input, simply because the eye movement seems to be independent of any motor command by the organism. Touching the ribs with the fingers in trying to tickle oneself sets up an efferent copy that is relevant to the tactile experience. Re-afference affirms the sensation with the percept of self-initiated touch, and no tickle is perceived. When someone else lightly touches the ribs, there is no efferent copy with the CNS, and the exafferent stimulus is interpreted as tickle.

The above examples illustrate how necessary it is to particularise the relationships between sensory and motor components in perception. A problem with many visuomotor feedback experiments is that they fail to specify those particular motor activities involved in the neurological processing of sensory input that generate specifiable perceptual effects. In a series of experiments Collins has shown that it is essential to specify the relevant muscular stimulation involved in the interpretation of space (Collins 1971; Collins & Lahy 1972; Collins & Lord 1971). Prior to these experiments all sorts of claims concerning motor involvement in judgments of kinesthetic space were being made. Many experimenters (Bakan & Weiler 1963; Moylan 1964; Zacks & Freedman 1963, and many others) used irrelevant muscular stimulation in studying kinesthetic spatial judgments. In much the same way, we feel that it is important to specify the nature of the visuomotor feedback and the processes involved.

The failure to replicate the Held & Rekosh study by Gyr et al. is just another instance of failing to replicate an experiment in this area. In a number of studies (Singer & Day 1966a; 1966b) we have tested Held's active-passive hypothesis relating to prism adaptation and have shown that motor activity is *not* a necessary condition for adaptation to occur. In one of these experiments adaptation was achieved with a perfectly still hand viewed through a wedge prism (Singer & Day 1966c), while S was making verbal judgments of hand position on a scale moved by the experimenter. These data suggest that judgmental activity, but not motor activity, may be a necessary condition for the adaptation process to occur when it is indexed by a motor response.

Gyr et al. recognise that systematic scanning of the visual field may be an important variable in their research. We maintain that this, not walking around in a cylinder, is the most crucial aspect of the experiment – because systematic scanning is likely to introduce judgmental activity. A passive condition – for example, the cylinder moving with S sitting, or someone else moving S about the cylinder—would have helped to sort out the relevance of walking. More specifically, experiments are needed to articulate clearly just what aspects of the motor system interact with visual-sensory processes to produce changes in spatial judgments.

The large literature on adaptation to transformed sensory input makes it clear that the necessary and sufficient condition for adaptation is a discordance between two sources of spatial information. If the change is to occur in a sensory modality and not just to consist of a learned compensatory motor response, then one source of information must be derived via that modality. The other information can come from many different sources, such as from self-produced movement, from the vestibular system and kinesthetic receptors in the neck that signal gravitational information, or from other sensory spatial modalities such as hearing or kinesthesis.

The only form of adaptation that can throw light on Gyr et al.'s question of whether the efferent motor signal is necessary to visual perception is a change in a visual judgment, not a change indexed by any form of motor response. As Gyr et al. note, sensory spatial adaptation of the kind described by Gibson must also be excluded. Of the comparatively few studies concerned with a change in the visual system, some must be excluded, since viewing a laterally-displaced visual field has been reported to produce an oculomotor change in eye position that can also be measured in the post-exposure phase (McLaughlin *et al.* 1966; McLaughlin & Webster 1967). One report of visual change (not confused with oculomotor change) in the absence of any movement comes from Rock (1966 *op. cit.*), who found that 30 minutes of exposure to an optically-reduced image of familiar objects resulted in a significant after-effect no different from that obtained under conditions allowing active manipulation of the objects viewed.

An example of a large change in the visual system that can occur without active motor involvement is the after-effect following exposure to a tilted room (Austin *et al.* 1974). Ss viewed a miniature room tilted 22° and made 10 judgments of the verticality of a bar at the back of the room (but without receiving kinesthetic stimulation from it); they showed a visual after-effect of 4.05° when tested 15 minutes after the exposure period ended. This procedure clearly precludes Gibson's sensory spatial adaptation.

Finally, the suggestion that motor activity has a critical role in visual perception is an extreme interpretation of the sensorimotor theory and one that the Gyr et al. replication does not test. It is also inconsistent with our present knowledge of nervous system functioning. With respect to the question broached by Held & Rekosh as to whether "the subjective geometry of the visual field can be altered by movement-dependent feedback" (1963 *op. cit.* p. 722), the examples given above should suffice to show that there is evidence that, while centrally-monitored motor output may be a sufficient condition for a change in the perceptual process, it is not a necessary one.

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Methodological considerations in replicating Held and Rekosh's perceptual adaptation study. Perhaps the Gyr et al. failure to replicate the H & R result could be due to small differences in method. I would be reluctant to dismiss the H & R finding because of this negative result, especially since one very thorough study has found curvature adaptation beyond that attributable to normalization (Cohen 1965).

Gyr et al. hint at, but do not spell out, what may, I think, be a crucial difference. H & R do not specify the texture density of their "random" field. They also fail to specify the visual angle of the bars in their test grating (Gyr et al. also omit this). We now suspect that the visual system processes not the specific features in a given scene, but rather the spatial frequency content (see Campbell 1974, for a summary). If the visual system is doing a Fourier analysis on input, then common spatial-frequency components between exposure and test conditions may be necessary for the curvature after-effect to be demonstrated. Thus, H & R may have fortuitously hit upon a "random" pattern whose fundamental Fourier components were similar to those of their test grating. Gyr et al.'s exposure and test-stimuli's spatial-frequency components may have been too widely separated for an effect to be generated. (For examples of perceptual after-effects dependent upon spatial frequency, see Anstis 1974, or Ware & Mitchell 1974.)

