Toeing the Party Line is Getting Harder:
In Response to Michaels et al.'s (2001) Reply to Commentators

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This response will first focus on Michaels, Withagen, Jacobs, Zaal, and Bongers (2001) direct reply to my commentary, both to the comments that they made and also to the points that I had made but they chose to ignore. I will end with a few general responses to their Reply as a whole.

Michaels et al. voiced great displeasure at my ascribing the functions of recognition and identification to an indirect-ventral system. As they noted in their conclusions "Ecological psychology has always offered a different (and, of course, we think better) way to approach the problems of identification and recognition" (p. 242). What is that approach? Michaels et al. (2001) did not offer an adequate answer to this question, other than: "In the ecological version, perceiving properties of objects and events is detecting information that specifies those properties." That sentence does not explain or deal adequately with recognition and identification. First, "perceiving properties of objects and events" is not the same as recognition and identification. It sounds much more like perceiving affordances. One can perceive the size of a ball and pick up its graspableness, or one can perceive the trajectory of a baseball that has just been hit (an event) and pick up the correct point of contact for catching it. But recognition and identification are something more than the perception of properties. Recognizing someone is not simply picking up the properties of that person's face, but being able to relate those properties to some stored information about past encounters with that face. It is not enough to perceive properties of objects or events to recognize them.

The second part of that sentence, "detecting information that specifies those properties", is also problematic. First, I would opt for Gibson's (1986) later usage of "picking up" information rather than "detecting" information. But this is a minor point and the phrase "picking up information that specifies those properties" really tells us nothing about how identification transpires. How does a person identify a car as a Ford Taurus? It's not enough to say that she picks up the information that specifies that Ford. She might be shown a car of a make that she never saw before, and while she picks up the information that specifies that car and its various affordances, she is not able to identify it at all. Recognition and identification are more than mere picking up of information; they must entail some sort of comparison with stored information. I would contend that the picking up (or detection) of information is a part of the perceptual process that can serve the dorsal system in the direct perception of the affordances of objects or events. However, that pickup is not the whole story as far as recognition and identification are concerned; an additional comparison with some form of stored information is needed.

Michaels et al. take me to task for believing "that the need for a representational theory is so self-evident that no serious scientist could doubt it". (p.237) I did not make nor intended such a sweeping statement. All I said was that "recognition must of necessity be based on some sort of internal representation". Not only is the label "serious scientist" rather nebulous, but the term "representational theory" calls forth a long history of philosophical debate far beyond that I wanted to connote. All that I am claiming is that the processes of recognition and identification require a referral to

some stored information. I would concede here that I probably was not sensitive enough to the excess connotational baggage that the term “representation” carries with it. It connotes to some (including most ecological psychologists) a sort of image or picture in the head notion, a re-presentation of the object in question in the brain. My sensitivity to this interpretation was numbed by the excessive use of “representation” in the literature in the context of stored information, and I should have been more sensitive to its broader connotations and refrained from using it.

They also suggest that I attempt “to make representational theory more palatable” by “devolving the conceptual and theoretical issues of representationalism onto connectionist networks” (p. 237). I am afraid that I cannot take the credit for that “devolving”, the credit goes to Hatfield (1990) for evolving and developing the idea that a connectionist model can bridge the gap between the constructivist and ecological approaches. Michaels et al. state that they generally agree with Hatfield’s approach, but that the debate is over how to “characterize the function of the connected network, for example, as becoming better able to detect information vs. as doing computations and making representations” (p. 237). Obviously, they are on the “detect information” side of that debate, but is it not rather strange to suggest that a connectionist model that runs on a computer does not “do computations”. It obviously does, but the equation of “computational” with “constructivist” is misguided. As Epstein (1980) and Hatfield (1990) have pointed out, both theoretical approaches, the ecological and the constructivist, can be seen as computational, the differences between them lie in what type of information those computations process. In the constructivist approach the computations are on information beyond that found in the direct sensory stimulation while in the ecological approach they are limited to only the information in the stimulation. An example of an ecological computation might be that of the invariant tau, said to be used in the perception of time to contact.

