

About Hens and Eggs— Perception and Action, Ecology and Neuroscience: A Reply to Michaels (2000)

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Michaels (2000) suggested that vision for perception can and should be separated from vision for action. Methodological, logical, and empirical grounds for this conclusion are critically discussed. Data are presented that perception and action are not 2 different entities, and therefore, they can neither follow each other (the view that Michaels rejects) nor run in parallel (the view she appears to accept). They are 2 aspects of the same reality of behavioral control. Telling about perception is not perception itself; brains disconnected from an efficient speech apparatus may be able to extract environmental information. What is often referred to as *dissociation* between perception and action is not a fiction; rather, such cases indicate important distinctions between different classes of behavioral control. All classes, however, involve both perception and action.

We must question whether this distinction [between perceiving and doing] is more illusory than real. People perceive in order to move and move in order to perceive. What, then, is movement but a form of perception, a way of knowing the world as well as acting on it? (Thelen, 1995, p. 89)

In her recent review of Milner and Goodale's (1995) book and related data, Michaels (2000) proposed a "reassessment of the core concept of ecological psy-

chology: the relation between perception and action” (p. 241). The central point of her article is the demarcation line that the modern neuropsychology of vision draws between (a) the *dorsal stream* linking the visual cortex to the parietal lobes and, further, to the post- and prerolandic cortex, which is thought to be the basis of the visual control of action, and (b) the *ventral stream* connecting the visual cortex with the inferior temporal lobes, which supposedly provides the ability to consciously perceive visual objects. Michaels discussed arguments from the literature as well as from her own data demonstrating *dissociation* between the factors controlling action versus those controlling verbal reports on perception. She further discussed the model of perception–action interaction represented in the middle of her Figure 2 as “Gibson’s view ... and the theory of direct perception.” Finally, she came to the conclusion that there are different informational properties in the stimulation, some of which serve to build perceptions and others that serve to control actions; these two are, therefore, run in parallel rather than being consecutive.

Although I agree with the Michaels’s (2000) position on many issues I find it important to concentrate on the issues where I do not. In what follows I first address two methodological questions, that is, questions about how this problem should (not) be approached. These two are (a) the notion of “the” versus “an” ecological view on the perception–action controversy and (b) the relevance of neurophysiological data to psychological theories and vice versa. After this, I consider content questions. I argue that neither of the models depicted in Michaels’s Figure 2 is close to reality. Instead, I suggest that perception and actions are two sides of the same thing, a view that is at least 104 years old (Dewey, 1896).

THE ECOLOGICAL APPROACH

Michaels (2000) underscored that her answer was “not to serve as a definite statement of the ecological reaction to Milner and Goodale” (p. 242). This specific statement is surprising because, given the legal freedom of opinion and media in the Western society, it is usually self-evident that nobody claims the ultimate truth in his or her writings. Michaels seems to have another opinion, as she believes that “we should strive for a single, clear, ecological position” that she herself denotes as “the party line.” Accordingly, she uses systematically throughout the text the pronoun *we* when she express her own view. Obviously, she speaks as a representative of the group, although she accepts that some other members of the same group may have a different view.

In contrast, I hold that it is much more important to look for a correct view on the perception–action problem than for a view best compatible with a particular dogma. Whether this view, if correct, should be called *ecological* is less important. Although I believe that my own position agrees with main principles of ecological psychology, I shall not be sore if it does not.

PSYCHOLOGY AND NEUROSCIENCE

Michaels (2000) raised an important question of the relation between the neurophysiological data and behavioral theory. She expressed her irony about psychologists “behaving like poor cousins of neuroscience, waiting for and overvaluing its handouts and ignoring the extent to which neuroscientific research itself can rest on very poor, if tacit, folk psychology” (p. 246). Indeed! It is a pity, however, that this healthy skepticism is not held throughout the text, but rather the topic is shifted to the habitual question of how and what “we” (i.e., psychologists) should learn from the recent developments of neurophysiology.

Mulder et al. (1995) gave an excellent example of a misperceived relation between psychology and neuroscience. Their article, which at first glance seems to present a reductionist view, in fact demonstrates with the maximum clarity the priority of the behavioral theory over physiological findings. In describing the possibilities of the modern functional magnetic resonance imaging (fMRI) technique Mulder et al. formulated a task of fMRI studies as depiction of three main brain systems related to perception, cognition, and action. In doing this, neurophysiology would give a final support for this division. However, to formulate such a task one must be sure a priori that the brain does really entail these three systems.

