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Observations

Visually Perceiving Distance: A Comment on Shebilske, Karmiohl, and Proffitt (1983)

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Shebilske, Karmiohl, and Proffitt (1983) interpret their data as showing that (a) the reference tonus level of the extraocular muscles controlling vergence is affected by everyday conditions of close viewing, and (b) this naturally induced phoria affects the visual perception of distance under natural viewing conditions. We note that these interpretations do not fully concur with the data presented—for example, the second conclusion favorably conflates partial results from two separate experiments—and we identify a number of confoundings that reduce the likelihood that the reported inaccuracies in distance judgments were due to variations in efference.

In a recent article Shebilske, Karmiohl, and Proffitt (1983) claim to have demonstrated that efferent signals to the eye muscles are taken into account in the visual perception of distance in natural circumstances. Further, they claim that this demonstration runs counter to Gibson's views that (a) visual perception is ordinarily direct and, (b) relatedly, that the specification of surface layout by the (lawfully generated) optic array is ordinarily sufficient to support successful animal activity. It seems to us, however, that these claims are not warranted by the data and analyses that Shebilske et al. report. The justification for their conclusions seems to be more ideological than empirical. Our major misgivings are outlined below.

First, it is questionable whether Shebilske et al.'s criteria for naturalness are Gibson's criteria. Shebilske et al. claim that their full-cue condition mimics everyday conditions in which people orient and act toward objects. It is difficult to evaluate this claim on the basis of the data reported and the description given in the article and Shebilske's (1981) synopsis. With respect to the data, the pre- and posttest distance judgments in reduced-cue and full-cue conditions were not presented. And there was no analysis of variance to confirm the overall superior performance that must be found in the full-cue condition if it did, in fact, more closely approximate natural conditions. (For a similar contrast Foley & Held, 1972, did find the expected superiority.) With respect to the description, we gather that the observer's task is more properly described as estimating distances in the air rather than recession along the ground. If our reading of their description is correct, then it seems that Shebilske et al.'s interpretation of the ecological conditions of distance perception is not Gibson's. To quote Gibson (1979, pp. 159–160):

> Investigators in the tradition of space perception and the cues for depth have usually done experiments with a background in the frontal plane, that is, a surface facing the observer, a wall, a screen or a sheet of paper. A form in this plane is most similar to a form on the retina and extension in this plane might be seen as a simple sensation. This follows from retinal image optics. But investigators of environment perception do experiments with the *ground* as background, studying surfaces instead of forms, and using ecological optics. Instead of studying distance in the air, they study recession along the ground. Distance as such cannot be seen directly but can only be inferred or computed. Recession along the ground can be seen directly.

By our reading, Shebilske et al.'s experiments are consistent with the air-theory tradition that Gibson rejects in the preceding quotation and that he rejected nearly 30 years previously (Gibson, 1950). In and of itself this conclusion, if correct, undercuts the larger claim of Shebilske et al. that they are evaluating the contribution of efference to visual distance perception under the "boundary conditions" acceptable to Gibson (Shebilske, 1979, 1981).

Second, it is not clear what Shebilske et al. mean by the claim that minor motor anomalies (disturbances in efference) *cause* illusions. Conventionally, the term illusion is used when a subject's perception of a situation does not agree with the physical description of the situation (e.g., Coren & Girgus,

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1977). If an object is at a distance of x cm from an observer, but the observer behaves *consistently* as if it were at a distance of y cm, then it should be argued, in the conventional vein, that the observer is experiencing an illusion. How well do observers' perceptions of the distances of objects (in an experimental setting like that of Shebilske et al.) compare with the physically measured distances in the absence of minor motor anomalies? Included among the many things Shebilske et al. do not report are the before-induced-phoria data. An answer can be derived, however, from the data reported by other investigators. The answer is "not very well." Gogel's (1972) observers adjudged point sources at 10 ft (3 m) and 20 ft (6 m) in a homogeneous surround to be at the same distance. Foley and Held's (1972) subjects, in similar circumstances, consistently judged nearby targets to be about 25 cm beyond their actual positions. As a rule, perceived egocentric distance varies more slowly than physical distance, with the distance of nearby objects being consistently overestimated (Ebenholtz, 1981; Foley & Held, 1972; Foley, 1980; Gogel, 1977; von Hofsten, 1976; Wallach, Frey, & Bode, 1972).

