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Infants, Too, are Global Perceivers (Commentary on T.A. Stoffregen and B.G. Bardy, On Specification and the Senses)

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Commentary/Stoffregen & Bardy: On specification and the senses

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Abstract: Infants are global perceivers. They detect patterns in stimulation that allow detection of many affordances of the environment. Pick-up of structural patterns across forms of ambient energy awaits maturation of sensory systems and improvements in motor skill, but development proceeds rapidly during the first year. Researchers in perceptual development must devise and refine existing tools to examine infants’ abilities.

When I first examined infants’ emotion perception using an intermodal task (Walker 1982), I was asked why I compared an infant’s looking time to a happy facial expression projected alongside a sad facial expression and accompanied by a happy vocal expression to that shown in the opposite setup. That is, the comparison was with the infant’s looking time to the happy expression when it was projected alongside the sad expression, but accompanied by a sad vocal expression. The expectation instead had been that I should use, as baseline, looking at a facial expression when it was sad, vocal expression when it was happy, and a silent pair. I have struggled to answer this question, never able to furnish an argument that convinced although I was certain of my choice. I have argued that (1) two facial expressions and one soundtrack and (2) two facial expressions – comprise only two very different events. Stoffregen & Bardy (S&B) provide the rationale I have been seeking: the whole is qualitatively different from the sum of its parts. The integrated action of seeing and hearing leads to the perception of an “irreducible” product, in the present example an emotional expression that affords opportunities for action.

Infants, too, are global perceivers

The target article is the paper I almost wish I had written. S&B provide a logical, well-supported argument for specification. They point out how many of us have been unable to escape the assumption that the senses are separate channels even as we claimed we were rejecting that accepted wisdom. They clarify for me why it has been so difficult to maintain precision with such terms as “intermodal,” “crossmodal,” “amodal,” and “multimodal.” I have tried, for example, to reserve “crossmodal” for situations in which an observer viewed something and subsequently heard or felt it, a situation that may require the kind of inference that most theories assume.

S&B make additional points that speak to the common fallacy that infants will be unable to “process” information when they encounter in the lab a stimulus event such as a moving, computer-generated disk punctuated by a beep at the lowest point in its trajectory, flanked by another disk that is not. The usual assumption is that infants must compare information obtained via vision to that obtained via audition to determine which icon is consistent with the sounds, and that this will tax their abilities (Bahrick 1992; Lewkowicz 1993). Infants at a specific age may indeed fail a specific intermodal task, but not because they cannot deal with simultaneous presentations of separate optic and acoustic arrays. Sensory to a higher-order pattern is required for individual comparisons of information derived from single-energy arrays.

Two aspects of S&B’s paper could be improved. First, although they acknowledge that James Gibson (1966) provided the original example for information in the global array, they do not describe the scope of his contribution. This may represent misinterpretation, ambiguities in the theory, or evidence for the growth of Gibson’s own thinking. Congruent with the latter, Gibson (1979) himself described the theory of information pick-up as in an “undeveloped state.” But he went on to say: “Information is not specific to the banks of photoreceptors, mechanoreceptors, and chemoreceptors that lie within the sense organs. Sensations are specific to receptors and thus, normally, to the kinds of stimulus energy that touch them off. But information is not energy-specific” (p. 243). He asserted that we directly perceive the qualities of things in the world, especially their affordances. S&B take up these ideas and propel them much farther along the path Gibson had begun to clear.

Second, S&B little attend to developmental research. Consider results from Walker-Andrews and Lenon (1985) and Pickens (1994). In the earlier study, 5-month-olds observed videotapes of a Volkswagen (VW) moving toward or away from them accompanied by a noise that increased or decreased in amplitude. Infants looked preferentially at the videotape consistent with the engine noise – the approaching VW when the noise grew louder, the receding VW when it softened. Note that the rate of change in amplitude was correlated with movements of both vehicles, but infants responded to directional information as well. Pickens (1994) introduced critical refinements – a condition in which a toy train changed in size only (rather than moving in depth), another in which each film’s brightness varied, and one in which the trains moved up and down. Infants looked appropriately for motion in depth and, less so, for size changes. They did not show preferences related to brightness changes or vertical movement. Five-month-olds were sensitive to ecological relations specifying approach versus retreat and did not generalize to intensity or metaphorical relations.

Although S&B fail to capitalize on the wealth of infant data, they simultaneously present a challenge to developmental researchers. The authors proffer but dismiss the possibility that infants are initially sensitive to structure in single-energy arrays and develop sensitivity to the global array because this assertion demands the corollary that there are separate senses. In fact, infants appear to be sensitive to the global array. They detect some invariant patterns in the first few weeks of life (e.g., Gibson & Walker 1984; Melzloff & Borton 1979). Months later they can detect arbitrary relations such as those contrived between the color and taste of a substance (Reardon & Bushnell 1988) or a label and a moving ob-
Motion, frames of reference, dead horses, and metaphysics

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Abstract: Various annoyingly incorrect statements of Stoffregen & Bardy are corrected, for example, that perception researchers commonly use the term “absolute motion” to denote motion without any frame of reference, confuse earth-relative and gravity-relative motion, and err with respect to the frame of reference implied by their subjects’ motion responses, believe in sense specific motion percepts, and do not investigate sensory interactions at neurophysiological levels. In addition, much of the target article seems to concern metaphysics rather than empirical science.

