that one system would be modulated within the context of the other." This makes sense, but I would add that future research will spell out the exact modes of interactions between the two systems, and will detail just how symmetrical these interactions are. At the end of their commentary, Phillips et al. note that a theory tying the two systems is lacking; I agree, and once again say that this is also a task for future research.

Andersen does not see the dual-process approach as a means of reconciling the two rival theoretical approaches to perception. His view is based both on empirical findings on motion perception and on a different conceptualization of the differences between the two theoretical approaches. Noting that the brain areas involved in motion perception (MT and MST) are part of the dorsal system, he cites a study of his (Andersen 1989) where subjects viewed moving dots creating optic flow, which is processed in the MST. In spite of the dorsal nature of the processing the subjects were able to perform a clearly ventral task, judging the number of planes defined by the moving dots. He also mentions that Orban et al. (1995) found cells in the MST that responded to deformation, which can only serve in the perception of shape. These findings led him to comment, "Thus, one does not find a clear disassociation between the dorsal and ventral pathways in specifying motion information for action or judgment." Both these examples are indicative of a misunderstanding of the ideas in the target article. First, no claim was made that visual information processed in the dorsal system is not available to the ventral system when judgments are required. We would be in quite a predicament if we were not able to consciously access motion information for making judgments. Presumably, the optic flow information might be used more efficiently when processed by the dorsal system for carrying out some action (but see **Cutting**'s commentary, regarding optic flow) however it also must be available to the ventral system. What is more, as was pointed out in section 3.1, there is evidence for motion sensitivity in V4, which is part of the ventral system. As for Orban et al.'s (1995) findings, these are at least superficially similar to those of Sakata et al. (1997) briefly mentioned in the target article (sect. 3.1), indicating that there are cells that are sensitive to shape and size in the parietal cortex of monkeys (dorsal system). Clearly the dorsal system must also have access to shape and size information to allow the perceiver to interact with the perceived objects. Shape information does not only serve in judgmental responses but also in dorsal system motor responses.

Andersen goes on to say that Rock and Helmholtz are not the best examples of the constructivist approach, offering Marr's (1982) computational approach to vision as a better example. My feeling is that Marr's approach is not really as much in opposition to the ecological viewpoint as is that of Rock. In fact, it has features that are not at odds with the ecological approach at all, except that Marr includes constraints that are based on environmental regularities in his analysis. This is reminiscent of the friendly debate between Johansson (1970) and Gibson (1970) that both **Andersen** and **Braunstein** mention. Johansson claimed that an *a priori* assumption of 3-D rigidity is necessary for accurate space perception, and Gibson responded that that rigidity can be perceived through the motion of the observer or of the object. Gibson wrote,

> The rigidity and non-rigidity of things can thus be detected. . . . But that does not mean that the brain has to know *a priori* that space is rigid. I don't think that there are any built-in assumptions in the brain, but it is a great distinguisher of differences. (Gibson 1970, p. 77)

All in all, the differences in the approaches of Johansson and Gibson are not very great, and much less than those between Rock and Gibson. Johansson is saying that there is need for an assumption of 3-D rigidity and Gibson is responding that the information for rigidity can be picked up by the perceiver. In all other respects the two agree on the central contribution of stimulus information and the minimal contribution of mental processes. Johansson also writes:

> The efficiency of the system is given by a set of rules for stimulus data treatment (the programming of the visual computer, if you accept the metaphor), rules which work in an automatic way, but which result in a veridicality when the proximal stimuli are projections from moving rigid objects and/or a rigid environment in motion relative to the eye. . . . Such rules have been shown experimentally to work in a blind, mechanical way and leave basically nothing for subjective choice. (Johansson 1970, p. 73)

This describes a very dorsal-like system.

R3. Broader and more general topics

Several topics were raised that were common to more than one commentary and this section will contain responses to them. The presentation order of the topics is arbitrary.

R3.1. Memory and learning in the two perceptual systems

Several commentaries raised questions relating to memory and learning even though the target article did not deal in any depth with these topics, except to point out that the dorsal system does not seem to have much of a visual memory for the performance of short-term on-line tasks. This claim is based on research findings indicating that when a delay is introduced between the intake of visual information and the execution of a response, that response is no longer under the control of the dorsal system but under the control of the ventral system. In spite of the apparent lack of a short-term visual memory in the dorsal system, the fact remains that when a dorsal system perceptual task is repeated, perceptual performance improves with such practice and without the necessity of feedback (e.g., Gibson & Gibson 1955), that is, perceptual learning takes place. As **Toates** notes, this learning is based on implicit memory, and I would add that the implicit memory in this case differs from the implicit memory found in certain studies on amnesics who manifest learning on ventral type tasks such as word spelling (e.g., Jacoby & Witherspoon 1982). Although many equate implicit memory with procedural memory, in this case it might be said that the implicit memory is declarative. In contrast, the perceptual learning by the dorsal system is implicit memory of a procedural nature. I touched upon the procedural-declarative distinction in my Note 5, but there I referred to the declarative memory as "representational." It is now clear to me that this term is a red flag for many of the ecological bent, and as I do not want to make any claims about the exact nature of the memory involved, I have reverted to "declarative."

In general terms, I would suggest that at least two types of perceptual learning are possible, depending on which perceptual system is involved. In learning to identify people or car makes, for example, it is the ventral system that is involved, but when learning to catch a ball on the fly, for example, it is the dorsal system that is involved. Thus, after seeing many VW Beetles from many viewpoints, one becomes adept at quickly recognizing it as a car make that was encountered before and at identifying it as a Beetle. This is perceptual learning by the ventral system. This contrasts with, say, learning to correctly perceive a rapidly nearing baseball or cricket ball, allowing it to be batted efficiently. This is perceptual learning by the dorsal system. I would further suggest that these two types of perceptual learning are subserved by different kinds of memory systems. The dorsal learning system utilizes a procedural memory system and the ventral learning system a declarative memory system. Dorsal perceptual learning is a process of honing the perceptual mechanism, allowing one to get better and better at differentiating ever finer differences in the perceptual array (Gibson & Gibson 1955). Ventral perceptual learning is a process of learning to associate a feeling of familiarity and labels to perceptual information (Postman 1955). The former does not require feedback for learning to transpire, while the latter often does, especially if the learning of labels (identification) is involved. As was mentioned above, dorsal learning is usually implicit, while ventral learning can be both explicit and implicit. In learning certain complex motor tasks, such as driving a car, it is suggested that initially ventral perceptual learning is also involved but later the task falls under control of the dorsal system (see below).

Two commentaries by adherents of the ecological approach touch upon learning. **Cooper & Michaels** write, "In ecological psychology, learning is not conceived as storage, but as the education of attention to variables that specify a to-be-perceived environmental property." My interpretation of "education of attention" is that they are referring to something quite similar to the honing of the perceptual system I mentioned above, in other words, to dorsal perceptual learning. The dorsal perceptual system learns to differentiate increasingly fine aspects of the to-be-perceived environment. I would contend that such a mechanism would not allow the learning of perceptual identification responses. Cooper & Michaels might say that ventral system learning does not entail perception but some post-perceptual process. Indeed, the labels that we attach to different processes are rather arbitrary; following this line of thought one would have to say that reading is also not a perceptual process, as the letter- and word-shapes must be identified to allow the reading process to transpire.