Michaels et al. contend that: “A connectionist network does not have two simultaneous (or successive) entities whose characteristics are compared” (p. 238). This is certainly not true when the network is in the process of learning when its outputs are continuously compared with some criterion, but might be said of what occurs during recognition, especially if “entities” above refers to some “image in the head” conceptualization (e.g., template, schema, etc). Clearly a connectionist representation is something very different, some pattern of activations distributed over a set of nodes. But there must be some homomorphic relation between the specific distribution of activations and the given stimulus input that allows recognition, and it is that distribution of activations which can be seen as a means of storing information. If Michaels et al. can abide by such a conceptualization of stored information, then perhaps we have no bone to pick. I am certainly willing to ascribe to a connectionist network mode of storing information as a viable mechanism for maintaining stored information, and if that is not problematic for Michaels et al. then we have reached one point of agreement.

In my commentary I mentioned a few results from the recent paper by Runeson, Juslin, and Olsson (2000). Michaels et al. saw this as a “gambit” on my part for “citing evidence of indirect perception as presented by an ecological psychologist” (p.237). While I am fully aware that Runeson is a prominent adherent of the ecological approach, this was not my intention. I simply was very impressed with the findings of that study, and would have quoted it all the same if Irvin Rock or Richard Gregory had authored it. It was not the denomination of the author that intrigued me, but the findings that indicated a transition from indirect-ventral processing to direct-dorsal processing with practice.

Michaels et al. (2001) offer two perspectives on the Runeson et al. (2000) findings. The second, the one they prefer, is that "addressing inferential processing opens the possibility of finding its proper place in cognitive science". They add: "There is no doubt that people make inferences; the ecological thesis is simply that inferences are based on perception, not the other way around" (p. 237). I find this sentence troublesome in its usage of the term "inference". While Helmholtz used the term "unconscious inference" to describe perception and modern day constructivists adopted the term (e.g., Rock, 1977), this usage differs from the thought processes labeled "inferences". There are undoubtedly inferences that are based on perception, but those kinds of inferences are usually slow processes. As I pointed out (Norman, in press), there is evidence for quick indirect processes in perception. What is more, there is something I do not understand in Michaels' et al.'s response to the Runeson et al. findings. On the one hand they admit that in that study there is "evidence of an inferential mode of processing which with practice transfers to a direct mode" (p. 237), but on the other they claim that perception precedes inferences. So what sort of perception precedes inference in the inferential mode? It cannot be direct perception, as that only appears after practice, so it would seem to be indirect perception.

Their first perspective on the Runeson et al. findings "is that it is the beginning of the end; indirect processing has its foot in the door and will soon crowd out direct processing, indeed move it to the back of the cortical bus, the dorsal stream" (p. 237). This sentence, and several others, point to the existence of some anxiety stemming from the notion that coexistence with constructivism will be the demise of the ecological approach. It would seem that the "defense mechanism" they have adopted is one of extremely dogmatic and stringent adherence to a very single-minded faith in the motto: "No matter what, we will only accept the direct approach". This, sadly, indicates that I was not at all successful in putting my message across in my commentary, at least as far as Michaels et al. are concerned. As indicated by "Not to worry" in my title, my commentary tried to convey the idea that the direct-dorsal aspect of perception is of utmost importance, that most of our perceptual activities fall under its aegis, and that not being able to deal with tasks like recognition and identification does not mean that holders of the ecological view will be out of work or ostracized.

This "blind faith" appears to have left Michaels et al. insensitive to the many signs out there. Michaels' own research and that of many of her ecological colleagues deals in the main with the relations between perception and action. This is not fortuitous as the topics that they study are in the domain of dorsal system functions. I would also suggest that it is not fortuitous that the titles of the first twelve volumes of the journal *Ecological Psychology* contain the words "recognition" and "identification" only once, and that has to do with "self-recognition in infants". What is more, at the end of their Reply, Michaels et al (2001) state: "The goal of (what we would consider) hard-core ecological psychology is nothing less than to promote and to develop a single natural science that can handle intentional and informed movements" (p.240). Are recognition and identification "movements"? I would suggest that they are not. But movements are clearly under the influence of dorsal system pickup of information in the ambient environment, although I believe that this is not the sole function of the dorsal system.