Not only is the interpretation of the data obtained using modern imaging techniques completely dependent on the implicit psychological conception of the authors, but also the very decision to search for these and not for other data depends on it (Sarter, Bernston, & Cacioppo, 1996). Taking into consideration the huge number of neuronal connections, how could a neuroanatomist plan his or her study without an underlying behavioral theory, which predicts that some two brain structures should be connected? Not long ago the Nobel Prize-crowned ideas of neuron detectors with fixed receptive fields (Hubel & Wiesel, 1968, 1977) were the cornerstone of the neurophysiology of visual system. Now we have learned that these same findings can give rise to completely different accounts of the function of the visual cortex in the hands of scientists who base their research on different psychological theories (Braitenberg & Schüz, 1991; Pribram, 1991). Likewise, during the dominance of Sternberg’s (1969a, 1969b) serial stage model in cognitive psychology, most neurophysiological data based on single-cell recordings were quite compatible with this model, but new data were found (Lecas, Requin, Anger, & Vitton, 1986; Requin, Riehle, & Seal, 1988) as soon as continuous flow models (Eriksen & Schulz, 1979; Miller, 1991) challenged Sternberg’s position.

I do not want to push the pendulum to the other extreme and say that psychology has nothing to get from neuroscience. Neurophysiological data are not only psychology driven, but they can also be used for verification, falsification, and—mainly—specification of psychological models. (The previously mentioned serial-stage model was finally rejected after a number of astute physiological studies of the Champaign group: Coles, Gratton, Bashore, Eriksen, & Donchin, 1985;

Eriksen, Coles, Morris, & O'Hara, 1985; Gratton et al., 1990.) The relation is much more like that between the hen and the egg (I have forgotten which came first) than the notorious soft–hardware dichotomy.

An obvious theoretical weakness of the position “ventral stream for perception, dorsal stream for action” is its implicit definition of perception as talking about what one sees or hears. From this point of view, neither animals and toddlers nor neurological patients with severe disorders of expressive speech are able to perceive. It is ironic that just neurophysiological data demonstrate the absurdity of this definition as they give a means to “read out” brain responses to stimulation independently of the ability of patients to express their perceptions in speech or other overt behaviors. These data indicate that in such conditions as coma (Fischer et al., 1999) and apallic syndrome (Kotchoubey et al., 2001) the brain of some patients is able to extract meaningful features of stimulation. Talking is not perception, it is just a kind of action, although a rather strange one.

GIBSON'S VIEW IS NOT HIS

The traditional ecological view on perception and action, which Michaels (2000) intended to revise, is presented on the middle panel of Figure 2 of her article and is attributed to Gibson. Perception is conceived of as parallel to and independent from sensation, and action is regarded as the stage following perception. I would be very grateful for an exact reference to a text where Gibson expressed this view, but as long as such reference is lacking, I continue to doubt that he ever thought so.¹ It is easy to see that perception and action on this scheme are nothing but stages of information processing, the notion from the cognitivist glossary. The only difference from cognitivism is that processing stages are avoided within perception; however, they are permitted within the perception–action cycle. Banished through the door, they came back through the window.

Indeed, Gibson (1966b, 1979/1986) has assumed that sensation is not the first stage of perception but that perception and sensation are largely independent from each other. He also thought that perception and action are, in contrast to perception and sensation, intimately related to each other. From these two postulates, it is only one step to the position that perception is the first stage of action. I believe that it is a wrong step.

In fact, the claim that perception is necessary for action, that perception serves for action, can imply at least two completely different ideas. One of them will be shared by most (cognitive) psychologists and neuroscientists. Perception is obtaining (and, possibly, processing) of information that will be used in organization of

¹All references to Gibson in Michaels's (2000) article concern his view on the relation between perception and sensation, not on that between perception and action, which is the point here.

action. From this point of view, perception precedes action. The former is the necessary precondition for the latter. One first perceives, and then one acts.