Burke & Hayward also take a very strong ecological stand in their commentary and devote much of it to the question of learning. They suggest that mine is a misrepresentation of the direct (or ecological) approach in that I claim the information pickup is largely an unlearned process. In reviewing the target article I find that I did not or did not intend to make such a claim, but I did quote Gibson (in sect. 2.2) who wrote "the basic affordances of the environment are perceivable and usually perceivable directly, without an excessive amount of learning." (1979/1986, p. 143). The possible source of misunderstanding is my brief foray into matters developmental, where I suggested that dorsal invariants are innate and ventral cues are learned. This was, of course, an overstatement and it needs correction. What I should have written is that those perceptual abilities that are innate are dorsal, but undoubtedly these innate abilities are honed by the dorsal perceptual learning processes outlined above. Burke & Hayward are certainly right in noting that the perception of the affordance of gap as "jump-over-able" changes as the perceiver grows bigger. They go on to state that the direct theorists consider all memories to be procedural rather than declarative, citing an article by Crowder (1993). In that article Crowder argues against the idea that memory consists of "'stores' – receptacles into which information is placed at learning and from which it is later retrieved after a delay." He prefers a procedural concept where "memory storage for an experience resides in the same neural units that processed that experience when it happened in the first place." First, it should be noted that Crowder, unlike **Cooper & Michaels**, does conceive of memory as "storage," but the storage is at the site of the processing rather than in separate stores. Here, once again, I would suggest that Crowder is describing the dorsal system's memory, while the ventral system's memory might well be housed separately to allow retrieval of familiarity and identification tags and labels. Burke & Hayward suggest that learning involves a "tuning in to," and I see this as paralleling the honing process of dorsal learning. They go on to say that the brain changes associated with this learning result in new sensitivities "but these changes don't 'represent' the external situations." Once again, one can live without using the term "representation" and its derivatives, but I would suggest that ventral perceptual learning must use a different type of storage than the dorsal learning that I believe the commentators are referring to.

Several other commentaries also deal with learning and memory. **Binsted & Carlton** look at the transition from control of motor processes by the ventral system to that of the dorsal system. While they find the dual-process approach "superficially consistent" with accounts of skill acquisition, they raise the question of "how the dorsal system is able to benefit from learning apparently accomplished within the ventral stream." The example they choose, that of learning to ride a bicycle, probably entails relatively little ventral involvement, with the bulk of the learning simply requiring the honing of the pickup of the relevant dorsal information needed for riding the bike. Much of the perceptual information needed for that task is not visual but rather vestibular and proprioceptive. A better example, to my mind, would be learning to drive a manual shift car. The perceptual input requiring the shifting of gears is most probably processed by the ventral system at first, and only later is this control shifted to the dorsal system. Ongoing research is looking at how perceptual-motor skill learning switches from ventral control to dorsal control. For example, Willingham (1998) has posited that both types of learning, implicit (dorsal) and explicit (ventral), transpire simultaneously. In a paper that provides empirical support for this idea, Willingham and Goedert-Eschman (1999) write that "the explicit process supports behavior until the simultaneously acquired implicit representations is sufficiently well developed to support behavior, at which time the explicit process is simply not used any longer. . . ." In other words, both perceptual systems are learning simultaneously, the dorsal implicitly and the ventral explicitly, and when the dorsal system has learned to become proficient it takes over the control.

Binsted & Carlton also raise the question of how the two systems learn to work together when the dorsal system has no memory. This, I fear, is a misunderstanding stemming from my noting that the dorsal system is lacking more than a very short-term memory for the performance of online actions. It does have long-term implicit *procedural* memory and presumably it is that memory system that takes over from the ventral system's memory when it has attained sufficient proficiency. These commentators also raise the interesting but difficult question of how a switch from dorsal to ventral system control occurs when conditions hamper dorsal system functioning. This leads them to suggest that it follows that some (unconscious) intelligent agent (homunculus) is needed to evaluate the efficiency of each system. Continuing along these lines, they contend that the dual processes suggested in the target article are counterproductive and that it would seem that a single coherent explanation is needed.

As far as I am aware there are no clear answers currently available to **Binsted & Carlton**'s question. However, it might be helpful to look at the study by Marotta et al. (1997) (see target article, sect. 5.2.1) where two visual agnosic patients and control subjects were required to reach and grasp blocks of differing width either with monocular or binocular vision. The patients were no different from the control subjects with binocular vision but showed a lack of size constancy with monocular vision, unlike the control subjects who were able to carry out the task under both conditions, albeit a bit less efficiently with monocular vision. These results were seen as indicating that the dorsal system could only utilize the binocular cues, and that the monocular cues were processed by the ventral system. The relevant question here is how did the ventral cues take over the control of the hand movements in the control subjects under monocular viewing. The answer, I suggest – at our current stage of knowledge – is that the visual system switches automatically to ventral control under monocular vision. In other words, the type of input reaching the visual system determines which of the two systems, dorsal or ventral, will carry out the task.

The addition of a hypothetical homunculus seems unnecessary; we have a system with built in redundancy that can switch between available mechanisms as the conditions require. Here both systems function unconsciously, unlike the conscious functioning of the ventral system in the perceptual learning discussed above. Of course, the switch between systems can also depend on the task, judgmental tasks requiring ventral system intervention.

The commentary by **Elliott et al**. deals mainly with the effects of illusions on target-memory and learning (see sect. R3.7). They note that both eye- and hand-movements are susceptible to "range" or "context" effects, where movements that occur later in a block of trials are influenced by, say, the distances (far or near) of the targets earlier in the block. This leads them to state, "At some level, this averaging requires a memory for past events that lasts at least several minutes." Once again, I have the feeling that there is confusion here between the lack of much of a short-term memory to carry out a specific action on a specific trial, and the existence of an implicit procedural memory that appears to update itself over a relatively short set of experimental trials. I have no trouble with their next to last sentence: "Certainly, memory appears to play a role in even very rapid, unconscious perceptual-motor behaviours." I would just add that the latter is not the same memory that controls the online aiming movements, but one that implicitly accumulates experience and hones the response.

Postma et al. are also disturbed by what they interpret to be my claim that the dorsal system has no memory, and the same response as that given above is valid here as well; the dorsal system does have an implicit procedural memory. They state, "most affordances are assumed not to be present at birth but to develop during a life time." There is some confusion here as "affordances" are not "in the perceiver" but are mutual relations between the organism and its environment. As was seen in the quote above, Gibson did not think that the "basic affordances" needed to be learned, but clearly these change with experience and with the changes in the organism's size as it grows. Postma et al. cite a recent study that further strengthens the idea of two separate processing systems with separate memories. Holdstock et al. (2000) showed that a patient with bilateral hippocampal damage exhibited poor performance in an allocentric spatial memory task, but was within the normal range on an egocentric spatial memory task. The hippocampus is part of one (or more) memory system that supplies the ventral perceptual system with stored information, and damage to it should affect allocentric tasks more than egocentric tasks. The latter, egocentric, task probably utilizes a different dorsal short-term memory system possibly in the parietal cortex (see e.g., Quintana & Fuster 1999). Postma et al. see the fact that this patient could perform the egocentric task as indicating that the dorsal system has a memory for performing spatial tasks, but they fail to mention that the longest delay used in the study between stimulus exposure and test was only 60 seconds, and this is still within the range of a very short-term memory.

R3.2. Development of the two perceptual systems

Two commentaries picked up on my brief foray into matters developmental, while a third commentator, **Toates**, included the topic of development as part of his brief review of dual-process accounts of behavior in general. At the end of section 5.2.1, I speculated that the dorsal system invariants were probably innate and that the ventral system cues were probably learned (but see sect. R3.3 on the usage of "invariants" and "cues"). Infant research has demonstrated that newborns and very young infants exhibit quite striking perceptual capacities, such as size constancy (Slater et al. 1990), shape constancy (Slater & Morrison 1990), and the discrimination of face-like stimuli (Goren et al. 1975; Johnson et al. 1991) – and these indicate that the infant is innately equipped with certain perceptual capacities. My suggestion is that these capacities are in the dorsal system.