One additional point: Walking involves some vertical displacement. In the H & R passive condition Ss were wheeled about in a cart. The active condition therefore involved some vertical displacement as well as horizontal translation, and this could provide additional information leading to active-passive differences.

The appropriate experiment remains undone. The basic H & R experiment should be repeated with: 1) movement in the active and passive conditions restricted to pure translation of the head toward the random field, and 2) test and exposure fields having equivalent spatial-frequency content.

NOTE

by **M. T. Turvey**

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The thesis of the efference-mediation of vision cannot be rationalized. I wish to argue that there is neither an evolutionary nor a logical reason for an efference-mediated mechanism of visual perception. If this argument is valid, it follows that any experiment intended to evaluate such a mechanism is evaluating something quite different, and that both classical and contemporary data gathered in reference to the efference-mediation issue will require careful re-examination and re-interpretation.

Optical flow perspective and physical facts at the scale of ecology. Let me identify at the outset a physical fact at the scale of ecology that, by any reasonable account, has held true for the entire course of evolution: When an animal locomotes, its body moves as a unit relative to the surroundings, but the surroundings (say, for a terrestrial animal, the ground plane and the objects that clutter it) never move as a unit relative to the animal. (Earthquakes might appear to be an exception. We could easily rule them out of contention on grounds that their occurrence is too uncommon to pressure the evolution of perceptual mechanisms. But it is more important to note, given the argument that follows, that earthquakes will not structure the light in ways identical to the ways light is structured when an animal moves relative to its surroundings.)

Here is a rough description of the light to the eyes of animals as they move about in their cluttered habitats. The surfaces comprising habitats are opaque and textured. A texture element is tentatively defined as a surface region bounded by a closed curve so that no intensity transitions exist within the region but a discontinuity in intensity does exist at the boundary. For any point of observation in an animal's habitat, the light reflected to that point from the variously-articulating opaque surfaces may be construed as a set of polar projections from the (unoccluded) surface-texture elements onto a projection surface. The polar

projections from a natural surface-texture element onto a mathematically-convenient projection surface can be referred to as an optical-texture element; and the set of those optical-texture elements can be referred to as the optic array (Gibson 1961; 1966 *op. cit.*; Lee 1974; 1976).

When an animal – be it terrestrial, aquatic, or aerial – moves relative to its surroundings, the optic array will undergo a concurrent transformation in all its parts. Thus, if an animal advances rectilinearly in any transparent medium (air, water), there will be an outflow of optical texture (with the center of the outflow specific to the direction of locomotion); if an animal drops from a higher to a lower ledge, there will be a concurrent upflow in all optical-texture elements, and so on. The point to be doubly underscored is this: A global transformation of the optic array is specific to a movement of an observation point relative to the surroundings. This specificity holds for any contemporary animal locomoting in its natural habitat, and it has held, by all reasonable accounts, for any present-day animal's ancestors. Importantly, no *sensible* case can be made for the claim that global transformations of the optic array specify for an animal a unitary movement of the surroundings relative to the animal. For the latter claim would be tantamount to saying that it has been common-place during evolution for the planet earth to move relative to the animals that inhabit it. In contrast, a sensible case can be made for the claim that transformations of parts of the total optic array relative to the total optic array are specific to changes in parts of an animal's surroundings relative to the animal.

No evolutionary motivation for efference compensation. There is therefore no substance to the often-voiced assertion that movement of the animal and movement of the surroundings are signalled by identical stimulation. On the contrary, the two kinds of movement are specified in the circumstances in which animals live and with respect to which they have evolved by very different patterns of visual stimulation. And we see, in short, that there is no evolutionary motivation whatsoever for "mechanisms that compensate for" the visual stimulation produced by movements of the perceiver relative to the surroundings. To be terribly redundant, these efference-mediated mechanisms have been promoted over the years on the assumption that self-movement and environment movement have the same visual consequences, and that an animal therefore needs some special, extravisual mechanism to distinguish its movements from those of its surroundings. But since the optical consequences are not identical and never have been, in the lengthy past of evolution, we should not suppose that nature was pressured into engineering the special brain mechanisms advocated.

The significance of the above line of reasoning—that there is no evolutionary rationale for the mechanism proposed by von Holst (1954 *op. cit.*) and by Sperry (1950 *op. cit.*) – seems to have escaped Gyr and his colleagues. They try to disarm the argument of Gibson (1966 *op. cit.*), that is under elaboration in this commentary, by the feeble conjecture that while global visual transformations may not be equivocal for a fly, they could be equivocal for a human. The conjecture is curious on two counts. First, Gyr et al. offer no logical or evolutionary reason why humans should be special in this regard. The facts of optical flow perspective are commensurate with the facts of physics at the scale of ecology – why should humans compromise them? Second, there is already substantial evidence in the literature contrary to the conjecture. Thus Lee and his co-workers (Lee & Aaronson 1974; Lee & Lishman 1975; Lishman & Lee 1973) have demonstrated repeatedly that in a room whose floor is stationary but whose ceiling and walls can be made to move as a unit, humans perceive themselves to be moving relative to the room when the room is moving relative to them. Moreover, when the room moves rectilinearly with the person, at the same velocity and in the same direction (that is, no global optical outflow accompanies the act of walking forward), the perception tends to be that of not moving relative to the surroundings, although the parts of the body are perceived as moving relative to each other in the manner of walking.

