# trends in interdisciplinary studies

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TRENDS IN INTERDISCIPLINARY STUDIES

# AVANT

## The Journal of the Philosophical-Interdisciplinary Vanguard

# AVANT

## Pismo awangardy filozoficzno-naukowej

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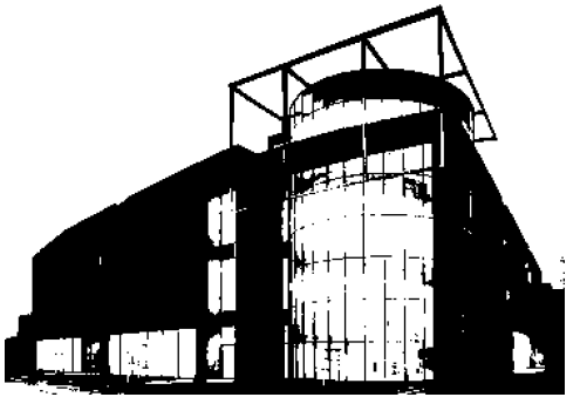
CONFERENCE

*"Thinking with Hands, Eyes and Things"*

**8-10 November 2013, Torun, Poland**



# PREFACE

AVANT, Vol. III, No. 2/2012
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# Preface

Dear Readers,

we are pleased to present this year's third issue of AVANT. The leading theme of the issue is the contemporary status of the concept of affordances: their significance and scope as well as the controversies related to them. Hovewer, we did not aim to provide a general, uniform evaluation, nor to prepare a complete current guide of a kind. The actual status of the studies on affordances is presented through reviews, comments and analyses by selected researches: from the esteemed ones – such as Michael T. Turvey, Anthony P. Chemero or Alan Costall – to the others who have not made their names known in the field yet.

The Embodied Interwiews with Ezequiel Di Paolo and Hanne De Jaegher as well as Frederique de Vignemont constitute a reference to the theme above, placed in a wider context. As a supplement, we also present selected book reviews.

The section devoted to studies on musical practice has also been visited by the ubiquitous concept of affordances. Hovewer, it also includes – a typical for AVANT – counterpoint: interviews with two gifted vanguard musicians: the Oleś brothers. The issue closes with a conversation with the Canadian painter Teresa Young.

We would finally like to take this opportunity to extend our special gratitude to Prof. Andrzej Klawiter (Adam Mickiewicz University) and Dr. Tom Froese (Universidad Nacional Autónoma de México).

Editorial Board

Torun-Warsaw-Bydgoszcz, 08.12.2012

# A REMEDY CALLED ALLEGIANCE

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# What will you do to me when you see me?
## Perception as searching for affordances in the environment

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translated by Monika Włudzik

**Abstract**

Contemporary research on action-perception coupling draws on Gibson's concept of affordances. The text outlines the conceptual basis of this issue, showing how the notion of affordance is embraced in the theory of perception, understood as an active search for structured information that could improve the functioning of the organism in the environment. It also mentions a philosophical parallel (Martin Heidegger's philosophy of tools) and concludes that the intuitions behind the concept of affordances have been more inspiring to cognitive scientists, neuroscientists and researchers in robotics than to psychologists studying perception.

**Keywords:** affordance, information pickup, tool use, tool perception.

Let us imagine that we have found ourselves in an unfamiliar room and we are looking around. What guides our sight when we look around? Do we want to reduce our ignorance and find out where we are? Or, perhaps, visual perception is itself part of an activity that has begun earlier[1] and that requires using perceptual information to plan and execute subsequent tasks? The following reply suggests itself: vision as cognition and visually guided action are two closely connected processes. The first one focuses on acquiring knowledge. This process comes down to receiving and processing sensory information long enough for a conscious percept to appear. After conscious perceptual knowledge is obtained, it can be used in various ways, including a con-