Mareschal & Kaufman present evidence that they claim indicates that the dorsal system develops more slowly than the ventral system, while stessing that "any theoretical account of perception and cognition must take developmental constraints seriously." Developmental constraints most surely should be taken into account, but their suggestion that my proposal "rests on the assumption that the dorsal route functions are innate or largely mature very early in infancy" is exaggerated, to say the least. They base their claim concerning the relatively late development of the dorsal system on event-related potential (ERP) and behavioral studies of infants. They note that ERPs to face images occur at 6 months, while an ERP study by Csibra et al. (1998) indicated that "the dorsal pathway is still not influencing eye movement control at that age." ERPs measure cortical activity and not *subcortical* activity; and, as **Toates** notes, Bronson (1974) suggested a long time ago that during the first few months an infant's vision is mainly under subcortical control (see Johnson 1997, pp. 76–82 for a review of the evidence for subcortical visual processing in the first months of life). As was pointed out in section 3.1 of the target article, the dorsal system receives input from both cortical and subcortical parts of the brain, and it would seem that during the very first months of life it is the subcortical input that is dominant. Thus, while these commentators might be right about the inefficiency of the occipito-parietal dorsal pathway very early in life, it is quite possible that it is the collicular-parietal pathway that is functioning at that time.

The behavioral evidence cited by **Mareschal & Kaufman** to bolster their claim that dorsal system development lags behind that of the ventral system, are studies that show very early facial discrimination abilities as compared to a study of the spatial representations that guide eye movements. They suggest that facial discrimination abilities are "a canonically ventral function." Face recognition and identification are indeed ventral functions, but the studies of the attraction of schematic faces for newborn infants (Goren et al. 1975; Johnson et al. 1991) are not of recognition, and the possibility exists that the early CONSPEC mechanism suggested by Morton and Johnson (1991) is part of the subcortical dorsal system. In fact, it would seem that the newborn is not capable of recognizing faces and this ability only develops during the second month (e.g., Bartrip et al. 2001). To contrast the face discrimination studies Mareschal & Kaufman cite the findings of Gilmore and Johnson (1997) that examined saccadic eye movements to a two-step motion, enabling the authors to differentiate between retinocentric and body-centered responses.

Gilmore and Johnson (1997) found that 3-month-old infants evidenced retinocentric responses, while 7-month-old infants showed the more appropriate body-centered responses. However, the retinocentric responses are not ventral allocentric responses; they are simply a more primitive form of dorsal response than the body-centered responses. Presumably the 3-month-olds were in a period of transition from subcortical to cortical control of the necessary eye movements. What is more, as **Hochberg** notes, saccadic eye movements serve to bring the item of interest to foveal examination. As the fovea develops relatively slowly (see e.g., Hendrickson 1993), it is possible that the 3-month-olds have a lesser need to study their visual environment with their foveas and this might explain the retinocentric responses. Thus, both the behavioral and ERP evidence cited by **Mareschal & Kaufman** does not, to my mind, negate the possibility that dorsal system functions developmentally precede ventral ones, and that some of them are innate. Clearly, these early dorsal functions are improved and better differentiated with the initiation of cortical control.

Van der Kamp & Savelsbergh accept the idea that the two visual systems follow different developmental trajectories, but are disturbed by what they see as my suggestion that the development of the two systems follows different principles. They appear to understand my claim to be that *all* dorsal perception is innate while ventral perception is learned. My intention was to point to the fact that the dorsal system allows the newborn to utilize basic perceptual mechanisms, but surely those mechanisms are highly refined through perceptual learning. I would, however, contend that the perceptual learning that transpires in the dorsal system does differ from that of the ventral system (see sect. R3.1). These commentators cite a study by Kayed and Van der Meer (2000) that showed that 5-month-olds time their blinks to visual looming stimuli appropriately if the velocity is constant, but not if it is accelerative, while 7-month-olds respond appropriately to both types of motion. As in the eye-movement study described in the above paragraph, it could be suggested that the innate responses to looming objects are not very well developed at first and develop further through perceptual learning.

The Kayed and Van der Meer study appeared in special issue of *Infant Behavior and Development* devoted in its entirety to action and perception in infancy. Van der Kamp and Savelsbergh (2000) wrote the introductory article to that issue in which, among other things, they review studies that focused on similar topics using research paradigms amenable to either dorsal processing or ventral processing. Thus, for example, they cite a study by Jouen et al. (2000) that demonstrated that 3-day-old infants adjust their backward head movements to optic flow velocity, while studies using habituation or preference methods (Dannemiller & Freedland 1989; 1991; Wattam-Bell 1990) found no evidence that infants younger than 2 months of age perceive velocity. In other words, when velocity perception is examined with a dorsal system motor response we find indications of movement perception in 3-day-olds, but when they are assessed with ventral system type responses there is no indication of velocity perception at 2 months. Similarly, Van der Kamp and Savelsbergh (2000) note that newborns direct their arm movements to a moving toy (Von Hofsten 1982) but 1-month-olds are not able to distinguish different directions of motion (Wattam-Bell 1996). These and other examples all appear to indicate that the dorsal system responses either develop earlier, or are innate and precede ventral system responses to the same or very similar stimuli.

R3.3. Invariants and cues

In two separate commentaries, **Cutting** and **Kingdom** expressed dissatisfaction with my usage of the terms "invariants" and "cues." In section 5.2.1 I used the terms "dorsal invariants" and "ventral cues" as labels to distinguish between the types of visual information used by the two systems. I used the term "invariants" for dorsal system information simply because that was the term that Gibson used, and the term "cues" as it is commonly used by the constructivist. Cutting rightly takes me to task for my inaccurate usage. In his book, he writes: "I suggest that a perceptual invariant must be mathematically specifiable in one of two forms—as a real number or as an ordered relation among reals" (Cutting 1986, p. 75), and adds in a footnote:

> My definition of an invariant differs from that typically given in the ecological approach (Gibson 1979, Michaels and Carello 1981) in my ardent demand for a particular kind of mathematical specificity. Generally, I claim that if the invariant exists, it can be measured, with a numerical value or relation among numerical values placed on it. (p. 267)

Cutting cites some recent studies from his laboratory that attempt to delineate the visual information used for the perception of heading. For example, Cutting and Wang (2000) examined the effectiveness of two invariants, a convergent pair of stationary objects and a decelerating divergent pair, and of one cue (labeled "heuristic" in their article), an accelerating divergent pair. They found that "observers appear to be able to use both the invariants and the heuristic in making heading judgments." I would contend that all three sources of information are first analyzed by the dorsal system, and therefore the label I used, "dorsal invariants," is inappropriate and it would have been better simply to talk of dorsal and ventral visual information. Cutting also asks: "Why hardwire either from birth?" True, the immobile human infant does not need visual heading information at birth, as do foals or mountain goat kids; but I would suggest that the infant does need some sort of visual motion processing information at birth, on which to build the finer mechanisms needed later in life. I agree with Cutting's last point that some (or many) of the invariants are learned, but would contend that this learning transpires by selective honing of the very basic dorsal mechanisms the infant is born with. (See discussion in sect. R3.1.)