Michaels et al. divide perception into "awareness-of-environmental-properties" and "action". They then add: "The distinction to be drawn is not about awareness (or consciousness), but what awareness is of. In both awareness-of-environmental-properties and in action there is a demonstrable awareness. Whether awareness is conscious in either or both cases is a peripheral issue from our perspective …." (p.

229). A bit further on they suggest that I only picked up one of those two meanings: “He had missed the definition of perception as the detection of information when he wrote ‘ … there cannot be action without perception …’ Michaels’ [2000] intended point was that the movement might be initiated without awareness of an environmental property.” Unhappily, I still do not understand this bifurcation of perception or its underlying logic. What is the detection of information and how does it differ from the perception of environmental properties? Presuming that the distinction between sensation and perception is viable, is “detection of information” at the level of sensations? The answer must be negative as Michaels et al. begin their discussion of the topic by referring to Gibson’s (1963/82) distinction between sensation and perception, and then go on to further divide perception into these two subcategories. In his last book Gibson (1986) preferred “pickup of information” to “detection of information” and one might interpret this choice to indicate his wish to conceive of perception as something that occurs over time and not something that transpires in a flash. Perhaps Michaels and her colleagues feel that in the case of action the pickup of information is very rapid and therefore it is preferable to use the label “detection”. But in what way is this detection different from awareness-of-environmental-properties? Michaels (2000) suggested that stimulation could lead directly to action without a need for perception interceding, leading me to make the comment quoted above. Now it would seem Michaels et al. are revising that claim by stating that “detection of information” is a second form of perception. But what advantage accrues from this division of perception into two types? Cannot a falling ball (to be punched) be an environmental property or event, albeit a quick one? What does it mean to say that “the movement might be initiated or modulated without awareness of an environmental property”? What was it then that brought about the movement, some mysterious entity labeled “information” and detected by the second type of perception. That information is none other than a brief environmental event.

Michaels et al. (2001) did not respond to my pointing out that Michaels (2000) adopted the Milner and Goodale (1995) distinction between “vision for perception” and “vision for action”, but this is not really very important, and I would guess that Michaels might agree that some the wordings in her first commentary were not commensurate with her general theoretical viewpoint. They also did not comment on Gibson’s (1976) *Purple Peril* that I quoted, which I saw as indicating that Gibson was aware of the two types of perceptual processes, but chose to focus on only one, that labeled the dorsal system here. This I find disconcerting since Michaels (2000) model is built upon Gibsonian theory, and we all see Gibson as the most important contributor to our understanding of perception. Happily, the latter is a clear point of agreement between us.

Finally, Michaels et al. paid no heed to my claim that the dual process approach provides a clear answer to the question of whether the perception of affordances is carried out by the dorsal or ventral system. As I explained, the pickup of affordances is the prime activity of the dorsal system. My view (see Norman, in press) is that the concept of affordances is an extremely important contribution of Gibson’s, but that at times he overstated his case by attributing affordances inappropriately. A postbox does not afford letter mailing; the slot in the postbox affords inserting an object of a given size. Gibson stated that affordances are perceived directly and in my view only what Neisser (1989) labeled "physical affordances" (see Palmer, 1999, p. 411) are perceived directly. These are only the functional properties of objects and not their "meanings". Some of the views concerning affordances in the Michaels et al. Reply are totally inconsistent with this view. “The perception of affordances could be said to be the means by which goals and means are selected” (p. 236). Or, “the perception of affordances is more related to making decisions about what to do in a situation and perhaps how to do it…” (p. 236). Both these statements are completely

incommensurate with my view, and hard to reconcile with Gibson's statement that affordances are perceived directly. "Making decisions" and perceiving affordances transpire at very different levels.