[1] After all, finding oneself in such a room is a result of prior activity. For instance, we have been looking for somebody (an acquaintance, a thief, a terrorist) or something (food, valuables, explosives), we have been following somebody, we have been invited or forced to go inside, etc. The situation that results in the fact that we are in an unknown room also influences the way we are looking around the room. The way we look at the visual scene depends on what we are looking for. Our behavior is different when we look for something we know and when we attempt to find something new, unknown to us that “disturbs” the scene we recognize. We can look around cautiously or fearlessly, look down, ahead or up, stand upright, bend, crouch, stand on the toes, revolve around our axis or walk around the room. In each of these and in similar cases, a visually reconstructed scene will be different, and the divergences will be the result of the fact that its construction depends on previous states and purposes we intend to fulfill.

scious support of action. The case of visually guided action, is completely different. In such cases subjects unconsciously exert perceptual control[2] over the components of their behavior to achieve the expected results which in effect change their surroundings. The chances of obtaining such results increase if the perceivers can skillfully select and process sensory information. Although the approach that intertwines perception and action seems to be natural and convincing, its use is limited, as it is not clear how this attractive vision can be transformed into an effective research program with specified theoretical models. It should not be limited to casual declarations regarding the relations between perception and action, but should give them the form of scientifically testable theses. Transforming intuitions into theory (a collection of hypotheses) would allow us to specify the nature of these relations and, as a result, to design experiments that could test this theory. If we content ourselves with bare intuitions about action-perception coupling, we will not be able to differentiate between standard cases, wherein action supports perception or perception supports action, from non-standard ones, wherein the adequacy of perception requires withholding action or the efficacy of action requires negligence of perceptual data. Perceptual activities such as reading a text, looking at a painting or watching a film belong to the first group, as recognizing letters or following changes between pictures requires refraining from or minimizing motor behavior and taking a position at a particular point in space. In the second group of situations, the efficiency of action depends upon the access to sensory data that we are entirely unaware of due to the specificity of circumstances. Typical examples of these include motor skills of sportsmen (returning the ball in tennis, coordinating actions in team sports, e.g. passing the ball to another team member or the way defenders react to a feint made by the forward) as well as behavior in everyday situations, such as using cutlery during a meal, writing with a fountain pen or auditory control of speech production. There is no doubt that without an access to sensory data and without using them, such activities will end in failure. Interestingly, the sensory data necessary for efficient action often differ from those which are used to construct a conscious percept. Moreover, the requirements of efficacy and smoothness of action, as well as time constraints, compel us to unconsciously select and process the content of such data. It has been shown, for instance, that certain visually guided actions are not affected by perceptual illusions (Kroliczak et al. 2006)[3].

Facing difficulties involved in constructing a unified conception of perception and action, researchers have usually decided to investigate each form of activity separately. The task of finding out how the two interact has been postponed until basic regularities will have been established separately for each type of activity. This approach has also shaped the way research has been conducted. In the case of research on percep-

[2] Conscious control would last too long and would disturb the flow of activity.

[3] It should be noted that efficient reading requires being at rest, as well as making learned and automatic movements of the eyes and the head. Moreover, this is an impoverished form of visual perception, because it is confined to a rapid identification of standardised graphic signs and their sequences located on a flat surface. On the other hand, the efficacy of actions controlled visually (reaching, grasping, writing by hand, eating with chopsticks or knife and fork, using tools, but also walking, dancing etc.) depends on a correct assessment of distance as well as on a spatio-temporal coordination. These assessments have to be accurate and, therefore, visually guided action is not affected by illusory data. (Milner & Goodale 2006).

tion, this has amounted to privileging static situations that involved a subject sitting in front of a screen and looking at simple graphic signs or pictures. Such a radical simplification of experimental setting was modeled after natural sciences and has resulted in a drastic reduction of the number and the complexity of investigated factors. There has only been space for the factors that could be effectively controlled by researchers. It has also been assumed that taking into consideration more complex cases, such as motion of objects in a visual scene or locomotion of the observer would not introduce changes that would significantly modify the perception process.