Cutting is, to my mind, overly critical of Gibson's invariants in his commentary. Following Gibson (1979/1986, p. 160) I wrote of two invariants, texture interception and the horizon ratio. Cutting reminds us that the texture invariant is "only for flat-lying objects occluding flat-lying texture on the ground." This is, of course, correct (see a clear explication in Gillam 1981). But Gibson wrote, "it intercepted or occluded the same number of texture elements of the ground," and while "occlusion" might be ambiguous, interception clearly refers to the number of elements at the base of the object on the ground. The caption of Gibson's well-known illustration of this invariant (p. 163) where two cylinders appear on a textured surface is "The base of each pillar covers the same amount of texture of the ground." Cutting also cites Sedgwick's (1986) constraints on the horizon ratio invariant. Without detailing these, I would point out that this invariant and its somewhat broader counterpart, the eye-level plane, have been shown to be a rather effective source of visual information in studies of perception of size (e.g., Bertamini et al. 1998; Wraga 1999a; 1999b). Finally, Cutting notes that "yet another Gibson invariant – the focus of expansion – has little currency, except at high speed." Recalling that Gibson's analyses were carried out only at a conceptual level without the aid of present day computational facilities, it should be noted that his were groundbreaking contributions to our overall understanding of what information the ambient environment affords us.

Kingdom also voices concerns about the invariant-cue dichotomy, although this is not the main focus of his commentary. He believes that the dual-process approach is viable only if ecological perception is "concerned exclusively with visual signals that control motor activity" and points out that for Gibson the invariants were not necessarily related to motor activity, as in the case where they served size perception. I agree with Kingdom in that there are instances where invariants are picked up and no overt motor behavior follows, but it is important to remember that for Gibson all perception involves a continuous interplay between perception and action. Kingdom completely misinterprets my thesis when he suggests that in size perception I am "forced to relocate such object-centered invariants to the ventral constructivist pathway and change their name from invariants to cues." I would contend that size information is in the main picked up by the dorsal system, allowing us to interact with our environment. The ventral system also has access to size information, but it is usually relative size and in object-centered terms, and is less useful for interacting with the environment. In experiments on size perception participants are often asked to make judgments, and – depending on the exact instructions and paradigm used – they utilize the two types of information to different extents. When walking through a narrow doorway or driving between two parked cars the size of the gaps is picked up by the dorsal system and normally the ventral system is not involved.

Thus, I agree with **Kingdom** that there are many examples "where both invariants and cues are exploited for a common purpose," but do not follow how this leads to his claim that they "are therefore likely detected in the same pathway." The example he cites in this context is lightness constancy, where he briefly notes that there are multiple mechanisms that enable its achievement. He notes that both invariants, such as the ratio of the object's luminance to its surround, and cues, such as various types of junction between edges, play a role in achieving lightness constancy. Kingdom (in press) elaborates on these points in a recent review chapter, where he writes: "I have now argued that two mechanisms contribute to brightness/lightness perception, a low-level contrast-sensitive, and a mid-level illumination-interpretive mechanism." Disappointingly, he does not seem to see the parallels between his analysis and the two perceptual systems described in the target article. Clearly, the neural pathways involved in lightness perception are very different from those in size perception, but there are many conceptual similarities.

R3.4. Biological motion

Two commentaries, by **Green & Pollick** and by **Schull & Bingham**, respectively, deal with the implications of the perception of biological motion (BM, labeled "patch-light displays" by the latter commentators) for the dual-process approach. The essence of both these commentaries is that the ability to rapidly perceive, say, a moving person from an array of moving light patches implies that the two systems function in very close interaction and any attempt to separate them is either useless (**Schull & Bingham**) or an impediment (**Green & Pollick**). The BM displays originally devised by Johansson (1973) are indeed very impressive examples of the ability of the visual system to process moving light-patch stimuli devoid of other content and quite dramatically perceive a whole being (human or animal) in motion. Our ability to recognize friends, for example, from BM displays points to a very special kind of interaction between the two perceptual systems. On the one hand it is claimed that motion information is mainly processed by the dorsal system, while, on the other, it is claimed that it is the ventral system that serves in the recognition of previously encountered items. It is because of this that the two commentaries suggest that BM is evidence of the inadequacy of the dual-process approach, implying that this rapid process cannot be explained within its framework.

BM is indeed a singular perceptual phenomenon where a few moving dots yield a very compelling percept of an animate being in motion. Because of this, many studies have examined various facets of its functioning. BM is perceived by adults as well as 3-month-old infants (Fox & McDaniel 1982) and even by cats (Blake 1993), and has also been shown to convey emotional information (e.g., Brownlow et al. 1997; Dittrich et al. 1996; Pollick et al. 2001). Psychophysical research (Neri et al. 1998) has shown that BM differs from simple motion in several ways, such as summing over temporal intervals eight times longer than simple motion. Several studies have utilized brain-imaging techniques to examine BM (e.g., Grezes et al. 2001; Grossman et al. 2000; Grossman & Blake 2001; Vaina et al. 2001). These studies show that several brain areas known to be involved in motion processing are activated by BM, but what is more interesting is that they all point to the involvement of the superior temporal sulcus (STS). Vaina et al. (2001) write in the summary of their findings,

> Thus, we conjecture that, whereas face (and form) stimuli activate primarily the ventral system and motion stimuli primarily the dorsal system, recognition of biological motion stimuli may activate both systems as well as their confluence in STS.

In contrast to the reservations voiced in the two commentaries as to the usefulness of the dual-process approach in the light of the BM phenomenon, I would contend that it is of pertinence here as well, albeit in a somewhat singular manner. One aspect of this singularity is the fact that the locus of confluence is in the STS, as STS is also implicated in social perception from visual cues (see sect. R3.9).

In their commentary **Shull & Bingham** draw a parallel between BM perception and event perception. According to them, (event) perception "normally occurs in the course of self-motion or motion of objects in the environment." The example they present is of someone recognizing her or his suitcase as it moves on a conveyer belt. They claim that this is similar to BM (or "patch-light recognition") "in that perception can be based on motion only." However, I would contend that the two are very different. Should the conveyer belt break down and stop running one would have no trouble recognizing the suitcase, but when a BM display is not in motion, nothing but a group of light patches are perceived, nothing is recognized! In other words, in the conveyer belt example the suitcase is recognized in spite of the movement, while in BM the movement itself conveys the visual information necessary for recognition. These commentators also note that "motion is processed primarily by the dorsal system" and suggest that "information must be transmitted from the dorsal system to the ventral system to allow for recognition and identification." They then go on to say: "How can the dorsal system transmit information to the ventral system if they use different information, that is, if the ventral system uses constructivist cues and the dorsal system uses ecological information?"

Unhappily these quotes indicate that my exposition was not clear enough. First, I would contend that both systems use the *same* information albeit for somewhat different purposes. That information is analyzed differently by the two systems, the dorsal system utilizing built-in mechanisms, the ventral systems utilizing cues or heuristics. The information in events includes motion information, and as **Shull & Bingham** note, motion information is *primarily* processed by the dorsal system. However, there are imaging research findings (Grill-Spector et al. 1998) that indicate that the lateral occipital area, part of the ventral stream, which is known to respond to object information, is capable of extracting object information from motion stimuli. What is more, Kourtzi and Nakayama (2002) recently distinguished between two object processing mechanisms in a psychophysical study; one, a view-dependent processor of static objects capable of bridging long temporal delays; the other, a view-independent processor of moving objects with little temporal storage. They suggest that these two mechanisms parallel the two visual systems, the ventral and the dorsal, respectively.