The stimulus identification module ... generates a stimulus vector that is passed on to the second module, the response production module. The stimulus vector consists of all the stimulus attributes or features encoded by the stimulus identification module. (Kornblum & Lee, 1995, p. 856)

One cannot negate that this is a computationalist position. This is the computers' modus operandi: to receive information; to process it; and to develop, on its basis, signals for effectors. Van Gelder (1997) imagined how Watt's steam controller would work if it were a computer: It would measure steam pressure ("perception"), calculate the difference between this actual and the optimal pressure ("cognition"), and generate a command to open or close the valve a little depending on the sign and the magnitude of this difference ("action").

Van Gelder's (1997) main thesis is, however, that Watt's controller was not a computer and did not work in this manner. Instead, it arranged the mechanical forces in such a way as to open the valve when too much steam was coming out and to close it when it was too little. In this work, no separate stages of perception and action can be picked out, one of which would precede or follow the other. The information (in the precise ecological sense of this word) necessary to control steam pressure is picked up, surely, but this happens within the same process of control.

This is, basically, what Dewey (1896) called *sensorimotor coordination*. When one sees a light and turns his or her gaze and head toward it, it is not the perception of the light that leads to the action, neither is it the directing action that leads to changing perception. It is *while* the head and the gaze are being moved toward the light that its perception changes. Perception and action are here regarded not as two different entities (processing stages) but as different aspects of the same entity, that is, the control of behavior.

Again, it is just brain physiology that provides one with a means to test this hypothesis. In reaction time (RT) tasks, the latency of the P3 wave and the lateralized readiness potential (LRP) are considered as indicators of stimulus- and response-related processes, respectively (Coles, 1989; Donchin & Coles, 1988; Gehring, Gratton, Coles, & Donchin, 1992; Kotchoubey, Wascher, & Verleger, 1997; Magliero, Bashore, Coles, & Donchin, 1984). If perception and action are two consecutive stages, then any RT delay with task difficulty can be due to either difficulties in stimulus perception (then the P3 latency should increase in about the same extent as the RT) or due to a conflict between incompatible response tendencies (then, if two hands are used for responding, a polarity inversion of the LRP is expected), or both. In any case, if the two measures are really related to two different classes of processes, as cognitive psychology and (following Michaels, 2000) ecological psychology assume, the impact of these two should be inversely correlated. Indeed, the larger the portion of an RT delay is explained by difficulties at

the perceptual level, the less remains to be explained by problems in the organization of action. Hence, changes in the P3 latency and the LRP together should explain more of the RT variance than any of these two variables alone. On the other hand, if the P3 latency or the LRP alone explain so much in the RT variance as both variables together, the logical conclusion should be that stimulus- and response-related processes are, basically, the same! This conclusion will be even stronger if, for example, the P3 latency variance explains the whole RT variance, because in this case there will be no room at all for the effect of response-related factors.

In 1997 I conducted a meta-analysis of the literature published until the end of 1996 about the relations among RT, the P3 latency, and the LRP (Kotchoubey, 1998). Recently I repeated the analysis with the data published until the spring of 1999, and the main result was the same (see Figure 1). A P3 delay as an index of stimulus-related brain processes, and a polarity inversion of the LRP as an index of response conflict, usually occur together. The larger the P3 delay, the more expressed are changes in the LRP, too: The two measures are thus directly correlated. Moreover, in many cases the P3 delay is equal to or even larger than the corresponding RT delay; nevertheless, LRP inversion is observed over and above this. I propose to call this phenomenon *overexplanation*: The P3 delay has already explained the RT delay, and the problems of action organization reflected in the LRP should yield an additional increment of RT.

