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Continuing Commentary

Commentary on **T. A. Stoffregen & B. G. Bardy (2001). On specification and the senses. BBS 24(2):195–213.**

Abstract of the original article: In this target article we question the assumption that perception is divided into separate domains of vision, hearing, touch, taste, and smell. We review implications of this assumption for theories of perception, and for our understanding of ambient energy arrays (e.g., the optic and acoustic arrays) that are available to perceptual systems. We analyze three hypotheses about relations between ambient arrays and physical reality: (1) that there is an ambiguous relation between ambient energy arrays and physical reality, (2) that there is a unique relation between individual energy arrays and physical reality, and (3) that there is a redundant but unambiguous relation, within or across arrays, between energy arrays and physical reality. This is followed by a review of the physics of motion, focusing on the existence and status of referents for physical motion. Our review indicates that it is not possible, in principle, for there to be a unique relation between physical motion and the structure of individual energy arrays. We argue that physical motion relative to different referents is specified only in the *global array,* which consists of higher-order relations across different forms of energy. The existence of specificity in the global array is consistent with the idea of direct perception, and so poses a challenge to traditional, inference-based theories of perception and cognition. However, it also presents a challenge to much work within the ecological approach to perception and action, which has accepted the assumption of separate senses.

Specificity in a global array is only one possibility

Eric L. Amazeen and Guy C. Van Orden
Department of Psychology, Arizona State University, Tempe, AZ 85287.
Eric.Amazeen@asu.edu

Abstract: The suggestion of seeking specificity in a higher-order array is attractive, but Stoffregen & Bardy (S&B) fail to provide a compelling empirical basis to their claim that specificity exists solely in the global array. Using the example of relative motion, the alternate hypotheses that must be considered are presented.

Many common perceptions are functions of multiple energy arrays, just as many common behaviors have multiple physical referents. In their target article Stoffregen & Bardy (S&B) (2001) took these facts as evidence that perceptual specificity exists solely in a global (higher-order) array. We agree that specificity may, at times, exist in the global array. However, concluding that specificity exists *only* in the global array – that there should be only one "global" mode of perceiving – is premature. We consider the conclusions of S&B to be one of a number of testable hypotheses. Although S&B admit that such testing should be pursued, their continued insistence on one type of specificity is inconsistent with this position. Testing for the existence of one possibility requires acceptance of the possible existence of alternative possibilities. Here we outline alternate hypotheses that must be considered in order to test for specificity in the global array.

The alternative hypothesis that information is detected in separate arrays, only to be combined later, is not an easy option to reject. Apparently, S&B assume that perception is straightforwardly a function of the global array when the perceptual report is a function of relations among energy arrays. In the moving room paradigm of Lee and Lishman (1975), for example, verbal reports and postural behaviors appear to be influenced by both the mechanical stimulation from a moving floor and the optical stimulation of the moving walls. S&B would like to conclude, then, that the action is specified only in the global array. Although this conclusion is attractive, it may not be warranted. The problem follows from the fact that a perceiver's perception and a perceiver's report (or performance) may not be the same (cf. Mandler 1985; Van Orden & Jansen op de Haar 2000). Consequently, the observation that a combination of energy arrays determines a report need not imply that it likewise influenced perception. S&B use this distinction to motivate their reinterpretation of subjective reports, but do not acknowledge that this distinction also weakens their conclusion.

We can generate a simplified model of relative motion in a moving room on the basis of General Recognition Theory (GRT; Ashby & Townsend 1986). This model will be used to illustrate the alternate hypotheses that are generated from a distinction between perception and performance. GRT theory is an extension of signal detection theory in which stimulus information gives rise to a distribution of perceptual effects. When faced with making a response, an individual applies a decision criterion to the perception in order to determine the appropriate response. A given perceptual report, then, is a function of both the sensitivity of the perception to variations along a physical dimension and the decision criterion that is employed. In a moving room, information is available from both the gravitational field (indicating no motion) and the optic array (indicating motion). Assuming that they may be detected separately, we may tentatively conclude that there is a perception of no motion relative to gravity and a perception of motion relative to the walls. These perceptions will result in a postural response when they are combined with some rule(s) for the control of behavior, such as "maintain a fixed distance between the head and the forward wall." So there are three components to the postural adjustments observed in a moving room: the information, the detection of that information, and the generation of a response on the basis of that information.

In a moving room, movements of the walls produce postural adjustments that would be consistent with movement relative to gravity. But where exactly are the influences of gravity and the optic array combined? Our simplified model shows that these two influences may be combined or separated in any of the three stages. A conclusion of specificity in the global array, however, refers only to a combination of information. Accordingly, there would be a

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perception of motion relative to the floor that is a function of both mechanical and optical stimulation (combined in the global array) and the postural adjustments would be some function of this perception. If this model were correct, then there would be ambiguity in either single-energy array, as S&B suggest. However, observing postural adjustments in a moving room does not require us to accept this conclusion. The seeming combined influences of gravity and optics may occur in the perceptions themselves. For example, one may have a perception of moving relative to the floor that is influenced both perceptually by the mechanical stimulation from the floor and cognitively by the perception of moving relative to the walls. This option is commonly referred to as percept-percept coupling and does not imply higher-order invariants in the global array. Moreover, the influences of gravity and the optic array could be combined in the generation of the response. For example, imagine that the perception of moving, relative to the floor, is strictly a function of mechanical stimulation; and the perception of moving, relative to the walls, is strictly a function of optical stimulation. The two perceptions do not influence one another directly. Nevertheless, both perceptions may influence the report if the observer is misled to believe that the floor and walls must move together.

Like the authors, we are intrigued by the theoretical possibility of higher-order, or even global, invariants. But S&B fail to demonstrate a compelling empirical basis for their hypothesis. To do this, they must first acknowledge the alternate hypotheses, because these hypotheses remain valid alternatives that must be rejected. Failing to do so could lead to the inappropriate conclusion that there is information-action specificity in a higher-order array when, in fact, the data show only a cognitive effect or a response bias. In the end, it is impossible for S&B to conclude both that specificity in the global array is testable and that it is the only option.

Teleological perception without a biological perceiver?

Théophile Ohlmann[a], Bernard Amblard,[b] and Brice Isableu[c]

[a]*Laboratoire de Psychologie et de Neurocognition, UMR CNRS 5105, 38040 Grenoble CEDEX 9, France;* [b]*Centre National de la Recherche Scientifique, Laboratoire de Développement et Pathologie du Mouvement, 13402 Marseille CEDEX 20, France;* [c]*Centre de Recherche en Sciences du Sport, UPRES 1609, Division STAPS, Université Paris Sud-XI, 91 405 Orsay CEDEX, Batiment 335, France.* **theophile.ohlmann@upmf-grenoble.fr amblard@lnf.cnrs-mrs.fr Brice.Isableu@staps.u-psud.fr**

Abstract: Strong between- and within-animal differences during spatial activities lead us to claim that a given animal is directly sensitive to a given substructure of the global array. This vicarious subset is not cut out by the senses but by redundancies emerging from physical properties. We argue that the subset is not a single ambient array, or a combination of single ambient arrays, but a complex holistic part of the global array.

The assumption that perception is not divided into separate senses does not imply that animals are directly sensitive to the structure of the global array (GA). This is rather often not the case; because of the animal/environment mutuality, the GA should be broken down or filtered into subsets according to each animal's idiosyncrasy. We assume that these within- and between-animal differences are not linked to senses but to physical properties of the ambient array. In any case, animals can only pick out a subset of the physical reality. Actually, as they are driven by reinforcement (Skinner 1984), their major purpose is to achieve actions, and not merely to extract an accurate perception of the reality, an assumption which would be teleological. Therefore, some subsets of the reality are sufficient and the multidimensional GA is not necessarily systematically sampled out.

Strong between-animal differences both in spatial activities and in spatial disorders provide evidence that senses work in order to give a sufficient idiosyncratic perception based on a functional subset of the GA (Ohlmann & Marendaz 1991). Biological systems (Schull 1990) do not have a predetermined or a priori solution for the world with which they are coping. In mild conditions, such as walking regularly or stabilizing one's posture on a flat resistant surface, redundancies can give rise to precise covariations between the different subarrays. In such a case, a quasi single array is indifferently *sufficient* to control the task. Is there anybody or anything that obliges the animal to work at a higher level? This question has nothing to do with the issue of separate senses. Clearly, in the perception of a subset of the GA such gravito-inertial (GI) forces involve a large set of senses: Golgi receptors, vestibular system, motor proprioception, kidneys (Mittelstaedt 1997), tactile pressures, body fluids, and so forth.

Moreover, in many circumstances there is no need to perceive reality accurately; indeed, action should be easier if one discards some disturbing aspects of the reality. In many cases, animals actively or passively make use of filter-like systems which are brought into play by the characteristics of the situation and/or of the individual.

According to Kimura's "neutralism model" (Kimura 1968; Kimura & Ota 1972), the level of constraints directly entails consequences about the between-animal differences. When an animal is confronted with low constraints, redundancies lead to a vicarious diversity (Reuchlin 1978). Therefore, if some information (data, senses, tools, affordances, part of the GA, etc.) is substitutable for some other information, then a given animal will rely on one kind of information whenever it finds itself in a similar situation.

A clear example of the non-necessity of picking out the GA each time is given by the visual Romberg's Ratio (Amblard et al. 1985). Body movements are successively recorded in total darkness and in illuminated environment. Postural stability is dramatically increased when optics are available. However, there is a strong between-animals variability caused by the extraction of a non-GA, with some subjects (Lacour et al. 1997) keeping the same level of stabilization in darkness and in an illuminated environment. Isableu et al. (1997; 1998) have shown that field-independent subjects (Asch & Witkin 1948) do not need visual information to stabilize their body even in a complex stance. In order to achieve almost the same level of postural control, field-dependent subjects need full optical information. Some subjects appear to be sensitive to both *geometrical and kinematic* optical information (Guerraz et al. 1998), while others rely on *dynamics* (either static or kinetic). These subsets constitute vicarious referents not based on senses but on physical properties of information. Pick (1974) assumed that nonvisual spatial information can be "coded" in a visual mode because of physical properties of optics independent of the visual modality per se. This was expanded by de Volder et al. (1999) who demonstrated that early blind subjects, fitted with ultrasonic devices, exhibited a distinct activity (PET) in the primary visual area. Furthermore, some other subjects showed a high sensitivity to forces, whatever their nature (inertial, frictional, gravitational), which led them to refer primarily to moments of inertia (Pagano et al. 1996), static moments, or gravito-inertial forces. For example, they easily found subjective or postural vertical, either directly by vestibular system, tactile compression, interoception, or by the dynamics of balance (Stoffregen & Riccio 1988; Riccio et al. 1992). Their superiority in any domains involving moto-somato-sensorial control could be easily explained by a postural scheme extracted from the inertial tensor associated with each rotating corporal segment. Finally, this vicariousness even appears at cell level. Waespe & Henn (1977; 1979), showed that in vestibular nucleus of awake monkey, one given cell works either with a visual stimulus or an inertial stimulus, or with a combined visuo-vestibular stimulus.

Why are between-subject differences about motion sickness so dramatic? The postural hypothesis of motion sickness, developed by Stoffregen and Riccio (1991) is unable to account for these dif-

ferences. In some circumstances, accurate reality or global reality is not helpful for the animal, as is well illustrated by the tilting train. On a curve when the train cabin is quasi aligned with GI force, the subject has the feeling of being upright while the landscape appears tilted (Neimer et al. 2001). This outside visual flow is a useless referent and severe motion sickness can appear even in a seated reading subject. Closing the blinds to reduce the available information suppresses motion sickness by annihilating the conflicting information provided by the two visual referents (cabin and landscape). Actually, a strong correlation appears between motion sickness triggered by a tilting train and motion sickness induced experimentally during a previous exposition to an oblique rotating optokinetic cone (a control cone rotating in pure yaw exhibits no correlation with the train motion sickness). This implies that some subjects who usually rely on visual geometrics (cabin) and kinematics (outside flow) feel an intrasensorial conflict between referents.

In conclusion, it seems that Stoffregen & Bardy's (2001) heuristic approach to perception may appear, in some circumstances, as unrealistic because of its excessive generality. We agree with the view that each situation is specified by the global array; however, we claim that different animals perceive different subsets of specification. In any case, these differences are piloted by the characteristics of the senses. If we take the Gestalt example used in the target article of the perception of a triangle, animals perceive a part of the whole – that is, they perceive an incomplete triangle which is not an isolated element but a sufficient substructure. This might explain why such theoretical positions as amodality, functional modality, intermodality, and multimodality are sometimes simultaneously possible.

On the subject of perceptual illusions, and the ambiguity of perceptual information

Arve Vorland Pedersen[a] and Hermundur Sigmundsson[b]

[a]*Department of Physiotherapy, Sør-Trøndelag University College, N-7004 Trondheim, Norway;* [b]*Faculty of Social Sciences and Technology Management, Norwegian University of Science and Technology, N-7491 Trondheim, Norway.* **Arve.Pedersen@ahs.hist.no**
Hermundur.Sigmundsson@svt.ntnu.no
http://www.svt.ntnu.no/iss/hermundur.sigmundsson/

Abstract: Stoffregen & Bardy (S&B) make the point that the statement "I am moving" made by subjects in a "swinging room" cannot be explained as an illusion of motion, and there is thus no perceptual illusion. In this they are correct. There is in fact motion, but of the environment. We argue that the subjects misinterpret this because the information to the visual system is ambiguous and also deceiving.