R3.5. Size and distance perception

Only two commentaries, those of **Cooper & Michaels** and of **Ross,** dealt with size and distance perception, the topics that first raised my interest in seeking a resolution of the very different theoretical explanations for these two types of perception. Cooper & Michaels briefly dealt with this topic in an attempt to show that percept-percept coupling does not occur for size and distance. They quote a short report of a study (Cooper 1999) of size-distance invariance (SDI) where twelve observers reported the sizes and distances of three spheres placed at three distances and viewed both binocularly and monocularly. Using Ashby and Townsend's (1986) proscriptions for determining perceptual independence, it was found that "perceptual and decisional separability held for each level of size and distance regardless of viewing condition." In other words, SDI was not found, and Cooper & Michaels see SDI as "due to post-perceptual decisional process, and not to an (unconscious) inferential process as Norman and the constructivists would argue." Unfortunately, the choice of size-distance invariance as an example of percept-percept coupling was not a good choice, as it has been known for quite some time that it does not hold. Both quite old and new studies have shown SDI not to be a valid description of the relations between the perception of size and of distance. As was mentioned in section 5.2.3 of the target article, a review of SDI written over forty years ago, Epstein et al. (1961) concluded, "the size-distance relationship expressed in the several formulations of the invariance hypothesis should not be assigned a unique or primary status in explanations of size perception." Even earlier studies noted the invalidity of the SDI and labeled this finding the size-distance paradox (e.g., Gruber 1954). Ross (in press) has recently reviewed the size-distance paradox, and newly reported research has further shown that the SDI hypothesis holds very little water. Haber and Levin (2001) in a study on "The independence of size perception and distance perception" conclude, "The size-distance invariance hypothesis was shown to be inadequate for both areas of research." It is probably for this reason that Rock did not include any studies of size and distance in his book *Indirect Perception* (Rock 1997). There are, however, many studies pointing to percept-percept couplings in Rock's book, which I suspect are more difficult to refute than the SDI hypothesis.

Ross' commentary is devoted, entirely, to size and distance perception. Ross begins by correcting my statement that Berkeley was a forerunner of Helmholtz (target article, sect. 4.1), noting that Berkeley, in contrast to Helmholtz, did not claim that distance information was taken into account in the perception of size. I am grateful for this correction. Otherwise, I find this commentary very disappointing in its negative response to the suggestions in the target article, especially in view of the fact that there are very many points in Ross' recent chapter on the size-distance paradox (Ross, in press) that can be at least partially resolved by adopting the dual-process approach.

In the abstract to her commentary **Ross** writes: "The dorsal/ventral solution is dubious for close displays and untestable for far displays." It is unclear what Ross means by "far displays," but as her commentary deals a great deal with the moon illusion, I would presume that these are her "far displays." It should be pointed out that perceiving the size of the moon is a very special kind of phenomenon, and except for the astronauts on the Apollo it is doubtful if anyone actually ever perceived the true size of the moon. There is surely a connection between the differences in the apparent size of the moon near the horizon and at the zenith, known as the moon illusion, and our everyday perception of size and distance; but I would agree with Ross that it is difficult to test the validity of the dual-process approach for explaining the moon illusion because the size of the moon is never perceived veridically.

As for close displays, **Ross**' claim that the dual-process approach is dubious is based on two studies (Wood et al. 1968; Zinkus & Mountjoy 1969), which she cites as evidence for the fact that "the size-distance paradox may sometimes occur when distance is adjusted manually." These studies are two, very brief, one-page reports of three experiments carried out by the same group of researchers. In two of these experiments the experimenter manipulated the conditions, but in one, the second experiment described in Wood et al. (1968), the thirty three participants were instructed to pull on a rope and adjust the distance of an overhead illuminated disc to that of a disc of similar size (actual size not given) 59 inches directly ahead. The experiment was carried out in a totally darkened room. The report notes that "all 33 Ss indicated a distance discrepancy by placing the overhead target closer to themselves," the mean distance being 42.70 inches. No other data or statistics are given. This experiment is an attempt to simulate moon-illusion findings where the moon is judged to be bigger at the horizon but farther away at the zenith, that is, the size-distance paradox, but the results are just the opposite of the paradox! It is not really clear why Ross sees these results as negating the dual-process approach. The experiment is carried out in the dark with sparse visual information and the rope-pulling response is indeed manual, but surely is not a response that one would normally make when making a motor response to a perceived distance. What is more, Ross (in press) writes of this experiment, "It could be argued that these motor adjustments are cognitive because they rely on memory." My feeling is that the report of the experiment is too condensed to really determine if memory does or does not play a role, but it cannot serve as evidence for the negation of the dual-process approach.

Ross' chapter (Ross, in press) contains many statements and examples of research findings that are in fact consistent with the dual-process approach. In reviewing the literature she often notes that it is possible that the perception of size and distance can each occur at different levels of consciousness. I would suggest that those instances that she labels "preconscious" or "unconscious" occur in the dorsal system, while those occurring at "conscious" or "higher" levels occur in the ventral system. In her section on "Optical distortion," Ross describes an experiment by Mon-Williams and Tresilian (1999) where observers viewed a target with a prism placed in front of one eye altering the vergence angle. Increased convergence brought about verbal reports of a farther and smaller target, while increased divergence yielded the opposite results, in both cases a manifestation of the size-distance paradox. In contrast, when observers were asked to reach for the targets (without seeing their hand) the convergence yielded nearer reaches, and the divergence farther reaches. These researchers interpreted their findings as indicating that the size-distance paradox is a cognitive phenomenon. Ross' response to this interpretation is, "A difficulty with this analysis is to define which methods define 'cognitive' judgements and which are more direct" (Ross, in press). I would suggest that interpreting the verbal responses as mainly involving the ventral system and the reaching responses as mainly involving the dorsal system helps in clarifying these results.

R3.6. Consciousness

Several commentaries deal with the question of consciousness, in the main with my claim that dorsal system processing is usually carried out unconsciously. **Neisser** raises the possibility that the dorsal system might entail two subsystems. One is a "system for controlling small-scale actions and another, yet to be discovered, for using similar information to locate the self in the larger environment." The latter deals with specifying the layout of the large-scale environment and the perceiver's position in that environment. Neisser (1988; 1995) has written about this in the framework of what he calls the "ecological self," that which situates the self within a given physical environment. Neisser believes that one is conscious of the ecological self (but see Hardcastle 1995) and he also suggests that this second aspect of dorsal system is conscious. Knowing very little about the topic of the self, I find it hard to comment on this suggestion, but simply note that past experience has proven it worthwhile to heed Neisser's intuitions.

In their strongly ecologically oriented commentary, **Burke & Hayward** complain that I asserted that all information pickup by the dorsal system is unconscious. While I believe that I was careful not to assert that *all* information is picked up unconsciously, I do believe that this is correct for much of it. As a counterexample they suggest the perception of a bird swooping down to land and its identification. I would contend that the perception of the bird's flight (as distinct from the bird in flight, a ventral identification response) is carried out in the main by the dorsal system, while its identification as an egret, say, is carried out by the ventral system. Burke & Hayward contend that both these functions are carried out by the direct (dorsal) system and believe that there is no need or justification for an indirect (ventral) system. In contrast, I would suggest that the initial pickup of information about the bird's exact flight path is carried out by the dorsal system unconsciously, and if the bird's path is directed straight at the perceiver (as in a Hitchcock film!) he or she will move to avert its path and that movement will be without conscious control. This is similar to the very quick pressing of a car brake before the perception by the ventral system that the object moving across the road is nothing more than a plastic bag blowing in the wind. However, one can certainly become aware of the bird's movement through communication between the two systems. (See my comments on recognizing a suitcase on a moving conveyer belt in sect. R3.4.) This point de-serves reiteration, dorsal system perception can and does occur often without accompanying consciousness, but much of that perception can be transferred into consciousness with the aid of the ventral system. As for identifying the bird as an egret, I see no alternative to this function being carried out by the ventral system. Ecologically oriented writers contend that identification can be carried out through the detection of information or through educated attentional mechanisms, but I find these assertions inadequate and would suggest that some storage mechanism must be involved for the perceiver to be able to identify the bird as an egret. That storage need not be in the form of a pictorial representation and might possibly be in the form of a change in the pattern of activations in a connectionist network, which might be more palatable to the ecologically oriented.