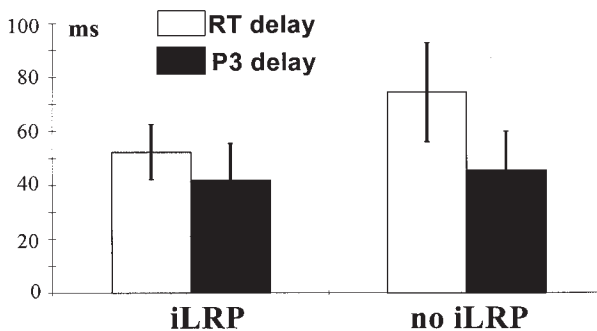


FIGURE 1 Delay in overt response time (light gray columns) and in P3 wave peak latency (dark gray columns) due to increasing task difficulty: means and standard errors across 42 data sets taken from 17 studies in which reaction time (RT), P3, and the lateralized readiness potential (LRP) were recorded. The relation between the delays of RT and P3 indicates the extent to which the RT delay is explained by stimulus-related factors alone. The presence of an LRP with inverted polarity (iLRP) indicates a response conflict. Were stimulus- and response-related processes two different classes of processes (i.e., two sources of the RT variability), the P3 delay would explain a smaller portion of the RT delay in the 19 data sets where the iLRP was found, because a part of the RT delay is explained by response-related problems. The opposite relation is observed, however, which is in line with the idea that stimulus- and response-related processes are but two aspects of the same class of brain processes.

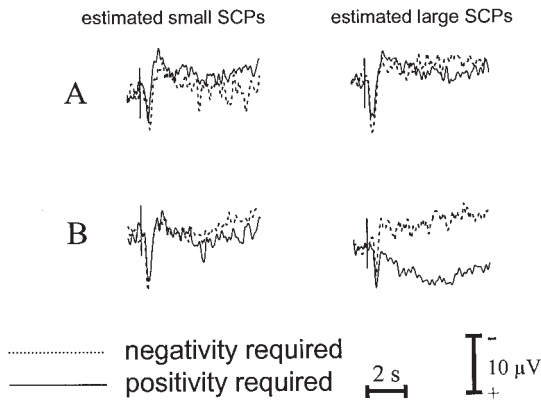


FIGURE 2 Slow cortical potential (SCP) shifts during the 30th training session in patients who were poor (A) or good (B) in SCP control. The latter group demonstrated very large SCP shifts of the required polarity on trials the patients estimated as “large” and small shifts on trials they estimated as “small.” Significant interactions were Required Task \times Estimation, $F(1, 21) = 12.05$, $p = .002$, and Group \times Required Task \times Estimation, $F(2, 19) = 3.65$, $p = .046$. The third group (medium quality of control) is omitted. Bars indicate the beginning of the trial.

If one, following these results, understands perception not as an entity separate from that of action but as a kind or aspect of behavioral control, then it should be possible to create new forms of perception on the basis of new kinds of behavior control. To test this hypothesis we (Kotchoubey, Kübler, Strehl, & Birbaumer, 2001) analyzed the data about self-estimation of slow cortical potential (SCP) shifts in epilepsy patients who had previously learned during 30 training sessions to voluntarily control these shifts. SCPs are low-frequency components of the electroencephalogram that have been shown to be controllable (Birbaumer, Elbert, Canavon, & Rockstroh, 1990). In fact, the patients were able to estimate the magnitude of the SCPs significantly above chance level (see Figure 2). Note that in contrast to the issue of perception of bodily functions, such as heart rate or gut motility (Jones, 1994; Vaitl, 1996), there are no receptors for brain electrical processes such as SCPs. Therefore, perception of these processes cannot be related to any sensation. The quality of SCP perception was correlated with the quality of SCP control and seemed to be secondary with respect to that: Above-chance SCP control in epilepsy patients was observed as early as after 5 to 15 training sessions, but above-chance SCP perception required 30 sessions. Additional studies showed that this perceptual activity resulted neither from cognitive strategies (such as vigilance monitoring) nor from perception of peripheral correlates of central brain processes (such as muscle tone). Supposedly, what patients were perceiving was their goal-directed activity to control brain waves: their inner actions.

DISSOCIATIONS

If perception and action are not two different entities or classes of processes but two aspects of, two views on, or two modes of analysis of the same class of processes of behavioral control, then they can neither follow each other, as in the middle panel of Michaels's (2000) Figure 2, nor run in parallel as in the bottom panel of the figure (see Gottsdanker & Tietz, 1992, for another proof of the same thesis). However, why do the psychological and neurological data listed in Michaels so convincingly demonstrate numerous dissociations between these two (from my point of view, absolutely unseparable) processes?