In their target article, Stoffregen & Bardy (S&B) (2001) discuss the frames of reference for motion and use as an example perceptual illusions. They suggest a reinterpretation of subjective reports of physical motion, which are to be found in studies in which perceptual information is manipulated (e.g., Dichgans & Brandt 1978; Lishman & Lee 1973).

The statement "I am moving," reported by subjects in the above-mentioned experiments, is said by S&B to be ambiguous because there is more than one frame of reference. The "swinging room" (Lishman & Lee 1973) or "rotating drum" (Dichgans & Brandt 1978) is one reference frame (the illuminated environment), and the earth is another. S&B say that subjects stating "I am moving" are correct because they are in motion relative to one of the relevant reference frames, namely, the illuminated environment. This interpretation we consider to be wrong – at least when, like S&B, we are not talking about normal postural sway. What is really ambiguous is not the statement "I am moving" but the perceptual information. The problem that subjects encounter in the described situation is that the visual system cannot uniquely specify motion relative to the environment as self-motion. Gibson (1968) suggested that one possible way for the visual system to distinguish between self-motion and motion of the environment is to detect part-whole differences: "motion perception caused by locomotion entails change in the whole of the textured ambient array whereas the alteration of perspective caused by an objective motion entails only change in part of the ambient array, the remainder being frozen" (p. 187). In the Lishman and Lee (1973) and Dichgans and Brandt (1978) studies, there are no such part-whole differences because the whole room (or drum) is being moved. A change in position relative to the environment, therefore, looks exactly the same whether the change is caused by self motion or is caused by motion of the environment. This is exactly what the subjects encountered. The change in position relative to the environment was caused by motion of the environment and not by any movement, active or passive, of the subject.

When subjects are faced with such a situation, they most often misinterpret this relative motion as self-motion. We argue (cf. Pedersen 1999; 2000) that this is not because – as Lishman and Lee (1973) and later Lee and Aronson (1974) and Lee and Lishman (1975) suggested – the visual system is the dominant system. Neither is it because, as S&B argue, the subjects are in motion relative to the (illuminated) environment, or relative to anything for that matter. It is because the information provided to the visual system is ambiguous, and in this situation, also deceiving.

S&B would, however, seem to be right when they state that this is not a perceptual illusion because there is, actually, motion; therefore, the perception is correct. The subject's visual system does detect motion of the room. The mechanical proprioceptors are also correct because they detect that the body is stationary, and they are not concerned with motion of the room at all. Why would the subject, then, interpret this as self-motion? The problem lies, as mentioned, in the information, which is deceiving because the fact that the room moves is inconsistent with all the subject's experience so far, whether as infant (Lee & Aronson 1974) or as adult (Dichgans & Brandt 1978; Lee & Lishman 1975; Lishman & Lee 1973). This means, in Gibson's (1966) terms, that a genuine biological invariant ("rooms do not move") has been destroyed. In such a situation, says Gibson, a subject must either accept the visual information and reject the postural (mechanical), or accept the postural (mechanical) and reject the visual, or alternate between the two, or compromise between the two. Of course, still according to Gibson, the subject may sometimes just be confused (Gibson 1966, p. 297). In the studies referred to (Dichgans & Brandt 1978; Lishman & Lee 1973), the subjects perceived that something was, in fact, moving, and they reasoned that it could not (at least it should not) be the room (or drum). The information was, therefore, interpreted as propriospecific when it was actually exterospecific.

On ventriloquism, audiovisual neurons, neonates, and the senses

Monique Radeau and Cécile Colin

National Fund for Scientific Research (FNRS) and Université libre de Bruxelles, Brussels, Belgium. **moradeau@ulb.ac.be** **ccolin@ulb.ac.be**

Abstract: The analogy between the rules that subtend ventriloquism and bimodal neurons responding suggests a possible neural mechanism for audiovisual interactions in spatial scene analysis. Perinatal data, such as those on synesthesia, sensory deprivation, and sensory surstimulation, as well as neuroanatomical evidence for transitory intersensory connections in the brain support the view that audition and vision are bound together at birth.

As illustration of sensitivity to patterns that extend across the acoustic and optic arrays, Stoffregen & Bardy (S&B) (2001) quote the McGurk effect observed in speechreading (McGurk & McDonald 1976). Additional evidence is provided by the ventrilo-

quism phenomenon that occurs in spatial scene analysis. When light flashes and sound trains come from moderately separated locations, the spatial separation is generally unnoticed, and perceptual recalibration is also manifested in aftereffects (Radeau & Bertelson 1974). The criteria for pairing visual and auditory signals from different locations are sensory factors like the timing of the signals (although strict synchrony is not required) and the distance between them. As demonstrated in barn owls raised from birth to adulthood with prisms (Knudsen & Knudsen 1989) and in human adults (Colin et al. 2001), ventriloquism decreases with increasing spatial separation, being maximal until 20°. Cognitive factors do not play any role. A context simulating a real-life situation, such as seeing the face of a speaker or the hands of a man playing bongos while hearing the sounds displaced, does not enhance ventriloquism beyond the level reached in more artificial situations, as when diffuse light is modulated by the sounds (Radeau & Bertelson 1977) or when the speaker's face is presented inverted (Colin et al. 2001). The system underlying ventriloquism has been considered as being based on primal knowledge of the Gestalt principles of common fate and proximity (Radeau 1994a), used both in visual grouping and in "auditory scene analysis" (Bregman 1990).

Contrary to the ventriloquism effect, which concerns localization, the McGurk effect concerns speech identification and is subtended by different spatial and cognitive rules. It is unaffected by the degree of spatial separation between the signals (Colin et al. 2001), but it decreases in cases of face-voice gender discrepancy for familiar speakers (Walker et al. 1995) or of face inversion (Bertelson et al. 1994; Colin et al. 2001; Jordan & Bevan 1997; Massaro & Chen 1996).

The two effects are probably achieved by specific mechanisms in a way consistent with their different functions (Radeau 1994b). Neurophysiological studies of vision in nonhuman primates have provided evidence for the "what" and "where" problems involving distinct neural pathways (Ungerleider & Mishkin 1982). Recent neuropsychological data from human patients with left hemisphere lesions argued for a "what" versus "where" distinction in the auditory modality as well (Poremba et al. 2003).

The discovery of multimodal neurons helps in understanding crossmodal responses because sensory convergence on individual neurons may well be the underlying neural mechanism. Multisensory neurons have been found in many species and in many parts of the brain. Especially relevant here are the audiovisual neurons found in the deep layers of the superior colliculus and in the polysensory cortex of cat and monkey (Stein & Meredith 1993). Although these neurons often fail to respond to unimodal stimulation, they exhibit vigorous responses under bimodal stimulation provided the stimulations come from locations not too far apart. Enhancement is eliminated around 20° of spatial disparity, and it is inversely related to temporal disparity without being restricted to temporal coincidence. The rules that govern responses of audiovisual neurons are therefore very similar to those that underlie ventriloquism, so these neurons could well constitute the neural substrate of this phenomenon.

What about the development of multisensory functioning? Does it result from amodal representations that are functional early in life or is it learned from experience of co-occurrent unimodal informations, as assumed by empiricist philosophy?

Probably due to the immaturity of the superior colliculus of the newborn cat, there is no evidence for multisensory enhancement before several weeks after birth (Stein et al. 2000). However, there is much behavioral evidence to indicate that there is a primitive unity of the senses, the sensory systems becoming gradually differentiated during development (Bower 1974; Gibson 1966; Marks 1978). In the first months after birth, neonatal humans (Lewkowicz & Turkewitz 1980) and rats (Spear & McKinzie 1994) respond to stimulation in all modalities; further, these responses are dominated by quantitative aspects of the stimulation without distinction of modality. On the other hand, synesthesia (joined sensation) is very important in the first month of life and decreases during development, being two to three times more frequent in infants than in adults (Marks 1975; Maurer 1993).

Data from studies on perinatal sensory surstimulation or sensory deprivation also provide support for early auditory-visual connections. Unusually early experience in a late-developing system interferes with sensory functioning in earlier-developing systems. Exposure of bird embryos to visual stimulation several days prior to hatching results in an auditory deficit, with ducklings (Gottlieb et al. 1989) and quail chicks (Lickliter & Banker 1994) failing to learn the maternal call.

Moreover, perinatal deprivation in a sensory system can affect functioning in the remaining modalities. Deprivation of patterned visual stimulation by binocular eyelid suture in ferrets (King & Carlile 1993) and barn owls (Knudsen et al. 1991) results in anomalous responses of auditory neurons.

Visual event-related potentials (ERPs) have been recorded in congenitally deaf cats (Rebillard et al. 1980) and humans (Neville 1990) over temporal brain areas, which in the hearing subject contain the auditory cortex. However, there was no change in humans who became deaf after the age of four years. Moreover, in congenitally blind humans, auditory and somatosensory ERPs have been found to have a more posterior distribution than in control subjects (Kujala et al. 1992; 1995). The observed compensatory changes can thus reflect stabilization of transitory connections in one modality (Changeux & Dehaene 1989; Edelman 1987) in the absence of competing input from another modality.

There is some neuroanatomical evidence for transient auditory to visual cortex connections around birth that disappear in the fourth week of age in the kitten (Innocenti & Clarke 1984) and in the ferret (Kennedy & Dehay 1993). Connections have also been found between the retinas and the somatosensory and auditory nuclei of the thalamus in the hamster less than 1-week old (Frost 1990). In primate newborns, auditory ERPs have been recorded over the occipital visual cortex of human 6-month-old babies but not in older children (Neville 1995).

All of these data argue for initial sensitivity to structures in the global array, experience probably leading to sensitivity to structures in single-energy arrays.

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Retinae don't see

John T. Sanders
Department of Philosophy, Rochester Institute of Technology, Rochester, New York 14623. **jtsgsh@rit.edu**

Abstract: Sensation should indeed be understood globally: some infant behaviors do not make sense on the model of separate senses; neonates of all species lack time to learn about the world by triangulating among different senses. Considerations of natural selection favor a global understanding; and the global interpretation is not as opposed to traditional work on sensation as might seem.

As Stoffregen & Bardy (S&B) (2001) contend, the theory of "direct" perception does indeed seem to be incompatible with the idea that sensory data should be understood as being gathered independently via several senses. In suggesting that the conflict be resolved by rejecting the latter idea, rather than simply by abandoning the theory of direct perception, they choose the more controversial route. In this brief response I offer a few further reasons to take their suggestion seriously.

In the first place, some infant behaviors simply do not make sense on the assumption that at birth the several senses begin providing independent information which can be brought together only after further experience and comparison. Excellent examples

of such behavioral phenomena are provided in the studies of infant imitation done by Meltzoff and Moore (1977; 1983a; 1983b) in which they found that infants can contort their faces in imitation of another person's facial expressions within minutes of birth. Since these children have no idea how their faces look, visual cues thus seem to be leading to direct and immediate matches with tactile/proprioceptive sensations, with no time and no mechanism available for learning about correlations among distinctly apprehended sensory modalities.

Second, consideration of experiments like these suggests, more generally, that the early lives of neonates of all species must be rich in similar cross-modal sensory integration, given the sophistication of what they are able to do almost immediately. Although there is certainly no reason to argue that the world *never* presents itself to animals via singular sensory modalities, such presentations must be rare. Yet animals need to work with information in their environments almost immediately, and it is often the nuanced information that crosses and combines modalities that is most crucially needed, whether for balance, for reaching, for flight, or whatever. Animals simply do not have time to learn about the world by triangulating among independent contributions of several different senses.

Third, the idea that sensation is primitively multimodular makes considerable evolutionary sense. It seems likely that the several sense organs of each species have evolved as specializations of earlier less-specialized organs, based on the proven value of each specialization in enabling species members to survive and propagate. More general sensation must arrive on the evolutionary scene earliest, and then becomes more specific as a result of the contingencies of the niche. This seems consistent with a treatment of sensation that understands it most primitively as a global sensitivity to the environment, focused by opportunities and dangers available there, organized and differentiated by natural selection over time in terms of various sensory surfaces.

Finally, students of perception have frequently disagreed about how to understand the role of the several sensory modalities. It has seemed plain to anyone who has ever thought about sensation that colors are remarkably different from sounds, and although this has seemed to require a sharp distinction among the contributions of the several senses, there have also been suggestions that this distinction must be moderated in any suitable analysis.

George Berkeley (1709; 1713; 1733; Jessop 1937), for example, tried to distinguish between "immediate and proper" seeing and a more liberal sense of "seeing." The first – the bare immediate and proper "seeing" – is to be understood as uninterpreted, stripped of all learned associations. But Berkeley's own texts show that he was himself very uncomfortable with the traditionally conceived implications of this distinction.