Efference is equivocal. The latter observation coupled with a more simple phenomenon – that of walking on a treadmill – brings home a fact of some considerable importance for the von Holst (1954 *op. cit.*) model and for efference-based models of perception in general. Recall that the raison d'être for von Holst's model is that afferent signals are equivocal on the issue of whether the animal moves relative to the surroundings or the surroundings move relative to the animal; therefore, one appeals to efference to resolve the equivocality. But efference, it can be argued, is itself equivocal; in the case of walking forward in Lee's moving room or on a treadmill, the efference is the same whether or not one is displacing as a whole relative to the surroundings. Presumably, then, advocates of efference-mediated visual perception should propose an additional mechanism, one that appeals to afference – more properly, optical-flow perspective – to

resolve the *efferent* equivocality so as to determine whether or not forward locomotion is taking place!

We ought to note that the treadmill case is not especially exotic. For the bird or insect flying into a wind, the fish swimming upstream, and the primate pushing or pulling a relatively immovable object, patterns of efference in these natural circumstances can be said to equivocate on movement relative to the surroundings, whereas optical flow perspective would be singularly univocal. In sum, the ambient optic array is exproprioispecific (Lee 1978); efference is not.

Visual information for the control of activity rather than response-eliciting stimuli. The arguments above and elsewhere (Turvey 1977a) underscore the absence of sensible reasons for efference-mediation of visual perception. Let us accordingly take another look at the experiments directed at demonstrating its existence.

It is assumed in the experiments of von Holst & Mittelstaedt (1950 *op. cit.*) that a striped drum rotating about an insect triggers a reflex, the optokinetic reflex, that rotates the animal in a direction opposite to that of the rotating stripes. The "stimulus" for the reflex is said to be a difference between the angular velocities of the stripes and the animal. But Varju (1975) has shown that when the insect is allowed to move freely and make normal scanning movements, the insect rotates *with* the rotating stripes at a constant rate rather than lagging behind at some value correlated with detection of slippage. Indeed, an insect rotating with the stripes, and taking one of them as a piece of the surround to keep aligned with, may – if it finds itself falling behind the fixated stripe – make a fast turn backwards *against* the direction of rotation and alter its fixation to a following stripe. On other occasions the insect may make a jump that takes it *ahead* of the rotating stripes. These observations suggest that rotation of the visual array relative to the animal is not a stimulus triggering a response but information about the fact that the animal is being rotated relative to its surroundings, and that the animal behaves in *varied* ways to preserve a fixed relation to its surroundings.

Consider another example. Srinivasan and Bernard (1977) superimposed a pursuable object on a large moving pattern. Normally, for a fly tethered in front of the pattern in such a way as to permit it to fly, the moving pattern would be responded to by wing-amplitude adjustments that right the insect with regard to the coordinates of the surroundings. In the presence of a pursuable object, however, the fly selectively ignores the large moving pattern that normally elicits a classical type of optomotor response until the pursuable object disappears [cf. Henn, Vonèche].

"Rules" for controlling locomotion. There are two points to be made: First, that the optical flow perspective, jointly specifying the surroundings and the self, is information for the control of behavior; and second, that the role that information plays depends on the control "principle" or "rule" in operation at the time. Consider the following as examples of the "rules" for *visually*-controlling locomotion: In order to hold one's position relative to the surroundings, move so as to cancel any global optical transformation; to turn the body toward an object, move so as to produce a rotational optical flow away from the location of the object; to approach an object, move so as to make the optic array flow outwards, with the object at the center of the outflow.

My strong preference is against construing these as rules or commands, invested in the brain or issued from the brain (see Fitch & Turvey 1978; Gibson, in press; and Kupfermann & Weiss: "The Command Neuron Concept" *BBS* 1 (1) 1978); rather, they are *laws of physics at the scale of ecology.* Writing them in sentence form encourages the idea that they are rules to be enforced by an enforcer; but this self-actional interpretation is at best regressive and at worst implies sui generis control (see Bentley 1954; Dewey & Bentley 1949; Shaw & Turvey, in press). What is needed is an interpretation that is considerably more consonant with the scientific enterprise and considerably tougher to come by – namely, that the control "rule" arises within the animal-environment system as a consequence of the mutual constraints of an animal and its environment. I make this remark here because Gyr et al. comment on the "complexity" of the efference-mediated view of perception versus the "simplicity" of the direct-realism view advocated by Gibson and others (such as myself). If, by a complex theory of perception, they mean that it prescribes very involved mental gymnastics to achieve perception, then the contrast is fair. But if by complex they mean the kinds of concepts *required by science* to account for perception and the control of activity without fall-back concepts that intimate unanalyzable internal interpreters, ascribers of meaning, and initiating powers, then I must take issue. The program of a committed (direct) realism is conceptually far more demanding (Shaw & Turvey, in press; Shaw, Turvey, & Mace, in press). And, on the subject of demandingness, it is not relatively easy, as Gyr et al. claim, to observe and

control the variables of vision – not when one is describing light at the scale of ecology in reference to the activity of animals (Lee 1974, 1976) and the perception of environmental events (Shaw & Pittenger 1977).

The fly of von Holst and Mittelstadt. Returning to the fly in the optomotor drum, when the drum is rotated about the fly and the extant control rule is to stay put, the optical flow specifies that the fly is turning relative to its surroundings, and it accordingly trys to rectify matters. When the extant rule is to turn to an object, the optical flow specifies that the animal is turning relative to its surroundings and in the right direction. Thus it keeps turning until aligned with the object.

Now consider the case where the fly's head is rotated 180°. The consequence of rotating the fly's eye would be interpreted by some as consonant with conventional physical optics and the idea of a retinal image, and contrary to the ecological optics promoted in this commentary. This is not the place to repeat the arguments against the retinal-image concept. Those arguments have been given in considerable detail (Bentley 1954; Gibson 1966 *op. cit.*; Turvey 1977), and they are sufficiently strong on independent and broadly-based grounds to suggest that the consequence of rotating the head (or the eyes) is only an apparent anomaly for the ecological-optics perspective. For the present it suffices to point out that a resolution of the anomaly must begin with the fact that the optical structure that the fly's visual system uses is not symmetric under the head-rotation transform (Mace, personal communication). At all events, given that the optical flow perspective under the 180° head-transformation specifies movement *away* from the object, and that the extant control principle is for turning *toward* the object, the fly engages in behavior to oppose the turning away.