These comments are also relevant to points brought up in two other commentaries. **McCarley & DiGirolamo** note that even in the experiment carried out by Gibson on the perception of the size of stakes planted in a large field, the participants had to be conscious of the sizes in order to report on them. I fully agree with this, but contend that size can be picked up by the dorsal system without consciousness, as when one quickly walks through a narrow aperture and rotates his or her shoulders accordingly. Clearly, when required to make a judgment, one has to employ the ventral system and this entails consciousness. In the discussion of size perception (sect. 5.2.4) many studies were described. All of these studies requested size judgments from the observers, but there were differences in the instructions, such as "apparent" versus "objective," that yielded more or less involvement of the dorsal system, respectively. Under "apparent" instructions the observers relied more on reading off the dorsal system size perceptions, while under the "objective" instructions the observers carried out the task more ventrally.

In his commentary **Ingle** points to the fact that the monkeys tested by Ungerleider and Mishkin (1982) for parietal (dorsal) functions, using the "landmark test," had to make judgments of relative distance. He notes that I suggest that relative distance judgments of distance are carried out by the ventral system. But I would quarrel with this interpretation of the landmark task; the monkey only has to pick up the position of the landmark. Patient DF can grasp wooden blocks without being able to consciously report on what she is picking up using only her dorsal system; so the monkey can learn that the position of the landmark designates which cup contains the reinforcement. Ingle continues this line of argument to state, "the investigators I have reviewed stress *conscious* percepts as representative of the spatial mode of vision" (his emphasis). Once again, I would suggest that the percepts only became conscious because the investigators requested judgmental responses from their (human) subjects, and that caused them to be transferred from the dorsal system to the ventral system.

R3.7. Illusions and the dissociation of the two perceptual systems

A great deal of research effort has been expended over the last six years in attempts to try and dissociate the two perceptual systems by showing that the ventral system is susceptible to visual illusions but the dorsal system is not. More than forty papers have appeared on this topic over this short period! The target article reviewed some of the earlier studies (see sect. 3.3) and pointed to the conflicting results in the studies that focused on size illusions. There it was suggested that these mixed results are probably due to the fact that performing some action when viewing an illusion is a rather unnatural task and under these circumstances the ventral system might override the functions of the dorsal system. It was also noted that we really do not have any adequate understanding of the processes underlying these illusory effects, making the interpretation of the results of these studies rather difficult.

One commentator, **Kingdom**, sees the negative evidence reported in some studies of the effects of illusions as more or less invalidating the proposed dual-process approach. He cites three studies (Franz et al. 2000; 2001; Vishton et al. 1999) and, on the bases of their negative findings, he argues against the dual-process approach as a whole, oblivious to the positive results in some other studies and to the studies that indicate that the effect can be found with one motor response and not with another (e.g., Brenner & Smeets 1996; Jackson & Shaw 2000). What is more, in the target article I explained that the study by Vishton et al. (1999) is not really at odds with the dual-process approach. No attempt will be made here to review all the recent studies; luckily, three papers recently appeared within a short period in *Trends in Cognitive Sciences* (Bruno 2001; Carey 2001; Franz 2001) and these give an overview of the state-of-the-art in this field. Hopefully, a careful reading of these papers will show Kingdom and other skeptics that it is too early to bury the notion of action-perception dissociation. To demonstrate that this field of controversy is still alive and kicking, I will briefly describe three recent papers.

Haffenden et al. (2001) have responded to the negative results of Franz et al. (2000) and Pavani et al. (1999) (see sect. 3.3 of the target article). Their study is based on earlier findings (Haffenden & Goodale 2000) indicating that the size of the gaps between the smaller circles and the central circle in the Ebbinghaus illusion can influence motor responses. When the smaller circles are very close to the central circular chip that has to be lifted, the motor response is affected in a manner similar to that of the illusion. These researchers carried out a study where they presented the small circles surrounding the central chip a little farther away from the chip (equivalent to that of the big circles display), and found that when the displays were thus adjusted they were able to replicate the original findings of Aglioti et al. (1995) showing no significant effect of the illusion on motor responses. These findings appear to indicate that the illusory effects of the illusion on motor responses was not on account of its perceptual effect but because of a motor artifact.

Bruno and Bernardis (in press) presented observers with large displays of Kanizsa's compression illusion. In this illusion a horizontal bar appears to be compressed (in length) when occluded by a much larger and vertical rectangle. The extent of this compression has been found to range between 4–6%. Rather than using a one-handed grasp response as in other studies, the observers responded with a two-handed grasp action, like grasping a car steering wheel. In the main experiment (Exp. 2), four response conditions were compared (between subjects, thirty in each group): visual matches, closed loop (visual feedback) manual matching, open loop (no feedback) mimed reaching, and open loop matching. In the open loop conditions the observers wore a blindfold eliminating visual feedback, while in the closed loop condition the observers were allowed vision of either their hands or the target bar (but not both simultaneously). The researchers' main finding indicated that the illusion occurred only in the visual match condition and the closed loop matching; no illusion was found in the two open loop manual conditions. These results, on the one hand, provide evidence for the dissociation of the two systems, but differ from previous research in that one of the conditions immune to the illusion was a matching condition, whereas previous research had found the opposite result and it was claimed that matching is a ventrally controlled task. Thus, many questions still remain to be answered.

Burr et al. (2001) utilized the compression illusion of visual space that occurs around the time of saccadic eye movements (see Ross et al. 1997). Stimuli that are flashed just before a saccade are seen as compressed toward the saccadic target. Burr et al. used two responses, verbal reports of stimulus position and pointing with an unseen hand. The verbal responses indicated strong and reliable compression, while with the pointing responses there was no evidence of compression, subjects pointed accurately to the target.

De Grave et al.[1] cite several studies and describe an experiment that they carried out, pointing to effects of background ("context") on both motor and judgmental responses to illusions. They list several studies where a motor response is affected by an illusory or a moving background and claim that these show that motor tasks can be affected ("fooled") by illusions. There are several possible explanations for these results; one is that in spite of the fact that motor responses were required, these studies actually tapped ventral responses. There seems to be a mistaken assumption in some of the commentaries that all motor responses are totally controlled by the dorsal system. There are quite a few lines of evidence that show that motor responses can also be under ventral control, such as those made during pantomime (e.g., Westwood et al. 2000), or motor responses that are made after a delay (e.g., Hu & Goodale 2000), or in the case of patients suffering from optic ataxia (e.g., Perenin & Vighetto 1988). What is more, the indications in some studies that a moving background influences a motor response can also, perhaps, result from interference between two basically dorsal perception tasks carried out simultaneously.