This discomfort emerges quite clearly in Berkeley's examination of whether distance can be seen. His view was that although it cannot be seen "of itself and immediately," it can nevertheless be seen in a less restrictive sense. Indeed, according to Berkeley, there are a great many factors involved in determining our visual perception of distance, one of which is plainly the apparent magnitude of the thing seen. But this did not lead him to conclude that distance is seen indirectly via such cues as size, since that latter perception is often based on how far away we think the object is. Neither cue is less "directly" perceived than the other. Each can help in discerning the other. Another factor involved in determining *both* perception of magnitude and perception of distance is the apparent faintness of what is seen. But that is simply another factor in a very complicated contextual situation.

Berkeley argued, finally, that what one sees with regard to distance and magnitude is determined in part also by the posture of the head and eyes, and perhaps with the help of contributions from other sensory modalities. What is important here, though, is his forceful argument that no "judgment" or inference is involved in such seeing – the distance is suggested immediately. In sum, Berkeley's claim is not really that we do not see depth, for he explicitly says that we do. But he insisted equally that it is *we* who see . . . not our retinae (see Sanders, forthcoming).

This line of thinking shows, in any case, that even Berkeley's thoughts on the subject examined by S&B were not as antithetical to their thesis as might be imagined. There is also support here for the idea that a theory of "direct" perception need not be as counterintuitive as has sometimes been maintained. The line of study urged by S&B would amount to a Gestalt switch of sorts, it is true. And it is important to acknowledge, even in their proposed research program, the importance of studying the separate sensory modalities in order to further understand their contribution to sensation. The upshot would be, though, that sensation would be understood not as taking place at sensory surfaces, in particular, but throughout a larger global sensory system which has those surfaces as parts.

Multi-sensory processing facilitates perception but direct perception of global invariants remains unproven

Lawrence Warwick-Evans

Department of Psychology, The University of Southampton, Southampton, SO17 1BJ, United Kingdom. **lawe@soton.ac.uk**
http://www.soton.ac.uk/~psyweb/staffpages/lawe/lawe.html

Abstract: The existence of sensory convergence does not establish that the senses function as a single unified perceptual system. Reality is fully specified only by a one:many mapping onto the totality of energy arrays, and these provide alternative frames of reference for movement. It is therefore possible that higher order crossmodal relationships are detected by skilled perceivers, but this has not been confirmed empirically.

After the initial agenda setting in the target article (sect. 1), Stoffregen & Bardy (S&B) (2001) consider in section 2 the three arguments (anatomy, energy, and neurophysiology) that are used to support the idea of the separate existence and functioning of each sense. They correctly reject the anatomy argument by pointing out that inputs are required from the two (anatomically separate) ears in order to localise sound; this establishes the point that anatomically distinct origins are not sufficient to preclude later functional interdependence. But the energy argument is not, as they claim, circular: the electromagnetic and vibrational energies to which the eye and ear respond can be defined independently of the eye and ear. The neurophysiology argument is a variation of the anatomy argument and similarly establishes that initial separation of sensory inputs is not evidence against later convergence and unified perception. In short, the anatomy and neurophysiology arguments are correct but not new, whereas the energy argument is incorrect. The authors' arguments from example do entail the limited (weak) conclusion that sometimes two or more senses act as a single perceptual system, but do not justify the universal (strong) claim that the senses never function as separate systems and that they all and always function together in a unitary and irreducible manner.

Section 3 of the target article distinguishes four possible relationships (no specification, modal, multiple independent, and multiple amodal specification) between reality and the ambient array. The authors conclude that "each (relationship) . . . is confronted with problems" (sect. 3.4) and that "all theories of perception derived from existing views of specification are compromised by fundamental errors" (sect. 1). Their pivotal reason for rejecting all four views is that (1) each view involves the possibility of sensory conflict, and (2) since the senses act as a unified single perceptual system, there cannot be conflict between the separate senses. This argument would not be sufficient to reject the four views even if the strong version of sensory unity were correct, because there is no a priori reason why there cannot be conflict within a system. Additionally, there is a major inconsistency between rejecting the independent amodal view of specificity defined as "a one:many mapping, with properties of reality being

specified by patterns existing in different ambient arrays"[1] (sect. 3.3.2) and later concluding that "specification exists . . . structure exists in the global array"(sect. 7). Furthermore, I would argue that there is no possible alternative to this view, not on any empirical grounds, but because it is a tautology: it is necessarily true since the reality of an object or event is *defined by* its effects on the ambient energy arrays. A more direct rebuttal of the argument that sensory conflict presents an insuperable problem for all four views can be derived from the fact that each sense uses a different form of energy for its frame of reference (see sect. 3.1.2, where this idea seems to be dismissed as the incorrect view of others, and sect. 5.1, where it is presented as their own correct analysis); consequently, there is no sensory conflict in the situation where one is stationary with respect to Newtonian three-dimensional coordinates but moving with respect to the illuminated or acoustic environment.

***2nd col.* Warwick CC on Stoffregen p. 5 Query** Warwick-Evans re the space here; is a "power of" decimal numbering in superscript missing here? Or should comma be added after "heaviness" and space closed up? – S.M. (Chief Copyeditor)

Section 4 reminds us that all movement is relative, that our senses provide movement information in relation to multiple, often independent, frames of reference, and that the relevant frame of reference for the control of behaviour is not always the surface of the earth. One might add that a much more widespread class of examples than diving for a fly ball is any action (e.g., pouring a drink) in a moving vehicle. It is, however, misleading to state without qualification that perception involves motions of receptor systems; for example, visual perception can be successful with the eye muscles anaesthetised and the head restrained by a dental bite, and one can correctly perceive speech via radio and telephone while motionless and therefore with no movement of receptors. In short, movement facilitates perception but is not essential.

Although the arguments in section 5 again show that reality cannot always be specified by one single energy array, they have two shortcomings. First, the examples here, as elsewhere in the target article, are limited to the McGurk effect and to movement. And what is unusually convenient about movement, but is not sufficient basis for the general conclusions that the authors wish to draw about all perception, is that, a fortiori, movement involves movement of perceptual systems. This is because almost every component of the organism is capable of responding to the energy fields associated with movement – for example, body surface, viscera, the flesh between skin and bone, free nerve endings, stretch-, pressure-, and angle-receptors, as well as otoliths, canals, auditory and visual systems.

Section 6 confuses physics with psychology. The *reality of the physics* is that the totality of energy arrays specify reality in two ways: first, by the state of each ambient array, and second, by the pattern of relationships across or between ambient arrays. It is not possible for one array to change without there being a change in at least some feature of the global pattern across the array, although a particular salient feature, such as the category of shapes conforming to triangularity, or the category of actions constituting destabilising movements, may remain an invariant. The claim that "the global array is influenced by events that do not structure single-energy arrays" is not supported by the subsequent examples of cars in motion or decelerating, static and dynamic ankle rotation, or Fouqué's pairwise comparisons; on the contrary these examples illustrate that changes in individual arrays entail a change in the global array. The *reality of the psychology* is uncertain: we *may* discern the separate activities of the different receptor types responding to different energy types, or the different activity in converging and diverging pathways and in the dedicated or shared projection areas; and by concurrently, or otherwise, processing this activity we may infer an appropriate percept. Alternatively, perception *may* be direct in the sense of being fully specified by the sensory input (including the perception of efference?) and requiring no memory, comparison, or inference, and what is directly perceived may be an intersensory superordinate relation. The heart of the authors' position is that there is direct perception of the left-hand side of the equation without the need for perception of the computational variables on the right-hand side. This latter view does not appear to be currently supported by any neurophysiological findings, nor could it ever be supported by a research agenda based on most of perceptual equations currently known, since these contain terms specific to only the same energy array (e.g., Equations 1 and 2 in the target article). Moreover, a study (Amazeen 1997) which yielded one of the few formulations relating perception to two types of energy array (perceived heaviness, dynamic touch$^{0.95}$, perceived volume $^{-0.18}$), also showed that each term from the right-hand side could be perceived separately.

To conclude, the article contains original analyses (types of specification, movement in relation to different frames of reference) and syntheses (the concept of sensitivity to the global array) and important practical recommendations (allowing participants to move during perception, assessing perception by action rather than by report); it demonstrates that in movement situations and sometimes in other situations veridical perception depends on more than one of the "separate senses"; it shows, perhaps unwittingly, that, within the limits of acuity and sensitivity, reality is fully specified both by the global array and by the total set of sensory inputs that constitute the global array; but it has not provided empirical evidence for direct perception of higher-order emergent properties in the global array.

NOTE

1. Although the word "redundant" appears in their first description of amodal specification, the authors later (sect. 3.3.3) ackowledged that this view includes both redundant and discrepant information.

Authors' Response

Theory testing and the global array

Thomas A. Stoffregen[a] and Benoît G. Bardy[b]

[a]*School of Kinesiology, Cooke Hall, University of Minnesota, Minneapolis, MN 55455;* [b]*Université Paris Sud-11, Center for Research in Sport Sciences, 91405 Orsay Cedex, France.* **tas@umn.edu**
benoit.bardy@staps.u-psud.fr **www.hfrl.umn.edu**
www.cress.u-psud.fr

Abstract: The new commentaries raise important issues about the target article (Stoffregen & Bardy 2001). The commentaries also highlight some assumptions, often implicit, that underlie traditional interpretations of perception. We argue that evaluation of the global array and its implications for perception requires both analytical research on specification in the global array and new empirical research on the use of information in the global array for the control of action.

R1. Physics versus psychology

Amazeen & Van Orden argue that the existence of specificity solely in the global array is a testable hypothesis, and they argue that other testable hypotheses also exist. We agree with both of these arguments (see sect. R12 in our response to the earlier commentaries).

In the target article, we made three claims. First, we claimed that specification cannot exist in single-energy arrays. Second, we claimed that specification exists in the global array. Third, we claimed that the information in the global array is necessary and sufficient for direct perception (i.e., perception that is accurate without inferential mental processing). This final claim led to the hypothesis that perceivers detect structure in the global array.

Amazeen & Van Orden, Pederson & Sigmundsson, and **Warwick-Evans** do not clearly distinguish between the claims that specificity exists in the global array and only in the global array, on the one hand, and the hypothesis that structure in the global array is detected by perceivers, on the other. Falsification of specificity in ambient arrays (the global array or any other) is an analytical problem, not an empirical problem (thus, specification is a testable hypothesis, but it cannot be tested using empirical data). As we noted in the target article and our response to the original commentaries (sects. 1, 6, R2, R5), the debate about the existence of specification is not about psychology but about ecological physics (Runeson & Vedeler 1993). Figure R1 illustrates the differences between reality, ambient arrays, and sensory stimulation (cf. Figure 1 of the target article).

The hypothesis of specification concerns relations between reality and ambient arrays. Characteristics of actual sensory stimulation are secondary, both temporally and logically, to relations between ambient energy and reality. Accordingly, empirical data, such as subjective reports and perceptually guided action, can be used to evaluate the detection and use of information in ambient arrays, but it cannot be used to determine whether patterns in ambient arrays bear a specificational relation to physical reality. Therefore, we regard the empirical effects discussed by **Amazeen & Van Orden, Pederson & Sigmundsson,** and **Warwick-Evans** as being irrelevant to the physical issue of specification in the global array, and having relevance only to the psychological issue of whether perceivers detect and use structure in the global array or in single-energy arrays. The best way to test our hypothesis about the pick-up of information from the global array is to develop methods that will permit it to be contrasted empirically with other, competing theories of perception (sects. 7, R12). An essential first step in such an evaluation is analytically to derive structures in the global array that specify some aspect of the animal-environment interaction and which, therefore, might be detected and used to perceive that aspect.

R2. Specificity cannot exist in single-energy arrays

Amazeen & Van Orden discuss perception in the moving room paradigm, and they consider three interpretations of how people may perceive motion relative to gravito-inertial referents and illuminated referents. One interpretation is ours: that perception and action are based on the pick-up of information from the global array. The two alternative interpretations they offer both assume that perception operates at the level of single-energy arrays. We regard it as unfortunate that they do not discuss how their interpretations relate to the concept of specification. One of our central arguments was that previous concepts of specification lead inexorably to discrepancies in the motions suggested by patterns in different forms of ambient energy, which entails a lack of specificity (sects. 3, 4, 5). In suggesting that perception may operate at the level of single-energy arrays, Amazeen & Van Orden, **Radeau & Colin, Sanders,** and **Warwick-Evans** do not address this problem (which was also not addressed by several of the original commentators; e.g., McMichael & Bingham 2001; Michaels & Oudejans 2001; Peper & Beek 2001; Rosenblum & Gordon 2001; Runeson et al. 2001; see sect. R8 of our earlier response). For example, Sanders claims that there is "no reason to argue that the world never presents itself to animals via singular sensory modalities." We argued at some length (sects. 3, 4, 5, R8) that unambiguous information about the animal-environment interaction (i.e., specification) cannot exist in the structure of any individual form of ambient energy. Based on this, our rejoinder to the suggestions of Sanders and others is that there is no basis in the physical world for an argument that reality can be specified in any single-energy array. As two examples, consider the following. The advance of visual technology (e.g., spatiotemporal resolution), of simulation software (e.g., computer models of optical dynamics), and of motion tracking systems (which make it possible to couple a perceiver's movements to a visual simulation in real time) suggest that in the near future we will have reached a point where any pattern that can arise in the optic array as a result of "real"

***2nd col.* Stoffregen Author Response p. 4 Comment:** CCE/AU: These names have been added to the ref. list. Entries need to be completed.