Strictly speaking, the observers in the experimental setting employed by Shebilske et al. always experience distance illusions. If minor motor anomalies, therefore, do not cause the misperception of absolute egocentric distance under the standard experimental conditions, what do they do? Perhaps Shebilske et al. would like to say that minor motor anomalies exaggerate the illusion. The logic of illusions, however, prohibits this move. Consider the following. The targets in the experiments of Shebilske et al. were close by (33 cm, on the average), suggesting that the subjects in the pretest conditions probably overestimated target distance (again, we are not told). Suppose that exophoria rather than esophoria had been induced in the interval between tests and that, as a consequence, the subjects perceived the targets as so many centimeters *closer* on the posttest. Then it would have to be argued, by the conventional logic of illusions, that induced phoria produced more veridical performance.

Third, there appears to be no basis for Shebilske et al.'s conclusion that their research ". . . suggests that everyday conditions induce errors in eye convergence (esophoric shifts) and corresponding illusions of visual distance in well-illuminated, structured environments (p. 274)". Only Experiment 1 tried to induce an esophoric shift through an "everyday condition." Experiment 1 found no effect of induced errors in eye convergence on distance judgments in the full-cue condition. Moreover, it is highly questionable whether Experiment 1 showed, as the authors claim, that the everyday condition of close handwork induced an esophoric shift.

Intervening between the phoria pre- and posttests the observers were subject to the full- and reduced-cue conditions as frequently as they were required to perform the close handwork task and, moreover, were subject to these conditions immediately prior to the phoria posttest. In the full- and reduced-cue conditions the observers fixated on, and took time to estimate the distance of, small luminous targets. Subjects reported their estimates by pointing with their unseen hands. On the average these targets were, as noted, 33 cm from the observer. We suspect that this egocentric distance of 33 cm was chosen because of its proximity to the physiological position of rest for the horizontal vergence system reported by Ebenholtz and Wolfson (1975) and Paap and Ebenholtz (1977). On the basis of Ebenholtz and Wolfson's (1975) data, fixating a target in the neighborhood of 33 cm should induce minimal aftereffects. Shebilske et al. report minimal shifts in distance judgments from pre- to posttests, namely, +1.22 cm in the reduced-cue condition and −.07 cm in the full-cue condition. All three posttests followed some (variable) period of fixating targets in the vicinity of 33 cm. The absence of a Cue (reduced, full) × Induction Period (5, 10, 15 min) × Test (pre, post) analysis of variance on the distance data encourages the speculation that there were in fact no significant main effects or interactions (in contrast to what is claimed via the t tests that Shebilske et al. do present). So, given that the distance-shift data are consistent with what one would expect from a few minutes of fixating at 33 cm, why should the phoria data be treated any differently? That is, the esophoric shift of 1.20 diopters can be credited to the distance perception pre- and posttests rather than to the close handwork.

In close, visually guided handwork, the limbs and trunk remain in view, and the places and movements an observer fixates upon vary markedly (which means that the fixation distances, and the fixation displacements from the midline, vary). Even the coarse measure of "viewing distance" during handwork varied as much as 17.8 cm according to Shebilske et al. Experimentally, it is known that the aftereffects of near and far viewing are reduced considerably, even eliminated, by relatively little variation in fixation (Ebenholtz & Wolfson, 1975; Foley, 1974; Paap & Ebenholtz, 1977). On both experimental and theoretical grounds it is widely believed that rich optical structure in general and the sight of the body in particular customarily tune the convergence system (e.g., von Hofsten, 1979; Lee, 1978; Skavenski & Steinman, 1970; Welch, 1978). In short, from what is currently known, it is unlikely that close handwork and other similar "naturalistic" activities, characterized by variations in fixation and rich optical support, incur substantial muscle potentiation effects in the vergence system. On the other hand, esophoria induction is to be expected from a task in which near fixation is approximately constant and the optical support for vision is severely reduced—such as judging distance in the reduced condition of Experiment 1. We conclude, therefore, that Shebilske et al.'s evidence for natural everyday activities inducing errors in eye convergence is weak, and the evidence that the convergence errors, said to be induced naturally, carry over into well-illuminated, structured environments is nonexistent in the experiments that they report. In sum, the answer to the first question raised in their introduction, namely, "Do naturalistic conditions induce an esophoric shift and a corresponding overestimation of distance?" is negative and, as a consequence, most of the argument in the discussion section is immaterial.