On reinterpreting the passive versus active data. Vision is capable of obtaining three kinds of information (Lee 1978): exterospecific information about the layout of surfaces (objects, events), propriospecific information about the layout of parts of the body relative to each other, and expropriospecific information about the body and body parts relative to the layout of surfaces. These three kinds of information hold for vision with prismatic distortion just as they do for normal vision. And the claim is easily ventured that different experimental manipulations (see Kornheiser's review 1976) impinge differently on these three kinds of information. The following also holds for distorted and normal vision: Variation in optical structure reveals nonvariation and permits the distinguishing of styles of change from nonchange. It follows, therefore, that the richer and more varied the natural transformations of optical structure that the observer-wearing-prisms experiences, the greater the opportunity to distinguish the novel transformational invariants specifying styles of change (e.g. observer relative to surroundings, limbs relative to body, objects relative to surroundings and relative to observer) from the novel structural invariants specifying the structures participating in the changes (see Pittenger & Shaw 1975; Turvey 1977). On such an ecological-optics analysis, active-passive is not the dimension of significance for the study of adaptation (cf. Kornheiser 1976). Rather, the concern should be a rigorous (mathematical, experimental) analysis of the various transformations (including the null case) of optical structure arising from movements of the animal or changes in the surroundings and the kinds of information those transformations make available. The latter concern, and that for understanding control rules and how they arise, define well-motivated scientific problems. The proposal for the mediation of vision by efference does not define a well-motivated scientific problem; in my view it is a proposal without any "residual plausibility" (in the words of Gyr et al.) whatsoever.

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Visuomotor feedback: A short supplement to Gyr's journey around a polka-dotted cylinder. My intention is to write a short supplement to Gyr et al.'s study, dealing with two of the points in the paper: 1) the theoretical question as to the necessity of observer movement for perceiving the properties of an object, and 2) the empirical findings under dicussion. I shall propose the embryo of an experiment to test visuomotor feedback in animals and humans.

It seems to me that the French mathematician Henri Poincaré has shown once and for all the *logical* necessity of movement for the existence of object properties such as shape, size, and permanence in space. Without his "group of displacements," such invariants could not be established, and we would all be in the position of Sperry's frog trying to catch our daily flies in the wrong quadrant of the trigonometric circle. There is an American phrase that expresses the same thing more forcefully by alluding to a hole in the ground to be distinguished from

another one located on the observer, but *BBS* is too academic a journal to print anything about academy figures.

Gyr et al.'s subjects seem to demonstrate that such a distinction is not clearly made by human beings under certain conditions, so I would like to submit two pieces of experimental evidence that I have collected that could help us understand Gyr et al.'s results, especially since nonadaptation could be explained in a variety of ways.

In an unpublished experiment run for the Geneva Astronomical Observatory on the question of the existence of star rings in the sky, I showed that astronomers could detect "rings" in a computer-simulated series of "star photographs," in which the location of the various stars was randomly distributed on the photographic surface presented to the Ss (who were *not* Geneva Observatory astronomers). But – and this is the relevant piece of data – the distribution of ring-detectors in the sample of Ss was not significantly different from chance level. This result persuaded us to follow up this point with a larger sample of "naive" Ss (astronomers are not so numerous, and they are very busy), who had to observe random arrays of colored dots of the sort described by Gyr et al. in their experiment. The same results were obtained, with the following additional effect: Ss would find "lines" and "rings" significantly more readily when instructed to "look for regular configurations" or just "regularities."

This last result could indicate that H & R instructed their own subjects in such a way as to induce a certain type of response instead of another, since the organization of randomness into order seems to depend so evidently on directed attention.

The second piece of evidence is briefly mentioned in my book on figural after-effects (Vonèche 1971). In an experiment conducted with David Bearison to separate the Gibson effect from adaptation, we presented a curved line of dots to children (aged 5 to 12 years) wearing prismatic lenses that made the line appear vertical. We expected this procedure to rule out what Gibson calls the "normalization" effect. The results were surprising: the younger children did not show any adaptation, while the older ones displayed adaptation only to the upper part of the "line."

This result tends to show that dotted lines are not perceptually equivalent to continuous ones; hence, when put together randomly, they require highly-trained observers to be perceived as lines, especially since the sequence of dots chosen as "line" dissolves as a function of head movement.

What, then, could be a good test of visuo-motor feedback? It seems to me that it should consist of a procedure allowing for a clear distinction between the optomotor reflex and compensatory movement. The following condition could be critical for this: instead of having just one stationary cylinder, as in H & R's and Gyr et al.'s experiments, I would propose having two concentric cylinders of almost the same diameter (just enough to allow for rotation without friction) of which one, the inner one, would be stationary and half as high as the other, which would be rotating at a constant speed. Both cylinders would be painted with alternating white and black vertical stripes. S would be brought inside the apparatus blindfolded. Once the cylinder was rotating, the blindfold would be removed and S's responses observed. Visually, it is impossible for an observer to distinguish between real and apparent movement inside the cylinders, since the two cylinders seem to move in opposite directions, as far as the mere succession of retinal images is concerned. On the basis of Wapner and Werner's (1957) sensory-tonic theory, my expectation would be that lower animals (and probably brain-injured patients) would exhibit ipsiversive visual tracking of the rotating cylinder, followed by active pursuit of it, whereas normal human adults would track with compensatory movements in the opposite direction. In addition to verbal (when possible) and motor responses, a double check could be provided by separate optometric measurement of S's vestibular, optokinetic, and post-rotatory nystagmus [cf. Henn].