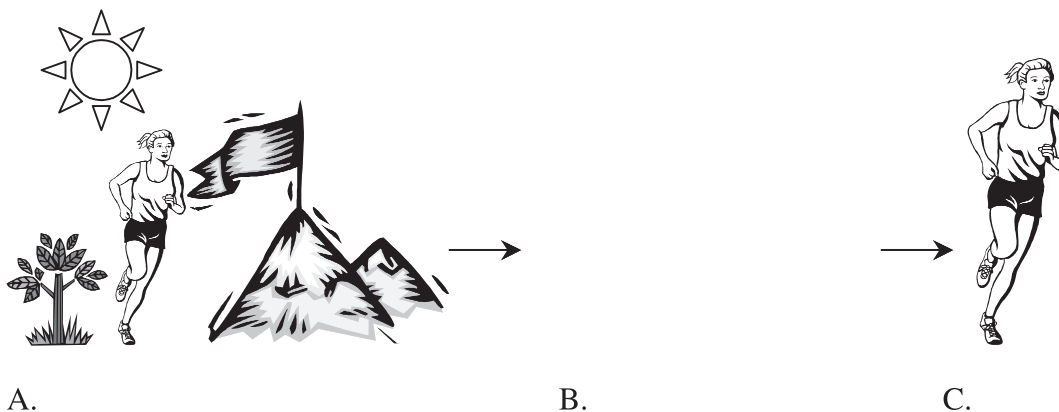

Figure R1. **A.** Animal-environment interaction (reality). **B.** Ambient energy fields (the global array). **C.** Sensory stimulation (activity at receptor surfaces). The animal-environment interaction structures ambient energy fields. Specification concerns only relations between reality and ambient energy fields. Perception depends upon sensitivity to properties of ambient energy fields (e.g., the nature of the energy, or the structure that it contains). The same person appears in A and C, indicating that actors are also perceivers, and that actions influence the information available to the actor's perceptual systems.

movement can independently be created as a result of simulated movement. In other words, all possible patterns in the optic array will correspond to at least two states of the physical world: an object or event in the physical world, an object or event in a simulated world. Specificity requires that each pattern in an ambient array arise from and correspond to one and only one aspect of the animal-environment interaction. Visual technology will cause that requirement to be violated in the case of any and all patterns in the optic array and, accordingly, no pattern in the optic array can bear a unique or specificational relation to any object or event in the world (Stoffregen et al. 2003). The same is true of patterns in the acoustic array. Technology for acoustic simulation and reproduction is rapidly approaching the point at which any given pattern in the acoustic array can be created by at least two aspects of the animal-environment interaction: a "real" sound-producing event and a "simulated" sound-producing event. Thus, 1:1 correspondence between acoustic patterns and states of the world is violated and, accordingly, the acoustic array does not specify properties of the animal-environment interaction.

Another point concerns relations between different single-energy arrays. Like many of their predecessors (e.g., McMichael & Bingham 2001; Michaels & Oudejans 2001; Peper & Beek 2001; Rosenblum & Gordon 2001; Runeson et al. 2001; Thomas 2001), in arguing for the existence of specification within individual forms of ambient energy **Radeau & Colin, Sanders,** and **Warwick-Evans** do not address the fact of the constant, simultaneous stimulation of multiple perceptual systems (sects. 1, R8). Multimodal stimulation is ubiquitous and is known to be detected and used in the control of action, including movements of the body (e.g., Berthoz 1997), of the head and limbs (e.g., Peper et al. 1994), and so on. As a result, a pattern within a single type of ambient energy is never the sole stimulus, and is never the sole information that is picked up. If specification did exist in single energy arrays (a point that we continue to dispute), this would still leave open the issue of how the animal deals with the fact of constant, simultaneous stimulation of multiple perceptual systems. We argued that this could be accommodated in an ecological theory only within the concept of the global array (i.e., cooperative pickup of global structures). Warwick-Evans and other defenders of specification in single-energy arrays do not address this issue. Warwick-Evans notes that intersensory conflict is possible within individual perceptual systems. We accept this assertion, but it is beside the point. Intersensory conflict would exist only if animals attempted to perceive on the basis of patterns available in single-energy arrays. If specification exists in the global array, and if perceivers are sensitive to the global array, then conflict will not exist within or between perceptual systems.

R3. Selective pickup of information in the global array

The existence of information in higher-order patterns within the global array seems to have suggested to many readers that if perception is based on the global array, it must be omnivorous – that is, perceivers are obliged to pick up all available information. Such obligatory, omnivorous pickup would be a problem, and would fly in the face of the obvious facts that perceptual systems are not sensitive to all dimensions of the global array and that perception is selective. However, the existence of information in the global array does not imply any obligation for perceivers to pick up all available information. Perception is selective, regardless of the nature or locus of perceptual information (Gibson & Pick 2000).

Ohlmann et al. argue that different animals should use the global array in different ways. We agree; we made just this point in responding to the original commentaries (sect. R11). In a given situation, different species or individuals will sample different portions of the global array, and a given species or individual will sample different portions of the global array in different situations, or when performing different tasks. However, the fact that perception is selective does not imply that perceivers will selectively sample structure in individual forms of ambient energy (in effect, Ohlmann et al. propose that people often ignore the global array). We agree that "subsets of reality" are sufficient for most behaviors and situations. Selective perception of situation-specific aspects of reality is commonplace. Selective perception implies that ambient information is sampled selectively and, therefore, in our account, the global array will be sampled selectively. But this does not imply that perceivers would choose to sample single forms of ambient energy. Sampling of structure in single forms of ambient energy would be useful only if a situation-specific subset of reality lawfully influenced the structure of only one form of ambient energy and did not influence the structure of the global array. Ohlmann et al. offer no argument as to how this could happen, or when it might. We have argued that anything that influences the structure of any single-energy array must also influence the structure of the global array (sect. 6). The heart of our critique of the concept of unimodal specification was our claim that, as a matter of physics, patterns in any single-energy array are intractably ambiguous with respect to physical motion (sects. 4 and 5). This leads to the conclusion that specification does not and cannot exist in single-energy arrays. Moreover, we noted that all objects and events that structure single-energy arrays also structure the global array. This leads us to argue that the information available in the global array is sufficient (in principal) for all perception.

Ohlmann et al. appear to assume that the fact of selective pickup implies selection on the basis of forms of ambient energy. This assumption suggests that they may have confused reality (what is perceived) with information (what enables perception). Reality is not categorized in terms of ambient energy. In daily life, people do not talk about "optical reality," "chemical reality," "gravito-inertial reality," and so forth. Rather, we classify reality into functional categories: food (eating), shelter (sleeping), friends (socializing), mates (reproducing), children (parenting), and so on. To assume that perception is categorized by forms of ambient energy appears to imply that we do not have direct sensitivity to the underlying reality; that is, it appears to imply that we have perceptual sensitivity only to ambient energy and not to the world that structures ambient energy. The ecological approach motivates the hypothesis that perceivers are directly sensitive to reality. If this is so, then there is little reason for perceivers to organize their perceptual-motor exploration in terms of individual forms of ambient energy. Indeed, perceivers should be strongly motivated to avoid limiting their pickup to single-energy arrays since (as we argued in sections 4 and 5) the patterns in single-energy arrays are fundamentally ambiguous with re-

spect to physical motion, and therefore are very unlikely to prove useful for the perception or control of the animal-environment interaction.

Ohlmann et al. argue that, in well-practiced conditions, single-energy arrays may be sufficient for perception and control of action, and that in such situations animals are not "obliged" to "work at a higher level." For us, the logic of their argument is backwards. If specificity exists in the global array and does not exist in single-energy arrays, then animals would always be motivated to use the information in the global array because it is these patterns that provide reliable information about the animal-environment interaction. If, as we claim, single-energy arrays are not specific to reality, then why would perceivers ever be motivated to resort to them? Ohlmann et al. appear to assume that, given the choice, perceivers would prefer to use patterns in single-energy arrays, that single-energy patterns would be more familiar, or easier to use, and so on. We see these assumptions as being testable, and we have predicted that they will be disconfirmed (sect. 7). Testing of these assumptions will require new research that is designed to contrast patterns in single-energy arrays and in the global array.

Ohlmann et al. reiterate the argument of earlier commentators (Cabe 2001; Brenner & Smets 2001) that perception need not always be accurate. In response, we reiterate our argument that the accuracy of perception is independent of the issue of specification (sects. 7, R6). That is, specification makes it possible for perception to be accurate but does not mandate or guarantee accuracy. Perception may be inaccurate despite the existence of specification if, for example, the perceiver picks up the wrong information, as in the case of the train illusion, in which people perceive themselves to be in motion (relative to the earth) when in fact it is an adjacent vehicle that has moved relative to the earth. This is a clear case of perceptual error, but it does not imply that ambient arrays bear an ambiguous relation to reality.

Ohlmann et al. refer to redundancies in potential sensory stimulation, but make no response to our argument (sect. 3.1.3) that such redundancies are rare in ordinary life. Relative to one another, single-energy arrays are non-redundant far more often than they are redundant. Also, the argument from redundancy again assumes that the level of single-energy arrays is somehow the main or primary level of analysis for perception. Sometimes there are redundancies between single-energy arrays, but this does not imply that perception does or should operate at this level of analysis.

R4. Physical referents for perception and action

Relative motion in the physical world is at the center of our argument against the existence of specification in single-energy arrays (sects. 4, 5). For this reason, we were surprised that motion relative to different physical referents was not addressed by earlier commentators (e. g., Michaels & Oudejans 2001; Peper & Beek 2001; Rosenblum & Gordon 2001; Runeson et al. 2001) who argued that specification exists in single-energy arrays. Developing a response to our arguments about relative motion is a serious challenge for scientists who argue that specification exists in single-energy arrays (sect. R7 of earlier response).

Pederson & Sigmundsson discuss subjective reports relating to self-motion. They appear to argue that in the moving room paradigm, imposed optic flow is ambiguous with respect to physical motion of the observer. We agree; we made exactly this point in sections 5.1 and 5.2 of the target article: The optic flow could be produced by motion of the room relative to an otherwise stationary observer, or by motion of the observer relative to an otherwise stationary room, or by different motions of the two. However, we did not claim that subjective reports of self-motion in moving rooms are fundamentally ambiguous. Rather, we pointed out the fact of relativistic physics that subjects in a moving room are actually in motion relative to the room, while simultaneously being actually in stasis relative to the earth.[1] There is no ambiguity in the physics; it is simply the case that subjects are in motion relative to some physical referents, while being in stasis relative to others. It is because of this that subjects can truthfully (accurately) report either that they are moving or that they are stationary, or both. The subjective reports are ambiguous only due to weakness in experimental design arising from the fact that experimenters, in requesting subjective reports, have neither (1) specified a physical referent for perception of motion or stasis, nor (2) asked subjects to indicate the referent that they are using.

We discussed perceptual reports in the moving room paradigm (sect. 5.2) as an example of the conceptual ambiguity that underlies many existing datasets. The main point of our example was that in most previous research there have been ambiguities (in design, in interpretation, or both) involving the physical referents for the perception of motion or the control of motion, or both. We agree with **Amazeen & Van Orden** that there may not be complete overlap between the information that influences consciousness and the information that is used to guide action, but this is tangential to the purpose of our example.

Warwick-Evans claims that we endorsed a position in one place and rejected the same position elsewhere. He notes, correctly, our rejection of sensory reference frames (sect. 3.1.2), but claims that in section 5.1 we endorse sensory reference frames. His latter characterization is incorrect. In section 5.1, we pointed out that individual forms of energy are structured by position and motion relative to limited classes of physical referents. Warwick-Evans appears to have confused sensory reference frames, in which the referent is the receptor, with reference frames in which the referent is something other than the receptor (e.g., rocks, moving rooms, planets, other people). In section 5.1 our discussion concerned referents that structure potential sensory stimulation, whereas in section 3.1.2 our discussion concerned referents (i.e., receptor surfaces) that structure actual sensory stimulation. There is, however, one point of similarity between our analyses of sensory reference frames (sect. 3.1.2) and of single-energy arrays (sect. 5.1). We argued that specification does not exist and cannot exist in sensory reference frames (i.e., patterns of actual sensory stimulation arising from a single form of ambient energy) or in single-energy ambient arrays.