Stoffregen & Bardy (S&B) state that “a common concept . . . among many researchers, including myself, is . . . the idea of absolute motion” not defined in terms of any frame of reference (S&B use the term referent). But neither I nor anyone else that I know has ever embraced such a peculiar idea. The relativity of motion has always been my explicit point of departure (Wertheim 1981) and I have argued that a theory must be flawed if it yields a concept of motion that is not definable in terms of a frame of reference (see e.g., my discussion of the “hidden reciprocity assumption” in Wertheim 1994, sect. R2). S&B call on researchers always to mention the particular frame of reference in terms of which they define motion. But this is what everybody has been doing all along (see e.g., Swanston & Wade 1988, Wertheim 1994, and many of its accompanying BBS commentaries, also sects. R5 and R6). In the literature (e.g., Kinchla 1971; Wertheim 1994, p. 302) the term “absolute motion” denotes motion defined in terms of the three-dimensional (3D) frame of reference dimensioned by the earth’s surface and the direction of gravity. Other names might have served just as well; for example, “motion relative to absolute space,” “Newtonian motion,” “exocentric motion,” or “earth-relative motion.” This is perfectly in line with Einstein’s claim that the idea of frameless motion has no meaning. When S&B defend Einstein’s views vis-à-vis those of perception researchers (see also Stoffregen 1994), they beat a dead horse.

The same can be said about S&B’s elaborate argument that there can be motion relative to the earth without it being relative to the direction of gravity (i.e., when perpendicular to the direction of gravity). To my knowledge nobody has ever equated earth-relative motion with motion relative to the earth’s gravity. S&B also criticize many researchers, including me, for stating that the perception of visually induced self-motion is often illusory. What those authors mean is that in the presence of a large optic flow field, one often experiences a perception of self-motion relative to the earth’s surface, while, physically speaking, one remains stationary relative to that surface. The most common example of this illusion occurs when an earth-stationary observer is seated inside a rotating optokinetic drum. S&B claim that this is not an illusion, because the relative motion between the drum and the observer is correctly perceived. It is, but that is not the illusion which concerns a different percept, namely, perceiving self-motion relative to the earth’s surface. S&B seem to believe that this is not really perceived inside the drum, although experimenters believe it is. They claim that there is no illusion; only a misunderstanding between observer and experimenter as to the frame of reference relevant to the observer’s percept, a misunderstanding which should disappear when the frame of reference is explicitly stated in the perceiver’s verbal report.

However, whether S&B like it or not, earth-relative self-motion really is experienced by observers inside an optokinetic drum: they believe that they are moving relative to the floor of the experimental room in which the drum is located (and perceive the drum as stationary relative to that floor). Since this is not physically the case, the term illusion is correct.

Contrary to what S&B suggest, researchers in the field of visual-vestibular interactions and self-motion (including myself) are always careful to correctly ascertain the frame of reference in which subjects report self-motion percepts. They either specifically ask about it, or use non-verbal methods (e.g., by asking the subjects to continuously keep a joystick pointed toward where they believe the door of the experimental room is located). In fact, these researchers were the first to recognize the dangers of verbal ambiguities about frames of reference; terms such as “exocentric” and “egocentric” originated from their work. S&B’s accusation that researchers “routinely exclude correct responses from their analysis because of verbal ambiguities in their subjects’ reports,” reveals a shocking lack of knowledge. This is not even kicking a dead horse, but kicking a nonexistent one.

Another problem is S&B’s claim that my analysis of percepts of “absolute motion” is sense-specific (see also Stoffregen 1994), that is, requires only one sensory system. This is incorrect. In my model (Wertheim 1994) the retinal coordinates of image motion are recalibrated into the 3D coordinates of the frame of reference defined by the earth’s surface and gravity. This is brought about with what I termed “reference signals.” These are compound signals constructed from sensory afferents generated by various sensory systems (somatosensory, vestibular, and visual). In addition, retinal and reference signals themselves have no perceptual meaning. It is their interaction which yields percepts of motion.

Consequently, on the perceptual level, one cannot speak of separate senses. This is also implied by other inferential theories, which use the theoretical forerunners of reference signals (“extraretinal signals,” “corollary discharges,” “efference copy signals”). Hence, no inferential theorist assumes that motion perception is sense-specific (see Wertheim 1999, for a more detailed analysis of this issue). Nor do vestibular researchers – who, for decades now, are trying to unravel the way how retinal, somatosensory, and vestibular afferents interact to bring about particular percepts of self-motion – assume that motion perception (of any kind) is sensory-specific (see e.g., Sauvan 1999). On the contrary, these sensory interactions are their core business, both on the perceptual and on the neurophysiological level.

Hence, it is not at all surprising that S&B have been unable to locate an explicit justification of the assumption of separate senses in the philosophical, behavioral, or neurophysiological literatures. Who would want to justify a false assumption? S&B’s call to search “for neural units that respond to patterns of activity that extend across different kinds of receptors, such as the retinae and the vestibule,” again is out of touch with the literature. Here too the authors beat a dead horse.

Finally, it is difficult to make sense of S&B’s discussion of various possible relations between an energy array and (aspects of) reality – a relation which is “prior to and independent of . . . psychological processes.” The point is that, reality per se is unknown. This is metaphysics, not empirical science. Reality can only be assumed: we assume that what we perceive is reality. For all practi-