This is only a thought experiment, and things are certainly more involved than this test. The only aim of this contribution is to lay the groundwork for spelling out the necessary and sufficient specifications for a good, crucial experiment of the central problem tackled by Gyr and his collaborators.

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Attentional factors in depth perception. The H & R study is, in my opinion, an excellent one. That they both predicted and obtained adaptation in the random-texture condition was not surprising to me; the experiment was an extremely ingenious demonstration of their own position. What of the Gyr et al. replication?

The studies seem dissimilar enough so that I, for one, would prefer to suspend judgment until others have tried to replicate the study. If the replications fail, and if they do not shed any light on the conditions under which the H & R results appear or do not appear, then we should consider the theoretical implications.

My own position is in disagreement with Held's, in that I feel that some of the developmental phenomena that he feels are acquired through interaction with the environment are unlearned. To this end, in a modified replication of Held & Bauer (1967) we tested monkeys reared without sight of the hands and found little deficit in visual reaching (Walk & Bond 1971). We also reared kittens for a short time in the dark – shorter than did Held & Hein (1963 *op. cit.*) – and found evidence for depth discrimination when the passive and dark-reared kittens were tested (Miller & Walk 1975). Held & Hein (1963 *op. cit.*) had found depth discrimination for active, not passive, kittens. But, except for their position on the early development of visual-motor behavior, I am basically neutral as to the role of re-afference in behavior.

Three experiments that we have performed with kittens (and are still carrying out) are relevant to this issue. The first experiment, referred to above (Miller & Walk, 1975), involved raising kittens under four conditions: 1) normal rearing in the light; 2) dark rearing for 17 days and then active locomotion for 3 hours a day for 10 days; 3) as in Group 2, except the kittens were passive, confined to a holder, able to see the environment, but unable to see their limbs; 4) in the dark until tested at 27 days of age. We found active animals to be similar to passive ones except for the first visual-cliff test. All groups discriminated shallow from deep sides of the visual cliff on a "calling test" in which they were called by the experimenter to come to him.

We next had a closer approximation to the Held & Hein (1963 *op. cit.*) study with the same four groups. The deprived animals remained in the dark for 56 days, then Groups 2 and 3 had ten days of visual exposure, and all groups were tested on the 66th day. These results were almost exactly like those of Held and Hein. The active group was excellent, as was the normally-reared group, while the passively-exposed and the dark-reared kittens were poor on depth discrimination on the visual cliff and when called by the experimenter from the shallow and deep sides.

These results led us to pick a middle period to test for the effects of increased attention on depth discrimination (Walk, Shepherd, & Miller 1978). We had the same four groups and added two "attention" conditions for the passive animals. Kittens were raised in the dark for 40 days before the active and passive groups were given 3 hours a day of visual exposure. The attention groups were, first, a group that watched a visual display and, second, a group that could control forward locomotion. The first, attentional (or "car-watching") group remained in their holders while they watched a toy roller-coaster with small cars that continually circled a track. The other attention group was a passive locomotion (or "go-cart") group; these kittens remained in their holders, but they could lift their heads to close a microswitch that controlled forward motion in a circular path. One animal made as many as 300 revolutions in the 3-hour period.

Our results are preliminary, but the two passive-attention groups seem roughly similar and not different from the active group, while the poorest groups are the regular passive kittens and the dark-reared ones. We hope that additional kittens will continue the same trend.

What might this mean? It would mean that "attention" can maintain depth perception that would be lost without it. The regular passive animals in holders typically seem to close their eyes for long periods; with nothing interesting in the environment they shut it out. Many experiments on prismatic adaptation have appealed to attention as a factor in perceptual-motor adaptation (Kornheiser 1976). Is attention, then, an alternative to self-induced locomotion? Is self-induced locomotion no more than a method for maximizing the attention of the animal? It would be premature to conclude this, even if our results continue to be the same after further testing. In our enthusiasm we had three animals that went 56 days in the dark before receiving additional attentional exposure. These animals were impaired and more similar to passive animals than to active-locomotion animals.

Suppose all of these results hold up? I would then hypothesize that depth discrimination is unlearned, that attentional factors can maintain it for some time, but that self-induced locomotion is needed for its efficient maintenance or recovery after prolonged periods of deprivation. Is this "re-afference" in the Held sense? I do not know, but the re-afference of self-induced locomotion is indeed powerful for recovering or maintaining depth perception.

Despite my disagreement with some interpretations of the Held research, I am indebted to him, as is psychology, in many ways. First, the experiments produced by him and his associates are ingenious, creative, and productive. The best-known research ranges from the prismatic adaptation research with adults to its thematic extension with orphanage children, kittens, and monkeys. Recently, he has produced distinguished research on the visual acuity of young infants with many practical implications. Second, the theory has been productive in terms of the research produced by him as well as by others. A strong theory inspires both refutation and extension, and psychology is the richer because the interaction of theory and experiment has helped us to understand visual-motor behavior. Indefinite theories spark little research. It takes a strong person to stick his neck out, and I feel perception and psychology have been enriched, and will continue to be enriched, both by Held's theory and by his experiments.

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Three functions of motor-sensory feedback in object perception. What is the function of motor-sensory feedback in visual and auditory perception? I shall try to answer this question by discussing three kinds of object perception where motor-sensory feedback is essential.