In their commentary **de Grave et al**. very briefly describe an experiment they conducted utilizing the Roelofs effect (see section 3.1 for a short description of this effect). Both the frame and the target could be shifted to the left or to the right and were presented in the dark. Unlike Bridgeman et al. (1997) who examined both judgmental and pointing responses, they only requested judgmental responses. They compared a condition where the subjects did not know whether they would be questioned about target or frame position with one where the subjects knew that they would be questioned about the target position. They found the Roelofs effect only in the "question known" condition, not in the "question unknown" condition. They see these findings as indicating that the effect of an illusion on a judgmental task is dependent on the exact conditions tested. It is difficult to comment on these findings in the light of the few details describing the exact methods used. It would seem as if the illusion is much less effective when one pays attention to the entire display rather than to the target alone.

Two commentaries deal with the effects of visual illusions on eye-movements. **Elliott et al**. cite several studies where the amplitudes of saccadic eye-movements to the vertices of Müller-Lyer figures are influenced by the illusion, unlike hand movements to the same targets. As they point out, these findings ostensibly raise problems for the dual-process approach. Saccadic eye-movements are considered to be very rapid and under unconscious control by the dorsal system. Then why should they be influenced by a visual illusion when hand movements are not? Elliott et al. suggest that these findings indicate that there is fairly early interaction between the two systems, and I tend to agree. As **Hochberg** pointed out in his commentary, saccadic eye movements play a central role in perception in that they bring the fovea to the areas of greatest interest in the visual environment. This would seem to call for some involvement of the ventral system in the control of these movements. The data presented in the studies quoted also appear to indicate some involvement of "higher processes" (also see discussion of the **McCarley & DiGirolamo**'s commentary following this). The mean reaction times of the saccades range between about 290 and 340 msec (Binsted & Elliott 1999b; Binsted et al. 2001), which is considerably more than found for regular (not express) saccades, the latter ranging approximately between 150 and 200 msec. These values appear to indicate that the saccades are under cortical control and not simply collicular control.

Elliott et al. also note a study (Proteau & Masson 1997) that showed that rapid aiming movements are influenced by a moving background ("contextual flow"). They suggest that as the aiming movements consist of moving a cursor with the aid of an isometric ("force") joystick – an "indirect task" in their terms – it involves the ventral stream. I would concur with this analysis and add two other reasons that indicate that ventral control was involved; a single "perturbed" trial with a moving background was interspersed between seven unperturbed trials, and feedback on performance was provided to the subjects after every trial. The fact that aiming movements can be performed under ventral control leads them to suggest that the ventral stream can operate very rapidly. In comparing the speed of the two systems (sect. 3.4.4), I wrote that "there are perceptual activities that clearly include a ventral component, such as reading, that appear to be carried out with extreme speed." In other words, though I feel that on the whole the dorsal system is the faster of the two, the ventral system can function very rapidly as well. It should also be noted that the study in question did not call for extremely rapid responses:

> Subjects were instructed to initiate their responses as they pleased and to try and make the cursor move and then stop on the target in a single continuous movement ranging in time from 450 to 550 msec. It was made very clear that this was not a reaction time task. (Proteau & Masson 1997, p. 731)

While half a second does sound fast, this is not really very fast for a motor response not requiring arm movement. Perhaps that is enough time for ventral-dorsal interaction. We do need to have access to much faster movements, such as when we quickly raise an arm to protect our head from a ball batted directly at it, and I would contend that such an action is totally under dorsal control.

McCarley & DiGirolamo's commentary also describes a study showing that saccadic eye-movements to the vertices of a variant of the Müller-Lyer illusion are influenced by the illusion. My response to this is identical to the one I have given to **Elliott et al**. above, but McCarley & DiGirolamo's study further strengthens the idea of the possible involvement of ventral processing in such eye-movements under certain conditions. In this study, rather than have the subjects move their eyes the length of the line, subjects fixated at the middle of the display and a light flashed at one of the vertices. The subjects had either to look towards the point of the flashing light or towards the vertex at the end opposite to that flashing. Saccades to the flash are called prosaccades and considered to be endogenous, while saccades in the opposite direction are called antisaccades and are considered to be exogenous. Although the commentators emphasize the fact that the illusion influenced both types of movements, I would point to the fact that the antisaccades indicated significantly greater effects of the illusion. This, I would contend, might be seen as greater ventral involvement in these eye-movements, under those conditions that are seen to be under greater control of top-down processes.

R3.8. On affordances

Two commentaries, by **Postma et al**. and **Chella**, touch upon Gibson's very important concept of affordances. Their comments indicate that the term "affordance" has more than one usage, not always consistent with Gibson's original aims, or my interpretations of them. **Chella**, for instance, talks of "conceptual affordances" which I feel is a somewhat different usage from what Gibson (1979/1986) originally proposed. In a similar manner, **Postma et al**. suggest that the processing of affordances is likely to occur not solely in the dorsal system, but in the ventral system as well. Among the reasons they give for this claim is the fact that patient DF has trouble in selecting the correct part of an object to grasp when that object is presented in an unusual orientation (Carey et al. 1996). Postma et al. suggest that DF does not have access to affordances that are processed in the ventral system. I would suggest, instead, that Gibsonian affordances are processed only in the dorsal system. Gibson (1979/1986) was very clear in stating that affordances are perceived directly; and according to the dual-process approach direct perception is carried out by the dorsal system (see target article, sect. 4.1). Others have taken the Gibsonian concept of affordances and broadened it to include objects and events that cannot be perceived directly. It would be helpful to differentiate between these two kinds of affordances. The first, I will label here, *physical affordances* (following Neisser 1989) and the second, *learned affordances.* As I pointed out in the target article, the physical affordances are the functional properties of objects that can be picked up directly by the dorsal system. In contrast, there is the perception of affordances of man-made tools or other objects that have uses which are not intrinsic to their exact structure, and their exact use has to be learned. These are the learned affordances. It should quickly be added that learning can also be involved in the case of the physical affordances, albeit a somewhat more implicit kind of learning, where through practice the organism learns to better differentiate the physical affordances of the objects and events in its environment.

Returning to the example of DF, with her dysfunctional ventral system, as raised by **Postma et al**.: DF can pick up only physical affordances, such as the "passability" of obstacles in her path or the "sit-on-ableness" of surfaces in her immediate environment. However, she has no access to learned affordances, and when faced with lifting objects that have their handles facing away from her, does not grasp them by their handles, as a healthy subject would do. The study on patient D.F. is but one of a number of neuropsychological studies that corroborate this distinction between physical affordances and learned affordances. Two other studies should be mentioned. One, briefly described in Note 6 of the target article, is a report by Hodges et al. (1999) on two patients suffering from semantic dementia who could not name or correctly handle man-made objects, but were as good as healthy subjects in picking up the (physical) affordances of novel tools. In a subsequent study, Hodges et al. (2000) tested nine patients with semantic dementia. These patients evinced normal mechanical problem solving in the use of novel tools, but their use of man-made objects was impaired and the level of performance was correlated with the naming and semantic knowledge. The good performance on the novel tool task, I would suggest, was due to pickup of physical affordances by the dorsal system, while the poor performance on man-made object use was due to their inability to perceive learned affordances.