Many of the commentaries, both previous and present, do not distinguish clearly between physical referents, ambient energy, and actual sensory stimulation (see Figure R1). Physical referents (e.g., rocks and trees) exist whether or not there are any ambient energy fields (e.g., they exist whether or not the environment is illuminated). Ambient energy fields (light, sound, and so on) exist whether or not there are perceiving animals or receptor systems, and whether or not animals search for, pick up, detect, or make

use of the information that such fields may contain. Actual sensory stimulation depends upon the prior existence of physical referents and ambient energy fields. Physical referents, ambient energy fields, and actual sensory stimulation differ qualitatively. In principle, action might be perceived and controlled relative to any of these. Control of action relative to actual sensory stimulation (i.e., sensory reference frames) would be problematic because sensory reference frames bear an ambiguous relation to the animal's interaction with the physical environment. Control of action relative to referents in ambient energy fields (e.g., angular position in the optic array) would be problematic because in most cases the entities of interest are in the animal-environment interaction, not in energy fields. Control of action relative to physical referents, if possible, would be best because most of our behavioral goals are defined in terms of physical referents (e.g., we seek to eat food, rather than to eat patterns of light that reflect from food, or rather than to eat patterns of retinal activity that arise from light reflected from food). If specification exists, then perception and control action relative to physical referents is possible (sect. 6).

R5. Perceptual-motor development

Radeau & Colin and **Sanders** endorse the idea that in early life perception is "rich in cross-modal sensory integration." **Sanders** also observes that in evolutionary history the emergence of receptor systems that are keyed to individual forms of ambient energy is relatively recent. Empirical research has documented the multisensory sensitivity of even single-celled creatures (e.g., Berg 2000; Pittenger & Dent 1988; see also Stein & Meredith 1993).

Radeau & Colin suggest that early sensitivity to the global array is followed by development of sensitivity to structures in single-energy arrays. In making this suggestion they offer no reason why a perceiver who was sensitive to the global array would want to acquire sensitivity to anything else. As we noted in the target article (sect. 6) and in our response to the original commentaries (sect. R8), if specification exists only in the global array, and if perceivers are sensitive to information in the global array, then sensitivity to patterns in single-energy arrays would be of no benefit. Studies that purport to document a gradual emergence of sensitivity to structures in single-energy arrays can be reinterpreted. Rather than learning to differentiate among different types of ambient energy, it is possible that neonates learn to differentiate among different external referents for the control of action. If perception is direct, then we do not perceive sensory stimulation or forms of ambient energy; rather, we perceive the animal-environment interaction, as such. Thus, learning would be "learning about the animal-environment interaction" rather than "learning about visual stimulation, auditory stimulation, and so on" (Gibson & Pick 2000). This possibility can be evaluated only if researchers decline to *assume* that perception consists exclusively or primarily of sensitivity to sensory stimulation, as such.

R6. Visual capture and neurophysiology

Radeau & Colin discuss the ventriloquism effect, also known as *visual capture.* Many events that influence the structure of the acoustic array also influence the structure of the optic array. Examples are speech and dribbling a basketball. In experimental manipulations, it is possible to separate the acoustic and optical consequences of events, in both space and time. Visual capture refers to the fact that observers often experience audible events as originating from the spatial location of visible events, even when they do not. Radeau & Colin review some of the typical characteristics of visual capture, focusing on the fact that visual capture is associated with aftereffects on pointing. Exposure to spatially separate visible and audible events leads to systematic direction errors in subsequent attempts to point to an audible event (while blindfolded).

Two characteristics of the visual capture paradigm are of particular relevance in the present context. The first concerns the response measures that are used. There is a disjunction between the subjective experience of visual capture (i.e., the experience that visible and audible events have the same location), and aftereffects of the spatial disjunction on pointing. Radeau and Bertelson (1977) created experimental separation of visible and audible event location. In separate experiments, subjects were asked to point to the location of the audible event or to press a key to report the subjective experience that audible and visible events had the same location. The experimental manipulations had different effects on pointing and key pressing. The disjunction between subjective experience and perception-action has been found in a wide variety of situations (e.g., Bootsma 1989; Fouque et al. 1999). This disjunction does not, by itself, implicate any theory of perception.

The second relevant aspect of the visual capture paradigm relates to perceptual-motor exploration. In research on visual capture, subjects typically are seated with the head restrained by a bite bar (e.g., Radeau & Bertelson 1977), or by the requirement to keep the head in contact with a stationary device (e.g., Radeau & Bertelson 1974). These restrictions on head movement severely limit normal patterns of both visual and auditory exploration. Head movements are known to be essential for ordinary sound localization (e.g., Gibson 1966; Kellogg 1962; Rice 1967; Wallach 1940). The inability to localize audible events (as occurs with visual capture) may arise from artificial limitations on perceptual-motor exploration that characterize the experimental design, as appears to be true for a variety of laboratory illusions, such as the Ames room (Runeson 1988). Interestingly, in research on visual capture, head movements have been permitted only when the dependent variables are limited to reports of subjective experience, with no perception-action component (experiment 3, Radeau & Bertelson 1977).

Studies of nonhumans (e.g., Knudsen & Knudsen 1989) are of doubtful relevance to the ventriloquism effect. Such studies provide no data about the subjective localization of visible and audible events. For this reason, even though such studies can confirm that humans and nonhumans have similar perception-action adaptation to prisms, their relevance to the ventriloquism effect can be only conjectural.

Radeau & Colin also review some of the research on multisensory neurophysiology. The existence of multisensory neurons is one of the most exciting discoveries in recent neurophysiological research. The foundational work of Stein and Meredith (1993) has given rise to a new field of research, and to the discovery that multisensory neurons can be found at several levels of the central nervous system (e.g. Wallace et al. 1992; 2004). We regard research on mul-

tisensory neurophysiology as being of great potential relevance to our hypothesis that perceivers are sensitive to information in the global array. However, we note that the existence of multisensory neurons does not, by itself, implicate any theory of perception (cf. sect. R1 of the earlier response). Researchers often interpret such neurons as sites of multisensory "integration" or "processing." These interpretations typically reflect the assumption that the nervous system acts to alter or enrich incoming perceptual information. The hypothesis that multisensory information is integrated by the nervous system arises out of the traditional assumption that perception consists of a set of channels that have access to qualitatively different stimuli (i.e., different forms of ambient energy). By contrast, if information exists in higher-order patterns that extend across forms of ambient energy (i.e., if information exists in the global array), and if perceivers are sensitive to global array parameters, then there would no need for multisensory "integration"; the integration exists outside the head and does not need to be created inside the head. Rather than combining or integrating stimuli from different perceptual systems, multisensory neurons may act to detect higher-order properties of multisensory stimulation, as we argued in section 6.2.4 (see also Stoffregen & Bardy, in preparation). The ability of neurons to respond to higher-order properties of stimulation has been documented in the context of the optic array (Saito et al. 1986; Tanaka & Saito 1989). We predict that it will be possible to document neural sensitivity to higher-order patterns in the global array. To evaluate our prediction, it will be necessary, first, to identify parameters of the global array that can be manipulated experimentally and presented in the context of neural preparations. For two reasons, existing research on multisensory neurons does not meet this criterion. First, researchers have assumed that the only perceptual information present was in single-energy arrays; that is, they have not taken into account the fact that events that alter the structure of individual forms of ambient energy also (necessarily) alter the structure of superordinate, emergent patterns in the global array. Second, any attempt to identify or experimentally control patterns in the global array has been complicated by the creation of arbitrary pairings between events (e.g., a tone paired with a flash of light; Meredith & Stein 1986). Given the conceptual and methodological characteristics of these studies, the identification of neurons that may respond to patterns in the global array is all the more remarkable.

R7. Perception and action without deception

In interpreting perception in the moving room paradigm, **Amazeen & Van Orden** suggest that individual perceptual systems might yield independent percepts of motion relative to different physical referents, and that these percepts might interact only in the organization of responses. This could happen, they claim, "if the observer is misled to believe that the floor and walls must move together." Deception is not required for people to exhibit postural responses to a moving room (i.e., to stabilize the head and body relative to the room as the latter moves relative to the floor), or for the subjective experience of self-motion (Lishman & Lee 1973). In our research, we have never attempted to deceive subjects: On the contrary, we have explicitly informed subjects that the room moves relative to the floor (e.g., Oullier et al. 2002; 2004; Stoffregen 1985: 1986; Stoffregen & Smart 1998; Stoffregen et al. 2004). Other researchers also have not attempted to deceive subjects (e.g., Lee & Lishman 1975; Lishman & Lee 1973).

R8. Need for new research

Several of the commentators offer alternative interpretations of some of the studies that we discussed in the target article (e.g., **Amazeen & Van Orden, Pederson & Sigmundsson,** and **Radeau & Colin**) or argue that perceptual sensitivity to the global array is not supported by existing research (e.g., **Warwick-Evan**s). They point out (correctly) that our interpretation of existing data in terms of the global array is not the only possible interpretation, and they argue that the data also can be interpreted in other terms, such as percept-percept coupling or response bias. We agree that existing data are amenable to multiple interpretations. It is for this reason that we argued in the target article (sect. 7) and in our reply to the original commentaries (sect. R12) that only new research can be used to evaluate (i.e., falsify) the hypothesis that direct perception is based on specification in the global array. Empirical evaluation of the hypothesis that perceivers detect information in the global array will require tests in which patterns in single-energy arrays and in the global array are manipulated independently, under experimental control. Such manipulations are possible (we gave a detailed example in sect. R12). They can be done most rigorously and most easily if researchers begin by identifying analytically patterns in the global array that are influenced by particular aspects of the animal-environment interaction. This places a premium on formal, analytical modeling of patterns in the global array. That such analysis is possible is shown by parameters of the global array that have already been identified (e.g., Bingham & Stassen 1994; Peper et al. 1994; Stoffregen & Riccio 1988, Eqns. 4–6).

Our analysis implies the detection of information in the global array. As suggested above, it has implications for how information is used in movement control – that is, for *control laws.* In the ecological approach to perception and action, control laws describe how animals can use some parameter of an ambient array to regulate a free parameter of the action system. Animals are assumed to select and exploit particular control laws to achieve particular behavioral goals. The concept was originally suggested by James Gibson (1958) and has recently undergone rapid development in the context of locomotion and navigation (Warren 1988; Fajen & Warren 2003) and manual interception (e.g., Montagne et al. 1999). For example, when lateral movements of the hand are used to intercept a ball in flight, Montagne et al. suggested that the lateral velocity of the hand is continuously coupled onto the optically specified velocity of the approaching ball. Control laws are important because they permit us to formalize the perceptual information that animals use to perceive and regulate behavior, on a task-specific basis. Ultimately, a successful theory of perception and action should be able to specify control laws for any given aspect of the animal-environment interaction. To date, however, efforts to formalize control laws have been conducted primarily in the context of single-energy arrays (e.g., Montagne et al. 1999; Warren 1988). We regard this effort as problematic because,

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as we have argued, parameters of single-energy arrays can never bear a specificational relation to the animal-environment interaction. If we are right, then laws of control cannot be formulated in terms of single-energy arrays.

There is no a priori reason why control laws should be limited to single-energy arrays. Regulation of action may be achieved by coupling parameters of the action system to patterns in the global array. Given that, by our analysis, specification exists solely in the global array, it follows that lawful relations between perception and action must refer solely to parameters in the global array. We argue that parameters of the action system can be appropriately tuned to properties of the global array, instead of properties of single dimensions of the global array (i.e., optics, acoustics). The existence of control laws that refer to the global array has been demonstrated in at least one instance. Equation 2 in the target article (sect. 5.3) constitutes a control law that is based on the global array. Peper et al. (1994) considered manual interception of moving objects. They developed a formal model in which the current velocity of the hand is continuously regulated based on a property of the global array. In section R12, we provided a detailed description of how perceptual reality of this control law might be tested. We suggested coordinated manipulation of optics (by deflating the ball during the approach) and haptics (by changing the eigenvectors of the hand's inertia tensor during the approach) that would be appropriate to contrast optical control laws and control laws based on the global array.

In the target article, we emphasized the need for analytical research that can formalize the existence of specification in the global array, and the need for new research that can test the hypothesis that perceivers are sensitive to information in the global array. We now note the need for two additional lines of effort: New analytical research is needed to derive control laws in which action can be regulated by parameters of the global array, and new empirical research is needed to determine whether action actually is regulated on the basis of these control laws.

R9. Concluding remarks

We are pleased that several of the new commentators (e.g., **Amazeen & Van Orden, Sanders**) appear to agree that our overall theoretical position has sufficient plausibility to merit further study. The utility of information in the global array arises out of relativistic physics, out of the facts that (1) perception and action must be controlled with respect to multiple, simultaneous physical referents, and (2) physical referents can move independent of one another, and animals can be in different types of motion relative to different referents. It is these relativistic physics that render single-energy arrays ambiguous with respect to reality (and, we argue, insufficient for perception and control of action). Yet these relativistic physics also give rise to specification in the global array. As we noted in section 6 of the target article, the global array is a fact, not a hypothesis. Higher-order relations between different forms of ambient energy exist. Research needs to be directed toward new questions that arise from the existence of the global array: Are patterns in the global array specific to reality, and, if so, do animals use information in the global array for the perception and control of action?

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NOTE

1. This description leaves out body sway; if we take sway into account, then the correct description would be that subjects were in different motions relative to the room and the earth. This is accurate but less helpful because subjects sometimes report stasis, which most likely means that they do not take into account their own body sway in formulating or expressing their subjective experience.