1. *Processes that bring about the perception of a stable environment.* When we move we produce relative displacements between the environment and our eyes or ears, leading to visual or auditory stimulation that, taken by itself, is indistinguishable from stimulation that might be provided by some objective motion. Such stimulation would, therefore, cause perception of environmental motion were it not accompanied by motor-sensory information representing the body movements that caused the relative displacements. A compensation takes place in which the visual or auditory information about motion is more or less accurately matched up with motor-sensory information, leading to nonperception of movement-produced displacements. We now know of five movement-produced sensory inputs for which such compensation occurs. They are: image displacements due to eye movements (Mack 1970; Mack & Herman 1978); changes in visual direction due to head-turning or nodding (Stratton 1897; Wallach & Kravitz 1965a; Wallach & Bacon 1977); changes in auditory direction due to head-turning (Wallach & Kravitz 1968 in Wallach 1976); rotation of the visual field due to head tilting (Wallach & Bacon, 1976); and rotation of objects one passes when moving forward (Wallach et al. 1974 in Wallach 1976). The accuracy of these compensations has been measured by a method designed by Wallach & Kravitz (1965a). The method consists in adding objective motion that is physically dependent on the subject's movements to the relative motion caused directly by the subject's movements. The proportion of added objective motion needed to cause perception of that objective motion measures the accuracy of the compensation – the precision with which the visual (or auditory) input and the motor-sensory information are being matched up. This proportion was found to vary widely. It is about 20% for eye movements, 3% for head-turning and visual direction, 15% for head-turning and auditory direction, 5% for head-tilting, and about 40% for rotation caused by moving forward.

The variety of these compensatory processes is actually quite limited – namely, to subject-relative displacements – as the following case shows. When one turns one's head, the amount of displacement of a stationary object relative to the eyes also varies with its distance from the eyes. This effect of distance is taken into account in the compensation process as well (Wallach et al. 1972 in Wallach 1976). Of two objects at different distances from the eyes, each one will be seen as stationary during head-turning when it is given singly. But when they are simultaneously visible, one object will be seen to move. Then they are also displaced relative to one another, and this object-relative displacement amounts to a change in configuration. Although the object-relative displacement is caused by one's own movements, there is no compensation; one of the objects is seen to move. While there is compensation for the movement-produced displacement of each object, there is none for the emergent configuration change. This is a striking instance where motor-sensory feedback cannot touch figural perception. It fails to have an effect on the configurational process, although it affects its parts.

2. *Perceptual processes where movements have an information-gathering function.* In order to interest us here, such a process must have two characteristics: that movements elicit sensory inputs unavailable without them, and that motor-sensory information about the movements be needed in the processing of these inputs. I know of only one instance where the second characteristic is certain – namely, auditory localization. Perceived sound directions vary in two spatial dimensions. One dimension, the angles that the sound direction forms with the aural axis, is mediated by time-of-arrival differences at the ears. Information about the other dimension, the angle that the sound direction forms with the horizontal plane, is obtained by head movements. This angle of elevation is given as a quotient between the changing angle representing the time-of-arrival

difference and the changing angle representing the head rotation (Wallach 1940 in Wallach 1976). Elevated perceived sound directions can be produced by displacing a sound source in the horizontal plane dependent on the head–turning. If, with the displacement of the sound source constant, the head rotation is made larger or smaller, the elevation of the sound direction becomes smaller or larger.

I do not believe that it is possible to conclude from this particular case, and others like it which may yet be found, that motor-sensory feedback plays a pervasive role in object perception. Such cases fill specific gaps where stimulation of the pertinent modality alone cannot provide needed information.

3. *Adaptation in visual perception of shape that requires movements on the part of the subject.* In addition to the displacement of the visual direction, wedge prisms cause two major shape distortions: straight lines parallel to the base of the prism become curved, and there is a distortion of distances in the apex-base dimension, in which distances near the apex are lengthened and distances near the base are shortened. As long as the observer remains motionless, these distortions could be properties of the visual environment. Only when the observer moves in such a way that the shape distortions shift relative to the visual environment and cause deformations in the pattern reaching the eyes are the distortions revealed as matters of the viewing conditions.

Wallach & Flaherty (1976) obtained rapid adaptation such that the distance distortions caused by a wedge prism became diminished. During the exposure period Ss wearing base-up or base-down prisms made nodding head movements while looking at a regular pattern of horizontal stripes. After exposure the regular pattern looked distorted, and this effect was measured by compensation. During exposure the nodding head movements caused the prism to tilt up and down, and this tilting transformed the distance distortion caused by the prism into deformations.

It seemed possible that head movements were needed here only to transform the shape distortions into deformations; in that case motor-sensory feedback would have no role in this adaptation. Wallach & Flaherty, however, showed that this is not so. In a variation of their experiment, head-nodding was replaced by having the prism, no longer fixed to S's head, tilt on its own. Although this arrangement caused the same deformations to reach S's eye that were caused by nodding, the adaptative alterations failed to develop. Motor-sensory feedback did play a role in this adaptation.

A corresponding result was obtained by Wallach & Barton (1975), who changed the curvature effect of the prisms into a depth effect, which was much more conspicuous then the curvature. They used the curvature effect to produce retinal disparities, which, in turn, caused a strong concavity to be perceived in patterns located in S's frontal plane. Again, only deformations produced by S's nodding head movements caused adaptation; deformations produced by shifting the pattern relative to the distortion, with S's head stationary, did not. Here, then, are two instances where motor-sensory feedback is essential for shape adaptation.

The experiment by Gyr et al., in which the walking subject was a dot pattern shift through the prism worn base-up, corresponds in most essential features to the successful experiment by Wallach & Flaherty. Gyr et al. probably did not obtain an adaptation because the deformations caused by the curvature distortion were too small. No deformation connected with the curvature effect would occur under the conditions used by Held & Rekosh – that is, from a horizontal displacement between the head and the environment when the prism is worn base-right. I agree with Gyr et al. that no adaptation can be expected to develop under these conditions.