Michaels et al. also raised the question of the "size" ("scope" might be more appropriate) of affordances. This question can be analyzed at two levels. The first level has to do with complex or "nested" actions, such as brushing one's teeth. As they rightly point out, brushing one's teeth consists of a sequence of actions rather than a single action. Can we say that one directly perceives the affordance of the toothbrush for brushing (and all the nested actions)? Or, going to even more complex sequences of actions, can one say that one directly perceives the affordance of a car for driving? What are the limitations of the scope of affordances? Once again, my claim is that affordances are perceived directly by the dorsal system. But how can we determine just what the dorsal system is capable of picking up? One possible approach is looking at neuropsychological studies on patients with damaged ventral systems. Several such studies have appeared recently attempting to determine just what actions patients suffering from visual form agnosia or from semantic dementia can perform. For example, Hodges, Spatt, and Patterson (1999) tested two patients with bilateral temporal lobe (ventral system) atrophy, suffering from semantic dementia. These patients were unable to name twenty familiar objects and also performed poorly when asked to demonstrate their use. In those cases where the patient did not succeed in demonstrating the objects true use "he demonstrated a use that was incorrect but largely compatible with the object's physical properties (e.g., he carefully removed each match from the matchbox, commenting that they looked like "little pencils" and holding them as if to write…..)." In contrast, these two subjects performed flawlessly on a "novel tool task" in which they had to "select the appropriate one of three novel tools for lifting a wooden cylinder (with a special feature matched to the appropriate tool) out of a socket." In other words, they were able to pick up the affordance of the novel tool as appropriate for the task at hand. In a subsequent study by this group of researchers (Hodges, Bozeat, Ralph, Patterson, & Spatt, 2000) they end the abstract with: "The results suggest that object use is heavily dependent upon object-specific conceptual knowledge, supplemented to some degree by a combination of visual affordances and mechanical problem solving." Noteworthy is the fact that no mention is made of Gibson in this paper, indicating that the concept of affordances has been assimilated into psychological thinking to such an extent that it needs no referencing!

The second question deals with an attempt to broaden the scope of affordances to encompass a broad range of psychological processes. My initial reading of Michaels (2000) comment about academia affording career building was that she was being humorous. In the Reply they chide me for doing so and let me understand that the idea is a serious one. This extreme broadening of the scope of affordances is, in my opinion, taking Gibson's brilliant idea and voiding it of all its impact. In fact, I see this attempt as symptomatic of what I find to be a serious fault in Michaels (2000) approach, reiterated in the Michaels et al. Reply. They clearly adhere to the idea of encompassing all of psychology in a direct theory built upon Gibsonian theory but going far beyond the perceptual phenomena that interested Gibson. While this in its own right is a very worthwhile effort and has produced important new insights, it is the attitude voiced by Michaels and her colleagues that I find distressing. One gets the impression that theirs is not a scientific endeavor but a religious revelation. Any one who dares come out with suggestions to the effect that there might exist explanations for phenomena not commensurate with the Michaels et al. understanding of ecological psychology is impolitely chided. Science is not characterized by dogmatic and ultimate answers, but by slow accumulation of empirical results that lessen the existing equivocality. That equivocality is never

completely eliminated, only diminished, and there is no room in science for dogmatic and inflexible approaches.

Early in their response to Pickering's essay Michaels et al. note that "We, too, have been disappointed that ecological psychology has not assumed a more central position in cognitive science" (p. 238). Agreeing with Pickering, I would contend that this is due in part to the dogmatic and quite narrow-minded stance taken by Michaels (2000). Being a very great admirer of Gibson's ideas and those of many of his followers, but also cognizant of phenomena that are difficult to explain in the Gibsonian framework, I tried to suggest a means of reconciling the ecological and constructivists approaches (Norman, in press). That effort might be completely or partly incorrect, but its negation should be based on contrary empirical data and not on dogmatic statements based on what are claimed to be infallible beliefs. In her original essay Michaels (2000, p. 242) wrote of an ecological party line and stated that she believed that "we should strive for a single, clear, ecological position …". As I wrote in the title of this response, my feeling is that it is getting harder and harder to toe that party line.

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