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[The letter "r" before author's initials stands for Authors' CC Response references]

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Commentary on **David L. Hull, Rodney E. Langman & Sigrid S. Glenn (2001). A general account of selection: Biology, immunology, and behavior. BBS 24(3):511–528.**

Abstract of the original article: Authors frequently refer to gene-based selection in biological evolution, the reaction of the immune system to antigens and operant learning as exemplifying selection processes in the same sense of this term. However, as obvious as this claim may seem on the surface, setting out an account of "selection" that is general enough to incorporate all three of these processes without becoming so general as to be vacuous is far from easy. In this target article, we set out such a general account of selection to see how well it accommodates these very different sorts of selection. The three fundamental elements of this account are replication, variation and environmental interaction. For selection to occur, these three processes must be related in a very specific way. In particular, replication must alternate with environmental interaction so that any changes that occur in replication are passed on differentially because of environmental interaction.

One of the main differences among the three sorts of selection that we investigate concerns the role of organisms. In traditional biological evolution, organisms play a central role with respect to environmental interaction. Although environmental interaction can occur at other levels of the organizational hierarchy, organisms are the primary focus of environmental interaction. In the functioning of the immune system, organisms function as containers. The interactions that result in selection of antibodies during a lifetime are between entities (antibodies and antigens) contained within the organism. Resulting changes in the immune system of one organism are not passed on to later organisms. Nor are changes in operant behavior resulting from behavioral selection passed on to later organisms. But operant behavior is not contained in the organism because most of the interactions that lead to differential replication include parts of the world outside the organism. Changes in the organism's nervous system are the effects of those interactions. The role of genes also varies in these three systems. Biological evolution is gene-based (i.e., genes are the primary replicators). Genes play very different roles in operant behavior and the immune system. However, in all three systems, iteration is central. All three selection processes are also incredibly wasteful and inefficient. They can generate complexity and novelty primarily because they are so wasteful and inefficient.

GAS doesn't "turn the engine" when states are sequential or context-dependent

Liane Gabora

Department of Psychology, 3210 Tolman Hall, University of California, Berkeley, CA, 94720–1650 and Center Leo Apostel for Interdisciplinary Studies (CLEA), Free University of Brussels, Belgium.
http://www.vub.ac.be/CLEA/liane/ liane@berkeley.edu

Abstract: Selection theory requires *multiple, simultaneously-actualized* states. In cognition, each thought changes the "selection pressure" against which the next is evaluated; they are not simultaneously selected amongst. Cognitive change occurs not through selection among discrete "neural configurations," but through interaction between conceptual web and *context.* This introduces a non-Kolmogorovian probability distribution, hence a classical formalism (e.g., selection theory) cannot be used.

Hull et. al. (2001) wait until the last sentence of their response to commentators to invite us to refer to their General Account of Selection as GAS. I will do so, although – insightful and provocative though it is – I am not completely convinced it is the highest octane stuff around.

One problem with their attempt to unite biology, immunology, and behavior under the same umbrella is, as many commentators pointed out, the failure to identify what constitutes a "replicator" in operating learning. Since this is something I have addressed in detail elsewhere (Gabora, 2004), here I address another problem, the point made by Pepper & Knudsen (2001) that the mathematical formulation of selection theory requires that at any given iteration there be "multiple concurrent replicators that differ in their replication rate" (p. 550). Okasha (2001) makes the same point (p. 549):

> The authors note a significant disanalogy between operant learning and standard examples of Darwinian selection. In the latter, the variants on which selection operates are present at the same time, while in the former the variants form a temporal sequence, each one existing for a moment, before being replaced by another. The authors do not think this disqualifies operant learning from qualifying as a Darwinian process, but I am less sure.

Okasha then reasons that in attempting to apply selection theory to such a temporal sequence, whether or not something gets selected depends arbitrarily on how you break up the sequence. Hull et al. respond that things are not so tidy in biology either, and asked (p. 566) "what might 'the same time' mean in the case of organic evolution?"

There is a fairly straightforward answer to this question, which is intimately tied to their concept of iteration. In biology, there exist multiple entities at any one time and, although the selective pressure undergone by them will never be *identical,* they all at least constitute part of the *same iteration.* So by "same time" we mean there are multiple entities undergoing the selective pressure of a given iteration. However, this is not the situation where a sequence of thoughts is concerned. Because each instant of thought contributes to the context in which the next is evaluated, never do two thoughts or cognitive states undergo the same "selective pressure"; they are not being "selected amongst." Pepper & Knudsen (2001) suggest that, despite their reservations, the enterprise can be salvaged as follows (p. 551):

> If replicators consist of specific neural configurations that produce tendencies or proclivities for certain behaviors, it is not hard to imagine a population of such replicators that compete for the opportunity to be expressed as behaviors (interactors) and to be therefore strengthened or weakened according to their relative "success" (e.g., in eliciting positive affect). It is also not hard to envision that stronger neural configurations would be more likely to persist and to spawn variants.

However, the distributed, overlapping, contextually-elicited nature of memories and concepts prohibits there being multiple, distinct neural configurations from among which to choose. The conceptual network is a weblike structure; on what basis can one say it divides itself up into different "neural configurations" and selects one? This would require enough space to store, prefabricated in advance, every thought or idea one could possible have! Surely the mind exists in a state of *potentiality,* and the situation or circumstance provides the *context* that actualizes a particular thought or cognitive state. Why is this a crucial issue as far as GAS is concerned? Because if there are multiple *preexisting,* fully actualized states from which some are *selected,* then this is a deterministic process that can be described by a classical probability theory such as selection theory. However, it has been proved that in situations where a state of potentiality can only be resolved through interaction with a context, the change of state is nondeterministic, and this introduces a non-Kolmogorovian probability

References for Hull CC page 1 Comment: Query Gabora: Check date for Aerts & Aerts, comm. Text gives 1996 as the date.

model on the state space; a classical probability model cannot be used (Accardi & Fedullo 1982; Aerts 1986; Aerts & Aerts, 1997 Piron 1976; Pitowski 1989; Randall & Foulis 1976).

Because it is not the different possible pre-specified neural configurations that are being selected amongst, but the different possible *contexts* that the entire conceptual network could encounter, each of which would give rise to a different interaction dynamics, to describe the relevant change of state requires a nonclassical probability model (Gabora & Aerts 2002; in press). Selection theory, a classical formalism, is inappropriate for the description of cognitive change, and because operant behavior rests on cognitive processes, it is not able to provide a complete description of that either. But this need not dash all hopes of a unifying theory of evolution. There is no a priori reason such a theory has to be Darwinian or even involve selection except as a special case. In one such approach, evolution is viewed as the context-driven actualization of potential or CAP (Gabora & Aerts 2005a, b). Different forms of evolution vary with respect to the degree of indeterminism due to context, the degree of context independence, and the degree to which context-driven change is retained in future lineage(s). The approach gives us a glimpse of what a theory of change that applies across the social, psychological, biological, and physical sciences might look like (though only time will tell if CAP gets us further down the road than GAS). It should be noted that physicists use the word "evolution" without implying that any sort of selection is involved. As I see it, it is only because Darwinian evolution received so much attention as such an *unusual* form of evolution that it eventually cornered the word "evolution."

Finally, although my critique applies to the section on operant behavior, I think it is this section that generated the most fruitful discussion, and it is this section that may have the greatest impact on future "lineages of thought." One doesn't go too far out on a limb proposing that selection is important in biology. Given Hull's previous writings, it is a bit surprising that the authors of the target article didn't just take the biological account of these phenomena as more or less right as far as biology is concerned and have Hull focus on culture. In fact, given the BBS readership, it might have been more useful to replace the immunology section with a section on culture. But this is all after the fact, and I applaud Hull et al. for bringing these important issues onto the table. GAS may not replicate, but it certainly provides fuel for thought.

ACKNOWLEDGMENTS
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Authors' Response[1]

Multiply concurrent replication

David L. Hull[a] and Sigrid S. Glenn[b]

[a]*Department of Philosophy, Northwestern University, IL 60208;* [b]*Department of Behavior Analysis, University of North Texas, Denton, TX 76203.*
d-hull@northwestern.edu glenn@scs.unt.edu

Abstract: If selection is interpreted as involving repeated cycles of replication, variation, and environmental interaction so structured that environmental interaction causes replication to be differential, then selection in gene-based biological evolution and the reaction of the immune system to antigens are relatively unproblematic examples of selection processes. Operant learning and cultural evolution pose more serious problems. In this response we deal with operant learning as a selection process.

As the title of our target article (Hull et al. 2001) indicates, we tried to set out a General Account of Selection (GAS) for three sorts of phenomena: gene-based selection in biological evolution, the reaction of the immune system to antigens, and operant learning. **Gabora** is more interested in a fourth sort of phenomenon: social learning. Most of her paper concerns thoughts, cognitive states, memories, concepts, culture, and the like. In this response we do not discuss social learning as a selection process because we did not treat it in our original BBS target article. However, as Gabora notes, one of us has in the past discussed sociocultural evolution as a selection process (Hull 1988) and another has used GAS to explore the role of operant learning in the emergence of cultural interactors (Glenn 2003). Quite obviously we would like to see GAS extended to selection in sociocultural evolution, but unfortunately any attempt to deal with selection in sociocultural evolution in this response would require too much space and depart too radically from our target article. Instead, we will deal with Gabora's objections to our treating operant learning as a selection process.

As we mentioned in our target article, theories of operant learning come in a variety of forms. We chose to investigate what we thought was one of the best versions of this theory. We did not claim that operant learning involves nothing but selection, only that selection processes play a role in learning, an important role. Nor are we claiming that all behavioral phenomena involve operant learning. The issue is not how adequate or comprehensive operant theory is but whether it incorporates a selection process. B. F. Skinner, the father of operant learning, thought so, but he might well have been mistaken.

The most fundamental and pervasive problem with respect to our general account of selection stems from the fact that it is an instance of conceptual analysis. Some of the earlier commentaries on our target article wanted our analysis to be more stringent; others wanted it to be less so. Unfortunately, no cut-and-dried methods exist for making such choices. One begins with what one hopes are the most clear-cut cases and proceeds from there. For a long time, gene-based biological evolution was the main example of a selection process and it was treated as such. Any departures from selection in gene-based biological evolution counted against any additional examples. Too often, however, peculiarities of this paradigm example were treated as if they were essential. Our increased knowledge of the immune system has helped to rectify that bias. The reaction of the immune system to antigens is as good an example of selection as is traditional gene-based selection in biology. We now have two paradigm examples of selection.

Gabora's main objection to operant learning as a selection process is that it does not include anything that might be treated as a replicator; and even if it did, it does not allow for multiple concurrent replicators that differ in their replication rates. In this response, we deal with four questions: (a) Does selection require replicators? (b) If so, can replicators be discerned in operant learning? (c) Can these replicators be construed as multiply concurrent? and, finally, (d) Must they be?

In our target article, we characterized selection as consisting of "repeated cycles of replication, variation, and environmental interaction so structured that environmental interaction causes replication to be differential" (p. 513; italics deleted). We then discussed each of these entities

and processes in greater detail. **Gabora** does not object to our analysis of variation or environmental interaction. She restricts her criticisms to replication. Therefore, the issue turns from analyzing selection in general to analyzing replication in particular.

We defined replication in terms of information, copying, and descent. From the outset we admitted how problematic information is. None of the current analyses of information seemed adequate for our purposes. If an analysis of "information" can be produced which turns out to do what we need it to do, then that counts *for* GAS. If no such analysis of information is forthcoming, that counts against it. We also committed ourselves to copying and descent. In gene-based biological evolution and the reaction of the immune system to antigens, copying does occur and variations in the copying process get passed along via descent. Some other mechanisms may turn up that can accomplish these same ends, but so far all we have to go on are possible counterexamples and they are not persuasive enough to cause us to modify our analysis.

Since **Gabora**'s main objection to GAS concerns operant learning, a few words need to be said about this process. The recurrence of operant responses in lineages over time is not just a theoretical possibility but a reliably observed phenomenon. Further, the role of reinforcement in maintaining the recurrence of those responses and accounting for change over time in their characteristics is abundantly clear. These facts alone allow the term "selection" to be applied *metaphorically* to the operant case. However, in our target article we had a much broader aim in mind – to produce a General Analysis of Selection that applies equally to all three sorts of selection. As Pepper and Knudsen (2001) and Okasha (2001) noted in their commentaries, the locus of replication in operant learning is the nervous system.

Because empirical work on the neural activity involved in overt operant responding is scarce and not typically conceptualized in terms of selection processes, our account is necessarily more speculative than we would like. It rests on reasoning as follows. Because the responses (interactors) constituting any particular lineage of responses are only intermittently instantiated in a learner's behavior stream, the effects of previous fits between interactors of that lineage and successive selecting environments must be presented materially in the learner's nervous system, where they remain available for instantiation given the appropriate circumstances. "When an organism exposed to a set of contingencies of reinforcement is modified by them and as a result behaves in a different way in the future . . . what is 'stored' is a modified organism" (Skinner 1969). The observed result of the contingencies of reinforcement is the replication-with-a-minor-variation of responses that previously resulted in reinforcers.