Fourth, the controls and analysis provided seem to be inadequate for the task of resolving the second question raised in their introduction, namely, "Does the distance illusion that accompanies an esophoric shift occur in a well-illuminated, structured environment?" Shebilske et al. mean by this question: Does the extra innervation required to attain a given vergence posture (due to a shift in the reference tonus level of the vergence system) cause a change in visually perceived distance in a natural setting? In Experiment 2 it was shown that in a well-illuminated room, a subject attempting to place his or her concealed right index finger directly below a small visible object overreached the object by a greater distance (2.3 cm) after 10 min of near fixation in darkness (which induced an esophoric shift) than before. By itself this demonstration cannot provide an affirmative answer to the question above. Missing from Experiment 2 are measures and controls for determining the contribution of other possible changes during the pre- and posttests—that is, changes other than the reference tonus level—that might have been responsible in part or in full for the increased overreaching, such as (a) the magnitude and direction of variance in steady binocular fixation, (b) the perceived position of the concealed hand, (c) the state of postural tuning reflexes, (d) the exocentric distance between the target and other background objects, and (e) the target/background configuration.

Consider (a). In the air-theory perspective on distance perception it is a point of debate whether slight differences in the retinal images or in fixation disparity could be the source of differences in perceived distance that accompany differences in binocular parallax (e.g., Foley, 1978, 1980; Ogle, 1962). Was the binocular stimulation by a target at a given distance identical before and after the induction of an esophoric shift? Shebilske et al. did not report any before and after measures of convergence. They reported neither the precautions they took to insure convergence nor measures of the variance in vergence. There is considerable "error" in steady binocular fixation (e.g., Krauskopf, Cornsweet, & Riggs, 1960; St. Cyr & Fender, 1969). A disparity between the two visual axes is responded to by fast and/or slow movements ("flicks" and "drifts") that correct the disparity but do not do so completely. In consequence, a fixation distribution results around the zero disparity. There are suggestions that the pattern of the distribution and the mean fixation disparity depend on the observer and the fixated target (St. Cyr & Fender, 1969). Do these parameters of the binocular fixation pattern also depend on the degree and direction of a phoria shift? Models of the linked behavior of the slow and fast components of the fusional vergence system (e.g., Schor, 1979) suggest that they might. And we are reminded that a disparity as small as 2 s of arc is noticeable for at least some observers (Hochberg, 1971). At all events, a phoria-induced shift in the fixation disparity that is associated with a given fusional stimulus might affect the distance at which it is perceived.

The point to be underscored, one that has been given short shrift, is that a change in the steady state of the oculomotor system (with a concomitant change in its fluctuation pattern) may be necessary to overestimating the distance of things after prolonged near convergence, but it may not be sufficient. To show that the motor conditions of convergence matter, rather than the optical consequences of convergence, requires a manipulation that pits the two against each other, for example, holding vergence angle constant and optically specifying different vergence angles versus varying vergence angle and optically specifying a constant vergence angle. Michaels (1982, November; 1983) performed a manipulation of this kind. The outcome favors the conclusion that perceived distance is tied to the optical consequences, not to the motor consequences, of a vergence posture. (Michaels interprets binocular vision as a rotational transformation on a surface layout, with the amount of rotation specific to the body-scaled distance from the fixated surface and equal to the vergence angle.)

With respect to (b), just as the nonvisual registration of eye position relative to the head drifts in the absence of vision (see previous discussion) so does the nonvisual registration of arm position relative to the body (e.g., Gross, Webb, & Melzak, 1974; Smyth & Marriott, 1982). In the two experiments of Shebilske et al. subjects pointed to the position of the target with the index finger of their unsighted right hand. In Experiment 2 the posttest pointing followed 10 min during which no body parts were visible. Gross et al. (1974) and Smyth and Marriott (1982) report that when a person points with his or her left hand to anatomical landmarks of the concealed right arm (or refers to them verbally), there is a significant difference between the perceived and actual position of the concealed arm. Notably, the hand seems closer to the body than it actually is and is perceived increasingly closer the longer the interval of concealment. Smyth and Marriot (1982) report a difference between pre- and posttests of the order of 5 cm. Shebilske et al. evaluate neither the accuracy of their subjects' registration of hand position and hand displacement in the absence of vision nor the effect of prolonged concealment (the period of esophoria induction) on that accuracy. Relatedly, they do not consider the possibility that the nonvisual registration of the concealed limb's position and displacement is different, and interacts differently with the experimental manipulations, when the occluding surface and the body's relation to it are visible (their full-cue condition) than when they are not (their reduced-cue condition).