Do the experiments of Wallach & Flaherty and of Wallach & Barton demonstrate that motor-sensory inputs play a role in shape perception as such? I think not. To be sure, their results show that deformation by itself is not sufficient to cause adaptation, and that the deformations must be accompanied by head movements. But the motor-sensory feedback from these movements has no direct bearing on the shape distortions that are eventually altered by adaptation. The feedback is covariant with the deformations, and this covariance apparently initiates the adaptation process. There are no grounds for believing that the feedback has a direct effect on shape perception.

To sum up: motor-sensory feedback has several functions in object perception, but there is no evidence that it directly affects the perception of shape and configuration.

by **Robert B. Welch**
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Separating the issues involved in the role of bodily movement in perception and perceptual-motor coordination. Gyr et al. have used Held & Rekosh's (1963 *op. cit.*) comparison of active and passive movement in adaptation to prismatically-induced curvature, and the current attempt to replicate this experiment, as the context in which to raise three important theoretical questions: a) Can non-optic factors influence or determine visual perception? b) To what extent is motor activity involved in the neonatal development of perception? and c) Is motor activity a necessary condition for visual adaptation to prismatic distortion in adult subjects? Although related, these questions must be addressed separately. This is particularly important here because, as will be seen, it is only the last of these to which H & R's experiment and the present attempted replication are directly relevant.

Non-optic factors in visual perception. As Gyr et al. note, the fact that movement of the visual image across the retina is experienced quite differently if this motion is the the result of active head or eye movement than if it is due to physical motion of the environment, would appear to provide an affirmative answer to the first question. The authors argue that in order to refute Gibson's claim that visual experience can be completely understood in terms of the optic array (the "direct" theory of visual perception), it must be demonstrated that it is the *central* (efferent) component of the motor activity that is crucial in the maintenance of visual stability during bodily movement. This may be an unnecessary requirement, however, since even the demonstration that proprioceptive, vestibular, or other nonvisual inputs can serve this purpose would represent negative evidence for Gibson's theory. Indeed, very recent evidence suggests that neural feedback (afference) may be used to maintain visual stability during active eye movements (Shebilske 1977), contrary to the long-held belief that the extraocular muscles are incapable of signaling eye position.

In any event, it is incorrect to use the results of H & R's study as support for the conclusion that *concurrent* nonoptic factors can affect visual perception. That is, even if it were shown that active bodily movements are necessary for visual adaptation to prismatic curvature, and that the central component is crucial for this process, it would not therefore have been demonstrated that under these circumstances visual perception has a motor component. Gibson (or others) might merely claim that as a result of active motor involvement the observer comes to detect new things about the visual array which result in adaptation. Thus, it is important to distinguish between motor activity as a neccessary *pre-condition* for subsequent visual change and motor activity as a necessary *concomitant* of a unique visual experience. Presumably, only the latter constraint would represent a blow to Gibson's theory.

Motor activity and perceptual development. Studies of adult adaptation to prismatic distortion do not directly illuminate the issue of perceptual development. As Held and his colleagues acknowledged at the very outset of their brilliant series of studies on prism adaptation, the presence of perceptual plasticity in the adult organism is no guarantee that the perceptual capacity in question was originally acquired from experience, or that it was acquired in the same fashion as adaptation in the mature, experienced organism. In short, it is invalid to generalize from adult re-arrangement studies to perceptual development in the neonate. To properly evaluate this question one must examine neonatal organisms, as Held, Hein, and their associates (e.g. Held & Hein 1963 *op. cit.*; Hein, Held, & Gower 1970) have done. Unfortunately, the initially promising results of these studies have been subjected to serious criticism and re-interpretation (e.g. Ganz 1975).

Motor involvement in prism adaptation. Finally, we come to the issue of whether active (versus passive) bodily motion is necessary to produce (partial) adaptation to prismatically-induced curvature in adult observers. Although H & R found support for their prediction that active movement is a necessary condition for curvature adaptation, it has never been apparent how their "re-afference theory" actually applies to visual perception – a mystery that Gyr et al. have not attempted to resolve. Furthermore, it has been demonstrated by Victor (1968 *op. cit.*) that curvature adaptation can be effected by *passive* movement, given certain types of input (e.g., that resulting when the observer moves parallel to prismatically-curved lines). The latter fact suggests that it is not the presence or absence of active bodily movement that is crucial for the occurrence of adaptation, but the availability of unambiguous *information* about the nature of the prismatic distortion. Under some circumstances, as for example the random-dot environment of Held & Rekosh and the present experiment, active movement may provide particularly good information about the distortion. Furthermore, as Gyr et al., along with Victor (1968 *op. cit.*) and Rock (1966 *op. cit.*), have observed, an especially informative combination of bodily movement and visual feedback is provided by locomotion with base-up or base-down prisms.

Parenthetically, it is important to note that the relevant movements are of the entire body or head, but *not* the eyes (as Gyr et al. inadvertently suggest by their use of the term "scanning"). Clearly, unless the prisms are attached to contact

lenses, eye movements will in no way inform the subject that anything is wrong with his vision. This is not to be confused with the point that Gyr et al. raise about the importance of fixating a given spot while walking within the cylinder, a procedure which, although probably not *necessary* for adaptation, might facilitate it.

It is unclear from a comparison of the two studies why it is that Gyr et al. were unable to replicate the H & R results. Unfortunately, their criticism of the latter investigators for failing to use the optimal situation for curvature adaptation is greatly weakened by their own inability to find any adaptation with either this or the "traditional" prism-base orientation.