The question at hand, then, appears to be this: Does that which is preserved ("stored") function as a replicator? Both sexual and asexual organisms evolve in ways that incorporate selection. So do single-celled and multi-celled organisms. However, for the sake of simplicity, biologists frequently discuss selection as it functions in lineages of single-celled organisms that reproduce asexually. The results are then expanded to include more complex cases. We will do the same in our discussion of operant learning. As Hull (1988) has pointed out, one advantage of such generic terminology as "replicators," "interactors," and "lineages" is that one and the same entity can perform more than one function. For example, in narrowly biological contexts, genes can function as both replicators and as interactors. In our target article on GAS, examples of operant behavior were limited to lineages of single-component responses that were comparable to single-celled organisms in asexual species (i.e., key pecks and lever presses). We continue this emphasis in our response.

Can neural replicators be construed as multiply concurrent? Our answer is that replicators may be multiply concurrent even though operant interactors rarely, if ever, are. We suggested above that the changes in operant behavior that constitute learning are materially encoded in a learner's nervous system. They are what remain after the response becomes history. Therefore, what is required for operant behavior to be multiply concurrent is multiple variants of the neural coding for responses belonging to the same lineage. We take the problem of their multiple concurrency to be similar to the problem of multiple concurrency in lineages of single-celled organisms in asexual species. In the biological case, when a bacterium is in an environment where it can survive, it may replicate. The result is two bacteria. If the genomes of the two bacteria differ in some way that makes one bacterium more suited to the selecting environment than the other, that genome will be more likely to replicate. As replication and interaction continue, many genomic variants of the lineage exist, each following its own selection path. Imagine such a lineage in an environment where only one bacterium at a time can interact with its environment but where all the other genomic variants that have survived to the present exist in a dormant state. That is the kind of world where operant behavior lives. The result of differential interaction of responses having varying neural features with the selecting environment is the differential preservation of some neural coding. That coding, along with coding for all responses in the lineage that have been reinforced, is conserved materially and remains available for replication.

However, we must admit that it seems very unlikely that the contingencies of selection ever constrained biological evolution in the way that we suggest replication in operant lineages is constrained. If nothing else, our article has forced those of us who think that operant conditioning provides an unproblematic paradigmatic example of selection to admit that we are mistaken. We could not have reached this conclusion without taking GAS seriously. Whether or not future research supports the suggestion that a history of reinforcement for responses in an operant lineage results in concurrently existing neural variants, it is not clear that there is a logical necessity for replicators to always exist concurrently. Although it is apparently the case that replicators in biological lineages exist concurrently, phenomena in any domain are constrained by the environment in which they exist. If a general account of phenomena in different domains is possible, it will likely entail significant differences in any two exemplar cases. As mentioned earlier, we share **Gabora**'s interest in viewing cultural level phenomena in terms of selection processes. Although she prefers an alternative to our GAS even for biological evolution, we prefer at this time to continue to explore evolutionary processes in the terms we used in our original article.

NOTE

1. The authors regretfully inform readers that since the publication of our target article in 2001, one of our coauthors, Rod Langman, has died.

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[The letter "r" before author's initials stands for Response to CC references]

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Commentary on **O'Regan & Noë (2001). A sensorimotor account of vision and visual consciousness. BBS 24(5):939–973.**

Abstract of the original article: Many current neurophysiological, psychophysical and psychological approaches to vision rest on the idea that when we see, the brain produces an internal representation of the world. The activation of this internal representation is assumed to give rise to the experience of seeing. The problem with this kind of approach is that it leaves unexplained how the existence of such a detailed internal representation might produce visual consciousness. An alternative proposal is made here. We propose that seeing is a way of acting. It is a particular way of exploring the environment. Activity in internal representations does not generate the experience of seeing. The outside world serves as its own, external, representation. The experience of seeing occurs when the organism masters what we call the governing laws of sensorimotor contingency. The advantage of this approach is that it provides a natural and principled way of accounting for visual consciousness, and for the differences in the perceived quality of sensory experience in the different sensory modalities. Several lines of empirical evidence are brought forward in support of the theory, in particular: evidence from experiments in sensorimotor adaptation, visual "filling in," visual stability despite eye movements, change blindness, sensory substitution, and color perception.

Violations of sensorimotor theories of visual experience

Bruce Bridgeman

Department of Psychology, University of California, Santa Cruz, Santa Cruz, CA 95064. **bruceb@cats.ucsc.edu**
http://zzyx.ucsc.edu/Psych/Faculty/bridgeman.html

Abstract: Although the sensorimotor account is a significant step forward, it cannot explain experiences of entoptic phenomena that violate normal sensorimotor contingencies but nonetheless are perceived as visual. Nervous system structure limits how they can be interpreted. Neurophysiology, combined with a sensorimotor theory, can account for space constancy by denying the existence of permanent representations of states that must be corrected or updated.

Motor theories have a long history in psychology. Early psychologists thought that identification of form might depend on hand or eye movements, for instance visually fixating the vertices of a triangle to identify and characterize it. The theories were demolished in some of the first visual experiments, where objects could be identified from only a short spark-illuminated presentation. Later, Noton and Stark (1971) proposed a more sophisticated motor theory, with a particular motor scanpath (a sequence of saccadic eye movements) associated with each image experienced. The difficulty with this and related models is that the scanpath must itself be remembered, even though the mechanism that generated the sequence of saccades in the first exposure to the image could potentially generate the same scanpath again. Scanpaths could be seen experimentally only if normal strategies of parallel processing were discouraged, for example by using large, low-contrast images that forced fixation at many locations for identification.

The O'Regan & Noë (O&N) (2001) view, though less dependent on overt motor action, runs into similar difficulties. Visual sensorimotor contingencies, for example, can be violated without canceling the essentially visual nature of the resulting experience. The classic pressure phosphene elicited by pushing on the side of the eyebulb is experienced as visual even though it obeys none of the laws of projective geometry. Johannes Müller's much-criticized doctrine of specific nerve energies explains the result; O&N's account does not.

According to O&N's theory, entoptic phenomena such as afterimages, pressure phosphenes, and so forth, should not be experienced as visual. Afterimages, for example, move with the eye, unlike stimulation from the external world. O&N would have to predict that an afterimage would eventually be experienced in the retina, not the world, because the results of actions such as eye movements or locomotions that change the eye's station point have results that are consistent, but contradictory to the rules governing external objects.

Similarly, pressure phosphenes are consistently associated with felt pressure on the eyelid, not with external objects, and should be experienced as such. They are never experienced without the co-occurrence of a feeling of localized eyelid pressure. Nonetheless, they are perceived both as visual and as occurring at the "impossible" location predicted by projective geometry, a position symmetrically opposite the tactile stimulation but determined by visual neuroanatomy. Consequently, their motion, resulting from

moving the finger that is pressing the eyelid, is consistent with projective visual geometry but contradictory to the felt motion of the finger. If the finger moves upward, the phosphene appears to move down, and vice versa. This contradiction should not be a matter of inadequate adaptation because all of our experience with phosphenes works in this way. Sensory signals from the finger, eyelid, and retina should all line up – but they do not. The finger and eyelid perceptions are consistent, but the retina remains forever the odd man out, as the doctrine of specific nerve energies demands.

The identification of visual stability across saccadic eye movements as a nonproblem echoes the conclusions of Bridgeman et al. (1994a; 1994b). If the correspondence between visual direction and stimulation is established anew for each fixation, based on current retinal and extraretinal input rather than on stored positions, stability is not a problem that the system must solve. The function of saccadic suppression, then, becomes not to correct the eye/world correspondence but to destroy it. Retinal smear during saccades must be prevented from reaching perception because it is an artifact of rapid eye motion rather than an external reality. Smear alone cannot be used to signal to the brain that a saccade is taking place; however, real smears that are physically similar to saccade-induced smears are perceived (Bridgeman 1983), and targets interrupted during saccades, eliminating smear, are perceived normally as long as the target reappears before the eye stops (Deubel et al. 1996). Differentiating saccade-induced smear from real rapid image motion requires an extraretinal signal; the rules of perception must change during saccades. This contrast requires the brain to know that a saccade is going on from something other than retinal stimulation – hence the function of the extraretinal signal.

O&N have offered the significant insight that the idea of qualia as states is a category mistake. There is further backing for this idea from sensory neurophysiology. When we look into the brain for the mechanisms of perception we find not states but processes. The whole idea of sensory "states" is a relic of a long-superseded static view of the nervous system, a view that survives only in some corners of philosophy. Neurons summate inputs and fire in bursts, other neurons react with longer latencies, the sensory signal is repeatedly reorganized, and the activity eventually dissipates. The activity is ephemeral, ever-changing: There is never a "state" in the sense of a static, definable circumstance or condition. Experiences are something we do, or more accurately something that the brain does.

Moving beyond primitive forms of activity-oriented theories of perception, O&N allow potentials for change under motion to define percepts; modern neurophysiology demands that the changes can also be internal neural transients rather than literal motor events. In like manner, memory is a synthesized abstraction, not a record of the world but that part of experience that might inform current or future behavior, consistent with O&N's insight that the sensory world is not internalized.

The world as an inside working memory

Naoyuki Osaka

Department of Psychology, Kyoto University, Kyoto 606-8317, Japan.
osaka@psy.bun.kyoto-u.ac.jp http://psy.bun.kyoto-u.ac.jp/osaka/

Abstract: O'Regan & Noë's (O&N's) theoretical framework for studying visual perception and awareness is intriguing. This framework, based on sensorimotor contingency, can be examined in recent visual brain theories using neuroimaging methodologies. Here, I consider how a working memory (WM) system explains the sensorimotor account of visual consciousness. I believe WM inside the brain provides at least partial support for O&N's theory.

The target article by O'Regan & Noë (O&N) (2001) presents a new framework for thinking about why things appear as they do. According to their framework, things appear not because they are what they are but because of what O&N call the "governing laws of sensorimotor contingency." The authors argue that a role of vision is exploring the world based on sensorimotor contingency. According to this hypothesis, the experience of seeing occurs when we master these governing laws. Thus, O&N reject the theory that activation of the visual brain's cortex should produce a visual experience based on an internal representation of the world. However, they have provided little detailed explanation of how these governing laws work in the world as an outside memory. It is not clear how, by introducing these governing laws, O&N escape a "re"-presentation theory of the world inside the brain without the referring neural activity necessary for vision. On the other hand, their theory does appear to attempt a reconciliation of their Gibsonian-like view with current visual brain theories by positing the importance of the ventral/dorsal visual streams, even though they assert that the activation of cortical maps itself cannot explain the produced visual experience. O&N further argue that the visual experience cannot be "equated" with the simultaneous occurrence of a particular neural event. In the strictest sense, this is true; however, I think it is not "equated" but is just a "neural correlate" of the visual experience. They also contend that the "binding problem" is a pseudo-problem; however, it is our belief that neuroscientists need this "working" hypothesis for building an architecture for computational theories of vision. This architecture is scientifically testable, whereas O&N's external memory hypothesis is outside the realm of testing by evidence. We now have evidence that sparsely distributed groups of visual neurons work together in a temporally (synchronous binding) correlated manner. A number of neuropsychological findings also indicate that damage to a specific visual brain area results in loss of a corresponding visual experience, and this is strongly suggestive of a "neural correlate" of visual awareness. For example, patients with damage in the middle temporal (MT) area of the visual brain have been found to lack motion perception (Zihl et al. 1983).

O&N argue that something "extra" would appear to be needed in order to make excitation in cortical maps provide the visual experience. The something extra, from my viewpoint, is not a sensorimotor contingency but a kind of short-term visual WM that actively binds sensory information from the environment with appropriate planned action in a attention-guided manner. So how does WM work in the brain? WM keeps various modality-specific information temporally active and creates a goal-directed exploratory action. Miller et al. (1960), in earlier days, presented an important theory of WM and conceived of contextual processing in the prefrontal brain as playing a critical role for guiding planned exploratory activity. Other current computational neuroscience theories posit prefrontal cortex to be critical for active explorative behavior in terms of "temporal integration of sequential behavior" (Fuster 1997) and a "supervisory attentional system" (Shallice 1988), both closely connected to internally driven exploring activity. We have no need for sensorimotor contingency for active exploring. O&N suggest that to possess visual awareness is to make use of sensorimotor contingency for the purposes of thought and planning; however, this should be read as follows: visual awareness is to make use of WM for the purpose of thought and planning (Osaka 2002).

Recent brain-imaging studies have attempted to identify functional brain anatomy underlying WM systems based on a WM model originally proposed by Baddeley (1986). An extended model by us assumes that two types of active WM are subserved by distinct cortical structures under the control of a central executive (CE). One is a vision-related WM consisting of dorsal (e.g., spatial WM) and ventral (e.g., face WM) streams. The other is verbal WM located close to the ventral (semantic) stream. The CE, located in the prefrontal cortex (Petrides et al. 1993), controls the processes that make a planned exploratory activity possible and coordinates the two types of WM. The allocation of WM resources is assumed to be coordinated by the CE. An example is the *n*-back dual task in visual WM in which the subject is asked to make an

appropriate action when he or she detects the same visual pattern presented *n*-trials before the current trial. In this dual task, subjects have to store successively presented patterns, keeping them "alive" in short-term visual WM while updating information until the appropriate pattern can be monitored and detected (Smith & Jonides 1997). Recent fMRI studies clearly indicate that dorsolateral prefrontal cortex (DLPFC; Brodmann area 46/9), where the CE is assumed to function, is highly activated during *n*-back tasks (Owen et al. 1990) and object/face WM tasks (Courtney et al. 1998). CE appears to work also in the DLPFC in cooperation with the dorsal part of the anterior cingulated cortex, which is assumed to act as an attention-controlling system (Osaka et al. 2002; Petrides et al. 1993). Also, activation of the DLPFC has been observed when two kinds of exploratory activity are performed together (D'Esposito et al. 1995), and damage to the DLPFC is likely to decrease both the activity associated with exploring the environment and performance in spatial WM tasks (Owen et al. 1990). Therefore, we argue that the CE may work as a sensorimotor system based on attention-guidance. In this view, WM is the basis of visual awareness, and the seemingly outside world becomes the internal WM.