Possibility (c) follows straightforwardly from the preceding. Accuracy of nonvisual registration of limb position and magnitude of limb displacement might be affected by eye-muscle potentiation effects *qua* modifications of a postural tuning reflex system (Easton, 1972a; Greene, 1972). Fukuda (1961) reports systematic variation in the magnitude of displacement of the unsighted writing hand with variation in head position. Easton (1971, 1972b) reports that, in the cat, stretching the horizontal eye muscles facilitates a turning of the neck and head from the direction of gaze; and that stretching the vertical eye muscles might enhance forelimb extension (when the eyes look downward) and forelimb flexion (when the eyes look upward). Greene and Ruggles (1973, March) report that if a person tries to point straight ahead with a concealed arm/hand and with the eyes gazing to the left or to the right, the endpoint of the movement tends to be displaced from the midline in the direction opposite to that of the eyes. The reported effect is more reliable with the eyes open than with the eyes closed. We do not know whether such postural tunings are influencing the pointing response in Experiment 2 and in other similar experiments. For example, Shebilske (1981) tilted baseball batters at 45° and had them gaze upwards and backwards for several minutes prior to batting. As a result the batters swung lower at pitched balls than they would normally. Were the batters seeing the ball lower (because of exaggerated efferent signals to counteract upward drift), or were they swinging lower (because of the exaggerated postural tuning of the arms)? A putative control condition in which blindfolded batters swing at remembered target positions will not suffice: Eye muscle tensions during the visual fixation of a target are not duplicated with closed eyes.

Let us now turn to possibilities (d) and (e). It is well known that in experiments that are conducted in the air-theory tradition (of which Shebilske et al.'s might be an instance, as noted) egocentric distance (from target to observer) and exocentric distances (from target to other objects) interact in a complex fashion to determine judged egocentric distance in the air. Further, it is well known that judged egocentric distance in the air is affected by the target's background—even by something as simple as another "target" in the same plane at the same distance but at a different elevation (Foley, 1980; Gogel, 1977). For a *fixed* egocentric distance in the air, judged egocentric distance in the air can be made to vary substantially by manipulating exocentric distance and the target/background configuration (e.g., Foley, 1978; Gogel, 1961, 1964). Shebilske et al. report that in the pre- and posttests a subject faced different walls of the fairly ordinary office (see Shebilske, 1981) in which the experiments were conducted. This means that both the geometric configuration of the surfaces comprising the target's background and the horizontal distances between the target and the other objects differed from pre- to posttest. If previous research on judging the egocentric distance of small luminiscent targets is anything to go by, then the aforementioned differences should make a difference. However, Shebilske et al. report neither how judged egocentric distance was affected by these differences, in and of themselves, nor the details of these differences. If background surfaces and exocentric distances were counterbalanced over the pre- and posttests, then it remains to demonstrate that there was no order effect that could have yielded the 2.3 cm difference in pointing.

Fifth, it is unlikely that any sense can be made of Shebilske et al.'s (1983) suggestions in the opening and final paragraphs that "People may be continuously compensating for errors in registered eye position" and that ". . . the need to cope with minor motor anomalies has shaped both the phylogeny and ontogeny of the *visual* system" (p. 270, our italics). One reading of these statements is that in computing distance, the visual system always takes into account efference to the eye muscles, but because of the frequent infidelity of this efference the visual system has developed (or does develop) adjuncts to its distance algorithm that can correct for the infidelity. However, for the visual system to apply these corrections adaptively, it would have to be apprised of when and to what degree registered eye position is incorrect. The existence of a means by which the visual system could be apprised of such facts would obviate the need to use registered eye position at all—a conclusion contrary to the major thesis of the article. Further, if adaptation is to be invoked (Shebilske et al., p. 277), then surely it makes more sense to claim that evolution and/or learning would significantly prune or eliminate any voluntarily controlled activity that seriously and repeatedly undercut the accuracy and objectivity of visual perception. As remarked, baseball batters perform more poorly if they lie at 45° and gaze upwards prior to batting (Shebilske, 1981). Suppose that the ritual and its consequence were repeated fairly often, what would be more likely—that a baseball batter's visual system would fashion an algorithm with associated set-points to cope with the minor motor anomalies induced by lying at 45° gazing upwards or that baseball batters would learn, more simply, to avoid such odd and debilitating preparatory exercises? The theory implied by Shebilske et al. is full of paradoxes.

To conclude, there is little if anything in the data and arguments of Shebilske et al. to sustain their thesis that the everyday visual perception of absolute distance customarily takes into account the innervation of the oculomotor muscles. As Gibson (1966, 1979) and others (e.g., Kelso, 1979; Mace & Pittenger, 1975; Reed, 1982; Turvey, 1977, 1979) have underscored, there are some fairly profound reasons for rejecting an efferent contribution to visual perception. (By and large, these reasons have to do with the activity-relevant properties of optical flow fields and the requirements for coordinating a system of very many degrees of freedom.) These reasons for rejecting a general efference-mediation view have yet to be addressed seriously by its proponents, such as Shebilske et al.

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