Clearly, there is a need for further research aimed at delineating the conditions under which adaptation to prismatic curvature will occur. It is conceivable that the advantage of avoiding the Gibson effect by eliminating all straight lines in the visual field is more than offset by the potential disadvantage of not providing the salient information about the nature of the distortion that arises when straight lines are present. Since a few minutes of stationary prism exposure to these lines prior to the adaptation period will produce all of the Gibson effect that will ever occur, pre-exposure measures taken at that time should be free of this effect, and therefore any pre/post shift in apparent curvature should represent an unambiguous measure of "genuine" curvature adaptation.

Authors' Response

by **John Gyr**, **Richmond Willey**, *and* **Adele Henry**

Motor factors in perception

The points of view represented in the commentaries on our attempted replication of the H & R study on motor-sensory feedback and the geometry of visual space fall into three broad categories. There are commentaries that are broadly theoretical (1) and those that are more narrowly focused on the possible reasons why the replication, or experiments on adaptation in general, might produce negative results (2). The former can be further subdivided in terms of those that are supportive of the theoretical position one can ascribe to Held and his coworkers on these matters (1.1) and others that, for a large variety of reasons, are critical of such a position (1.2). All these points, together with still finer subdivisions, will be taken up below.

1. Comments pertaining to the general theory of motor-sensory feedback in visual perception

1.1 Comments supportive of Held's theoretical position: 1.11. Support from logical considerations. Vonèche reminds us that the eminent French mathematician Henri Poincaré (1952, 1958) concluded by logical argument that the delineation of object properties like shape, size, and permanence, presupposes observers who can move around and who know when they have moved. Only under these circumstances, according to Poincaré, can the distinction between changes of position and changes of state (e.g. changes in color) be made. The former are changes that an organism can bring about by its own movements, or for which it can compensate. Starting from this premise, Poincaré brings principles of mathematical group theory to bear on the formalization of the various possible changes of position. This is the same group theory to which Gibson (1966 *op. cit.*) alludes, without perhaps taking into account that his notions of how the organism obtains information about its own movements does not, logically, comprise the whole set of "knowing that one has moved." From a purely logical point of view, at least, such a set might well include efferent information. Our Response will consider whether, in addition to the logical argument, there are psychological and neurophysiological reasons for expanding the set proposed by the theorists of direct visual perception.

1.12. Supportive comments based on psychological considerations. Several authors list a number of perceptual domains in which motor events are thought to play a direct role in the perception of certain properties. (Haber, Ebenholtz, Wallach, and Welch provide a systematic overview.) An example of such a property is position constancy, discussed by Wallach. There is general agreement among these commentators (with the exception of Haber, who appears open on the question) that this constancy occurs because perceivers are able to discriminate environmental displacement caused by their own movement from those caused by movement on the part of the environment. Vonèche proposes a new experiment in this connection which, he thinks, would allow a refined test of the von Holst theory underlying the explanation of many of the above phenomena. It is essentially aimed at ascertaining whether organisms make the crucial distinction, assumed by von Holst's theory, between environmental movements and self-movement, or whether all movement is always interpreted as self-movement, as claimed by Gibson's theory.

To be sure, Gibson (1966 *op. cit.*) has attempted to show how some of these discriminations could be made on the basis of input from the optic array alone, including visual input produced by S's movements and input derived from S's perception of parts of his own body. This will be considered in detail in 1.21. However, as Shebilske notes, the perception of direction has been shown to be accurate without visibility of body parts and without the availability of those structural overlaps of successively-visible portions of the optic array upon which Gibson's theory is based. That is, direction perception is available to a stationary observer, suggesting that it may be based on the direction of gaze.

Additional evidence for position constancy and for the concomitant role played by motor events in the production of this constancy it cited by Bridgeman. He reviews the familiar case of pressing against the eyeball with a finger and seeing the environment move in a direction opposite to the passive rotation of the eyeball. To this demonstration Bridgeman adds his own experiment, in which the eye fixates a point in space while being displaced by a finger. In this case the eye does not move, but the environment is seen as moving in the same direction as that in which the finger presses. Bridgeman points out that there is in this instance no Gibsonian relative-movement cue, as there would be in a tracking test, from which S could glean information about direction of movement. The only events that can explain the resultant perception are motor: the extraocular muscles must resist the pressure of the finger in a direction opposite to that induced by the finger. Thus, he argues, muscle innervation explains the perception.

1.13. Supportive comments based on neurophysiological considerations. Murthy discusses the role of the reticular formation in modulating impulses from both the ganglion cells in the retina, and the stretch receptors in muscle spindles. He thus points to the existence in the CNS of integrated centrifugal control of more peripheral sensory and motor events. From this he adduces the reasonableness, or the likelihood, of concomitant and interconnected motor and sensory processes as a widely applicable principle in visual perception [see also Roland: "Sensory Feedback to the Cerebral Cortex During Voluntary Movement in Man" *BBS* 1(1) 1978].

Taking cognizance of the fact that higher centers in the CNS modulate sensory and motor activity in concert, Murthy suggests a modification of von Holst's *reafferenzprinzip*. Murthy's conclusions about brain organization, it seems to us, were at least implied in the theories of von Holst, Sperry, and Teuber, and they were explicitly assumed in the work of Festinger, Burnham, Ono, & Bamber (1967 *op. cit.*) when these authors argue that *perceptual readiness*, rather than actual sensorimotor activity, is adequate for perceptual adaptation. Arguments similar to Murthy's have also been advanced by Konorski (1967 *op. cit.*) and Pribram (1971), among others.

Other evidence from neurophysiology is contributed by Henn. Agreeing in general with Murthy on the essential impossiblity of separating purely sensory and purely motor processes in brain, Henn considers as "strange" the question asked by many psychologists in