We agree with O&N that seeing is a way of acting and is a particular way of exploring the environment. However, one should not necessarily reject an internal representation theory based on brain activity. A theory of sensorimotor contingency might possibly be attributable, in some degree, to the brain's dynamic CE function. Visual experience is just a product of the relevant CE dynamics which guide and coordinate visual information to appropriate action in the environment.

Virtual action: O'Regan & Noë meet Bergson

Stephen E. Robbins

Center for Advanced Product Engineering, Metavante Corporation, Milwaukee, WI 53224. **Stephen.Robbins@metavante.com**

Abstract: Bergson, writing in 1896, anticipated "sensorimotor contingencies" under the concept that perception is "virtual action." But to explain the external image, he embedded this concept in a holographic framework where time-motion is an indivisible and the relation of subject/object is in terms of time. The target article's account of qualitative visual experience falls short for lack of this larger framework.

> *[Objects] send back, then, to my body, as would a mirror, their eventual influence; they take rank in an order corresponding to the growing or decreasing powers of my body. The objects which surround my body reflect its possible action upon them.*
> – Henri Bergson (1896/1912, pp. 6–7)

So Henri Bergson would initiate his thesis that perception is *virtual action.* It is a more succinct phrase for the important theme of O'Regan & Noë (O&N) (2001) wherein sensorimotor contingencies underlie vision, though the latter concept, as developed, lacks an appreciation of the power of Gibson's invariance laws in specifying events and as input to the action systems. But the primary point here is this: O&N lack the framework in which Bergson embedded this concept, and for this reason their attempt to use it to explain visual experience suffers.

What does the "world as external memory store" look like? If a fly is moving by in the external field, is it the buzzing being of our normal scale, is it flapping its wings heron-like, is it a whirling mass of electrons, a continuously transforming ensemble of quarks, a local pool of pulses in a vast universal sea? The external world as we know it is not simply there to be sampled. The brain imposes a scale of time. It is itself a dynamical system integrating multiple scales, from quark through electron through chemical flows through neuronal patterns. It can be asked, as did Hoaglund (1966), if, *in principle,* the process velocity underlying this global dynamics can be changed, if, for example, the "buzzing" fly of our normal scale could become a heron-like fly, barely flapping his wings – that is, a new specification of scale?

Scale implies *quality.* The "buzzing" fly is a certain quality, the heron-like fly another. Our normal "red" is one quality; the far more vibrant red of the heron-like scale, nearer the individual oscillations of the electromagnetic field, is another. That the underlying dynamics impose a scale, already takes us beyond the origin of quality as simply the interrelation of actions – beyond "sensorimotor contingencies at play" (sect. 6.3). Scale also implies *extent.* The buzzing fly defines a certain time-extent – a multiplicity of *past* events, such as wing oscillations, summed in a blurred visual display. The heron-like fly defines a much lesser extent, the quark-fly far less.

On the one side, we see the brain with its dynamics inherently incorporating the motor systems via their reentrant connections to visual areas, and thus supporting the sensorimotor contingencies. This dynamics, characterized perhaps by an attractor, looks nothing like the world of experience. On the other side, we have the world-out-there as experienced – these are two completely different terms; hence the gap. O&N would have us stop here. We need only the external memory store, waiting to be sampled, virtually acted upon. But action upon what? The external field looks nothing like the world as experienced. What is the four-dimensional extent of this field? At the null scale of time, it is, in the root sense, a non-imaginable field. Sensorimotor contingency, in and of itself, cannot explain the origin of our normal *image* of this field.

Bergson, 50 years before Gabor's discovery, 85 before Bohm (1980), saw this field as a holographic field. He visualized it as a vast field of *real* actions where every object is obliged "to transmit the whole of what it receives, to oppose every action with an equal and contrary reaction, to be, in short, merely the road by which pass, in every direction, the modifications, or what can be termed *real actions* propagated throughout the immensity of the entire universe" (1896/1950, p. 28). Discarding the concept, as do O&N, that the brain develops a photograph or representation of the external world, Bergson argued in holographic terms:

> But is it not obvious that the photograph, if photograph there be, is already taken, already developed in the very heart of things and at all points in space. . . . Build up the universe with atoms: Each of them is subject to the action, variable in quantity and quality according to the distance, exerted on it by all material atoms. Bring in Faraday's centers of force: The lines of force emitted in every direction from every center bring to bear upon each the influence of the whole material world. (1896/1912, p. 31)

Individual perception, Bergson argued, is *virtual action.* An organism is a system of field elements organized for action. Embedded in the vast (holographic) field of real actions, those influences to which its action systems can respond are reflected as it were as virtual action, the rest simply pass through.

> Only if when we consider any other given place in the universe we can regard the action of all matter as passing through it without resistance and without loss, and the photograph of the whole as translucent: Here there is wanting behind the plate the black screen on which the image could be shown. Our "zones of indetermination" [organisms] play in some sort the part of that screen. They add nothing to what is there; they effect merely this: That the real action passes through, the virtual action remains. (1896/1912, pp. 31–32)

Put in holographic terms, the brain is now seen as a modulated reconstructive wave "passing through" a holographic field. The reentrant architecture, the resonant feedback loops, the "scales" of neural dynamics all ultimately support this modulated wave. As a wave traveling through a hologram specifies a virtual image, this brain-supported wave specifies a time-scaled, virtual subset of the field related to the body's action.

There is no homunculus here viewing a reprojected wave front (image). First, due to the holographic nature of the field, wherein each point carries the information for the whole, there is an elementary or "pure perception" in Bergson's terms defined across

the field at the null scale. This is reinforced by the time-motion of the field, a motion that must be treated not as a series of discrete states or "instants" but as indivisible. As does Nottale (1996) now, Bergson rejected the differentiability of the space-time continuum. It is this indivisible motion that fundamentally supports the qualitative aspect of the world with its time-extents – "buzzing" flies or heron-like flies, or "mellow" violins (Robbins, in press). Secondly, the modulated wave supported by the body/brain is not spatially distinct from the field. The crucial principle of Bergson is this: "*Questions relating to subject and object, to their distinction and their union, must be put in terms of time rather than of space*" (1896/1912, p. 77, emphasis in original). The buzzing fly and the transforming brain are phases of the same dynamically transforming field. At the null scale of time there is no spatial differentiation. But gradually raise the ratio of events in the matter-field to events at the highest scale or level (neural) of the brain and, from a vaguely outlined ensemble of whirling "particles," the form of the fly begins to coalesce, then barely move its wings, then becomes the heron-fly, then becomes the buzzing being of our normal scale. The dynamical state of the brain specifies a four-dimensional extent, a time-scaled subset of the *past* or the past motion of the matter-field; that is, *it is specific to a time-scaled subset of the elementary perception defined over the field.* Symmetrically, it is specific to the possibility of *future* action.

This is Bergson's framework for the relation of sensorimotor contingencies to external field, and therefore the origin of the "external" image; it is how we take "the perceived detail to be out there in the world" (sect. 6.7) The non-differentiable time-motion of this field underlying (scaled) four-dimensional extents is the true support for quality. Within this framework, and implicit in sensorimotor contingencies, is another implication (Robbins 2000; 2001; 2002). If perception is the display of virtual action, it is the display of capability of action (e.g., for the buzzing fly, his wing-beats a-blur, of the modulation of the hand-arm necessary to grasp the fly). But if the dynamics underlying this can be changed – that is, if the chemical velocities underlying this global dynamics are increased – then perception must change. The fly perhaps becomes the heron-like fly precisely because it is a new specification of the possibility of action, perhaps now showing the possibility of removing the fly from the air by his wing tip. This must be so if perception is to be ecologically valid. Albeit unclear practically how today, this is a testable consequence.

An epistemological account of visual consciousness

Peter D. Sparks[a] and E. E. Krieckhaus[b]

[a]*Department of Psychology, University of Colorado, Boulder, CO 80309;* [b]*Village Information II, New York, NY 10011.* **sparks@psych.colorado.edu krieck@worldnet.att.net**

Abstract: O'Regan & Noë's (O&N's) explanation of our stream of experience as activities depends on their denial of that palpable, most real aspect of experience: what they call "qualitative experience." Given the ontological primacy of the qualitative givenness of our experience and the complete absence of actions as experiences in our stream of consciousness, though, all such reductionistic attempts must fail.

According to O'Regan & Noë (O&N) (2001), visual awareness (or experience) is due to two processes: first, the brain abstracts laws of sensorimotor contingencies from sensory changes that co-occur with movements, and codes them. One must also exercise mastery of this information or expertly "know all about" (sect. 2.4 of the target article) these contingencies. Second, this knowledge is used for thought or planning, and with this, one becomes aware, or "you see it" (e.g., a color; sect. 2.6). It is important to note that the authors reject the phenomenal or qualitative aspect of experience, arguing that "[visual] qualia are an illusion" (sect. 6.3), and that qualitative experiences are simply "ways of acting. . . . They are things we do" (sect. 6.3). In essence "there is no simple, unanalyzable core of the experience. There are just the different things we do when we [experience]" (sect. 6.4.1). We have three critiques of their proposal.

First, at critical points they are either unclear or inconsistent in describing what experience consists of. For instance, they claim experience consists of many different activities, such as "eye movements, shifts of attention," and "the application of understanding" (sect. 6.4.1), but only one activity, the use of knowledge about sensorimotor contingencies, appears critical (sect. 2.6). In other sections, they equate experience with knowledge ("to experience a red object . . . is to know such things as" sensorimotor contingencies [sects. 6.6 and 6.8]), or knowledge with an activity, ("[seeing red] consists in the (implicit) knowledge associated with seeing redness" [i.e., the laws of sensorimotor contingencies; sect. 6.4.1]). Which of these "things we do" is critical for one to experience? Is experience just activity or is knowledge critical? If knowledge is a critical component of experience, then it is incorrect to state that there are just things we do when we experience unless knowledge is an activity. Since these authors argue against the intuitive understanding of the nature of experience as qualitative, it is critical that they should be clear in their description of their alternative reduction.

Second, and most fundamental, O&N have not provided a compelling argument to reject the existence of phenomenological or qualitative experience. In describing experience in terms of activities, they could argue one of two positions; qualitative experience exists but it is the same thing as activities (the identity argument), or phenomenological experience does not exist and the totality of experience is only activities (the eliminative argument). They argue for the elimination of the reds and qualia of experience, but their argument fails in this rejection of the qualia or red. Consizder their general argument for rejection: (1) vision consists of two processes; (2) if qualitative visual experience is a subset of vision, then experience consists of these processes; and (3) since processes are not qualitative sensory-like states, phenomenological experience must be rejected. Their argument depends on the first premise, that visual experience consists of only these processes. However, they argue only that these processes are involved. The possible existence of these processes, though, does not at all preclude the added existence of a phenomenological or qualitative aspect of experience.

This same basic problem occurs when O&N try to support their argument by demonstrating how experience can be described completely in terms of activities or processes, without reference to phenomenological states. In describing the experience of a red wall (sect. 6.4.1), they invite us to "try to describe the experience" and suggest "one thing you might do is direct your attention. . . ." Notice that they have already changed the argument from describing the qualitative aspects of experience to describing the things that you might do in order to have such an experience. In accord with their list of activities, they conclude "there are just the different things we do when we [experience]." Again, the existence of these unseen activities, and the role that they may or may not play in experience, does not in any way preclude the existence or indeed primacy of phenomenological experience itself. In fact, simple observation reveals there is something much more than activities; there is the immediate experience of red itself as qualia. In describing the experience of a red wall, one may say that its color is deep or dark, purplish or like a tomato. A complete description of the experience may occur without any reference to activities one engages in while experiencing. This suggests a fundamental difference between the experience of red and the things we do when we experience red. This invalidates O&N's conclusion that there are only the different things we do when we experience.

Finally, our critique, though largely a demonstration of the inadequacy of their model, fundamentally rests on the undeniable existence, indeed primacy, of qualitative experience. That is, experience clearly consists of qualities such as reds and sounds, but

does not clearly consist of things like activities or sets of contingencies – at least not as experienced. We view the issue of qualitative experience as an epistemological problem of the ontological character of experience. Any discussion of this issue must begin with the undeniable existence of qualitative experience, not how qualia are an illusion and that our stream of experience is simply a set of activities or contingencies.

Editors' Note: There is no Authors' Response to this commentary.

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