The answer contains nothing mysterious, and in fact, Michaels (2000) gave this simple answer on pages 252 to 253 and then again on pages 254 to 255. Separation of perception and action appears to be a big discovery, but there is nothing more natural than the fact that different classes of actions (or, more correctly, classes of sensorimotor coordination) are organized by different brain systems and affected by different experimental variables. Michaels pointed at the most important differences between the coordinations that are usually qualified as *actions* (such as grasping, jumping, or pressing a key) and those denoted as *perception* (such as recognition of a familiar object or telling the experimenter about one's visual impressions during an experiment). The former are short living, often highly trained activities related to the immediately presented and available environmental variables; many, although largely not all, of these activities are common for humans and their close animal relatives, that is, apes. The latter are related to the stimulation presented long ago or to stimulation yet to be presented (Gibson, 1966a). The former are controlled by continuously changing information variables such as tau (Lee, 1976); the latter are dependent on those invariants that are, to a large extent, not a function of time: Constancy is the basic feature of perception. To catch a ball, I have to behave differently from how I behaved a second ago: If I performed the same movement synergy a second ago, I already missed it. However, in order to recognize it as the same ball I played with last summer, I have to extract long-living information parameters. As Michaels precisely indicated, telling as such, that is, speech production (in native speakers) does not differ very much from typical *actions*, but some aspects of the content of communication abstracted from the immediate control of behavior are called *perception*.

The ventral–dorsal division is not the first clear-cut separation of perception and action. Let us return to good old visuomotor tasks with incomplete stimulus–response compatibility (i.e., tasks that ecological psychologists term *ecologically invalid*), in which participants perform a hand movement to a visual stimulus. Divorcing perception from action in such tasks is what RT psychologists have been doing for decades. If visual stimuli are masked or tilted, perception, but not action, is complicated, and if a complex pattern of finger movements is required, then action, but not perception, is complicated. The secret of this trick is, however, clear. It is not perception and action that are separated, but processes of eye control are

separated from those of hand control. Again, different classes and levels of control processes can be distinguished, and these distinctions may be of much importance, but they are not the sought distinction between perception and action.

The very informative footnote 3 in Michaels's (2000) article illustrated this statement. Stins and Michaels (2000) found different compatibility effects for button press versus touching movement. This interesting finding would be interpreted in any other context as a fine discrimination between two types of actions. However, being animated with the idea to finally disentangle perception and action, Michaels redefined button press as perception—a definition that will hardly be shared by many perception psychologists. A way of steady redefinition of basic concepts can, of course, always lead to the *quod erat demonstrandum*, but this is a slippery way.

Neither psychological experiments nor description of neuropsychological cases bring any evidence for dissociation between perception and action, although both kinds of data are very useful with respect to differentiation of action types. Let us be grateful for the discovery of dorsal and ventral streams, for this indicated a principal distinction between two different types of behavioral control; however, interpreting this distinction as that between perception and action is more than questionable and requires *ad hoc* definitions that are continuously adjusted to meet the data.

PERCEPTION FOR ACTION AND VICE VERSA

I suspect that Michaels's (2000) main problem is rooted in her decision to bracket out the so called exploratory actions. These actions are “activities that aim to reveal information that can help guide one to a goal,” in contrast to performatory actions that “bring one closer to a goal” (p. 245). Hardening this distinction may cause big difficulties.

The question is, how can one know that one is closer to a goal? Only information obtained from the environment can be the basis of such a conclusion. This was the idea introduced, independently from each other, by Bernstein (1967) and Powers (1973): The notion of behavioral control implies control of the input, not of the output. One does not control actions as such, because one is not interested in actions as such. One is interested only in their results, that is, the information whether one is now closer to a goal. Whether this information is consciously perceived depends on many factors, particularly on the definition of consciousness, and thus this question does not appear to be important. What is important is the close similarity between explorative and performatory actions. The former are aimed to pick up information that can help one to find a way to a goal, and the latter are aimed to pick up information that one is, really, closer to (or farther from) the goal. Both cases invert the habitual “information for action” conceptualization. Information is not a maid for action.

In “A note on affordances,” Michaels (2000) indicated the very point of the problem: “Two things cannot couple if one is part of the other” (p. 254). For the same reason, they cannot be decoupled, either. However, even the whole–part paradigm is not an adequate description of the relation between perception and action: Both are included in each other, and each of them may be regarded as “primary” or “secondary” dependent on the actual point of view. One can try to separate the hen from the egg in the attempt to solve the eternal question of their relative precedence, but in this business one risks losing both and starving.

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