The idea of dorsal pickup of physical affordances and ventral perception of learned affordances are not only corroborated by neurological findings: a recent experimental study by Creem and Proffitt (2001) on healthy subjects can be seen as strengthening this idea. These researchers utilized a dual-task paradigm with the aim of taxing the perceptual system, or systems. In the first experiment, one of the tasks was the grasping of hand tools placed in an irregular orientation, and the other task was either a paired-associates task (semantic dual task condition), or a spatial imagery task (spatial dual task condition), or no second task (control condition). In the semantic condition participants typically did not grasp the hand tools appropriately by their handles, whereas in the spatial and control conditions they did. In other words, in this experiment the grasping task was based on perceiving a learned affordance, and when the second task was a ventral task (paired-associates) it taxed the ventral resources and the subjects performed poorly. But when the second task was a more dorsal task (spatial imagery) or when there was no second task, the grasping was appropriate. In the second experiment, the main task was a much more dorsal motor task, pursuit-tracking, with the same secondary tasks, including the control condition. Here, the results were just the opposite of the first experiment; the tracking task was impaired by the spatial task and not by the semantic task or the control task. In other words, there was interference between the dorsal pickup of affordances in two simultaneously performed tasks. Of course, these findings are open to other interpretations, but they are also consistent with the proposals made here.

R3.9. Social/interpersonal perception

The topic of social/interpersonal perception is beyond the scope of the current undertaking. However, in the target article Neisser's (1994) tripartite division of perceptual systems was mentioned. Neisser suggested that in addition to the two systems discussed here a third system existed, which he labeled "Interpersonal perception/reactivity."

Green & Pollick's commentary dealt with this topic. And there are accumulating research findings that point to the links between the dual-process approach outlined here and social perception. This leads me to briefly touch on it. What is more, a short while after submitting the target article, I saw a large book on a colleague's desk entitled *Dual Processes in Social Psychology* (Chaiken & Trope 1999). As he, Asher Koriat, is a cognitive psychologist with expertise in memory and metacognition, I asked him about the book. I was surprised to learn that he had written a chapter in the book on metacognitive judgments, and even more surprised to learn about the amount of work being done in social psychology on dual modes of social information processing. In his commentary, **Toates** called our attention to the pervasiveness of dual-process approaches in many fields of the behavioral sciences, and social psychology appears to be one of them. As my knowledge of this field is limited, I want to only briefly mention a couple of interesting connections between visual perception and social/interpersonal processes that I have happened on.

In their commentary, **Green & Pollick** point to the connections between biological motion (BM) displays and social/emotional perception. Clearly, our ability to discern many facets of human motion from these patch-light displays is relevant to social perception. The ability to perceive emotion from BM displays of dance (Dittrich et al. 1996) is but one example of this. An interesting connection emerges at the neural level. As was discussed in section R3.4, neurophysiological studies in monkeys and neuroimaging studies in humans have shown that an important brain center activated by BM is the superior temporal sulcus (STS). Interestingly, many studies show that the STS is involved also in the processing of a wide assortment of visual stimuli that are related to social perception. Allison et al. (2000) survey experimental studies that indicate that the STS is activated by viewing movements of eyes (gaze direction), mouth, head, hands, and body. They also point to the connection with the two visual systems, writing,

> Parts of the monkey STS receive input both from the ventral object recognition system (the "what" system) and from the dorsal spatial location-movement system (the "where" system) suggesting that this region integrates information about form and movement. (p. 275)

These authors also note that the STS also responds to static images when those images depict an implied movement (Kourtzi & Kanwisher 2000), and that it has been suggested that the STS is responsive to images that signal intentions or intentional activity (Gallagher et al. 2000). Allison et al. (2000) also note that the STS projects to the amygdala and the orbitofrontal cortex, and suggest that these three together are the neural substrate involved in social cognition.

The fact that the STS is seen to be involved in the processing of visual stimuli of a social nature and receives inputs from both visual systems, dorsal and ventral, appears to indicate that both systems are involved in social perception. However, there seems to be evidence that some of the functions of the social perception system resemble those of the dorsal system more than those of the ventral system. This assertion is based on many studies in the social psychology literature that point to the automaticity of these perceptions (see e.g., Bargh 1997; Bargh & Ferguson 2000). The terminology used by social psychologists working in this area is of pre-conscious automatic perception of socially relevant stimuli. This preconscious automaticity resembles the dorsal system to a much greater extent than the ventral system. However, the stimuli of relevance are, at times, quite different from the stimuli that were seen to be relevant to the dorsal system in its perception of the ambient environment. For example, in one of the priming experiments reported by Bargh et al. (1996) on the effects of stereotyped attitudes of White Americans toward Black Americans, the stimuli consisted of very brief ("subliminal") presentations of masked faces, either of Black Americans or of White Americans. The results indicated more hostile responses in White subjects after presentation of Black faces than after White faces. Faces are stimuli that would not seem very appropriate for dorsal system pickup as they require something like a recognition response; for example, "subliminally" recognizing that the face is of a Black person or a White person. However, perhaps very familiar and highly over-learned stimuli like human faces can be processed in a manner similar to that of the dorsal system, by built-in hardware that responds quite automatically to those stimuli (see discussion of face stimuli in developmental studies in R3.2 above). It should also be added that experiments have obtained very similar results using visually presented words (rather than faces) as primes. It is even more difficult to incorporate verbal stimuli into a conceptualization of the dorsal system functions. This is somewhat reminiscent of Marcel's (1998) astounding findings of blindsight patients picking up word meanings in their "blind" field, briefly mentioned in section 5.1 of the target article.

R4. A final word

When I first submitted the target article nearly three years ago I was asked to write a short statement giving the rationale for soliciting commentaries. That statement ended with: "Hopefully, my response to the commentaries will include not only rebuttals of critical commentaries but also a revised version of the dual-process approach in the light of the commentaries." It is now clear that I set my hopes too high and that a revised version did not emerge from the accumulated knowledge espoused in the commentaries. Some contributions did, of course, add to the ideas suggested in the target article: such were **Neisser**'s suggestions that the dorsal system really comprises two subsystems, one, the system I wrote about, and a second one for a larger environment that includes an "ecological self." The latter subsystem, unlike the first, Neisser suggests, is conscious. In addition, **Hochberg** pointed to a major shortcoming in the present account; it does not examine the important role of saccadic eye-movements in the pickup of visual information. Quite a few commentaries touched upon topics somewhat ancillary to the central issues of the target article, but these commentaries did raise interesting questions that clearly need more research. Examples are: how memory and learning are involved in the two systems, or how the two systems develop, or how the two systems interact in the rather special phenomenon of biological motion, among others.

Some of the commentaries appeared to seek out specific indications of negative experimental results in an effort to negate the dual process approach. An example is the recourse to studies that have yielded negative results in the dissociation of the responses of the two systems to visual illusions. The problem is that these are far from "crucial experiments" and it was shown that other recent studies yield positive results. The dual-process approach will not fall or stand on the basis of the results of one or several experiments on a single prediction it generates, but on the accumulation of many research findings that will at best lead to refinements and variations on the original ideas, and at worst to a complete abandoning of the ideas it entails. The ideas expressed in the target article resulted both from my search for explanations for phenomena in the realm of size and distance perception, and for a "middle road" that would allow for the coexistence of two contrasting theoretical approaches, the ecological and the constructivist, both of which appeared to me to make contributions to our understanding of perception (in its broadest sense!). The commentaries as a whole left me with the feeling that the dual-process approach is still very viable, but I am sure that it will be refined and sharpened with accumulating research findings.

NOTE

1. These commentators note in their abstracts that I identify the issues of interest as perception and action. This was not my intention. I am interested in two *perceptual* systems: one, the dorsal, often but not always involved in the control of action; the other, the ventral, a more conscious system, often involved in recognition and identification but also at times involved in the control of action. To function efficiently in one's environment requires a great deal of synergistic interaction between the two systems.

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Letters "a" and "r" appearing before authors' initials refer to target article and response, respectively.

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