One of the most vocal critics of this approach was James Jerome Gibson who viewed separating perception from action as a fundamental mistake. He assumed that perception itself is a kind of action and that it is undertaken in order to facilitate non-perceptual actions. He believed that isolating perception from the natural conditions in which it is used by an organism results in false accounts of the way it works. In his critique of the standard approach to perception, he pointed out that situations rarely encountered in everyday life[4] are seen as paradigmatic for perception. This way of investigative conduct rules out the chance to account for perception, because artificial situations (with radically simplified and isolated stimuli, presented to an immobile observer) are treated as models of real perceptual activities. According to Gibson (1966), traditional schools in psychology erroneously conceptualized perception as a passive reception of imposed stimulation. As a result, the organism was pictured as engaged in constructing complex percepts from simple imposed stimuli. Gibson rejected this image and claimed that stimuli in the natural environment are not as simple as those produced in the laboratory and this is the reason why organisms have not developed systems capable of correctly responding to such signals. Instead, they are equipped with systems which can react quickly and efficiently to much more complex stimuli if only these are important for their survival. Moreover, organisms are not passive receivers of stimuli, but rather they actively seek stimulation in the environment, selecting stimuli that are important for survival in their habitat. This obtained stimulation[5] has the form of structured, complex information. Therefore, the task of the organism is not to extract information from physical stimuli and transform it in a complicated procedure of multilayered processing, but rather to "understand the content of messages" broadcast by the objects in its environment.

Since perception is understood as information pickup from the environment, it should be specified in what form the information occurs and what messages are so important for the organism that it makes an effort to obtain them. Gibson rejects the idea that information can be elicited from physical stimuli reaching the organism. The intensities of physical stimuli properties received by the receptors change constantly, so registering all the changes and processing all the information contained in the data would

[4] A typical experiment amounted to a static perception of simple stimulation presented in artificial, laboratory conditions.

[5] "A pure case of obtained stimulation would occur when an active individual moves his limbs or head, stretches his muscles, or scratches himself, or when, on the other hand, he pushes into the prod, looks at the light, listens to the sound, sniffs the odor, or seeks the draft of air. Imposed stimulation occurs with a passive observer. Obtained stimulation occurs with an active observer." (Gibson 1966: 32)

strain the computing powers of the organism. However, in these constantly changing data, there are invariant patterns[6] that refer to features of higher order and those are sought after and recognized in the process of perception. Perceptual systems are used to detect those invariants, since the information important for the organism is contained therein. It remains to be seen which information Gibson qualified as "important" for the organism[7]. What is significant for the organism are not physical properties of objects but their values or meanings[8]. It is the objects, states or events that provide the organism what it needs which are perceived as valuable. This value can be the ability to satisfy the organism's basic needs (food, drinks, shelter, sexual partner, ally, enemy), as well as the ability to satisfy more subtle needs that occur as a result of a learning process (objects edible after cooking, stimulants and other mood enhancing substances, verbal messages etc.). The organism recognizes such valuable features as located in the objects in the environment, but what they afford is assessed from the perspective of the organism's needs. Gibson proposed *affordance*[9] as the name for the object's ability to afford or to deliver that what is valuable for the organism. Framing perception as identification of affordances has radically altered the view of this process. It is no longer the process of detecting the physical properties of an object, but rather, of selecting meaningful messages broadcast by it into environment. The messages contain information regarding the possible actions that could be performed by the preceptor to obtain certain goals. Perceiving an affordance is not synonymous with becoming conscious of it, as perception is deemed successful if the perceptor knows how to make use of it in a purposeful action.

On the philosophical plane, Gibson's concept of affordances is close to Martin Heidegger's philosophy of tool use (1927/1996). The latter is more general as it concerns not only perception, but also all forms of tool use in action; moreover, it is basically an ontology of tool with a barely delineated theory of its use. For Heidegger, the classic theory of knowledge (cognition) is a special kind of tool-use theory. It encom-

[6] When e.g. we are looking at a person running away from us, the shape and the size of his/her silhouette are constantly changing on the retina. Yet, we see this person as having a constant shape and size. The constancy of shape and size is one of typical examples of perceptual invariants provided by Gibson.

[7] "When the constant properties of constant objects are perceived (the shape, size, color, texture, composition, motion, animation, and position relative to other objects), the observer can go on to detect their *affordances*." (Gibson 1966: 285)

[8] "I have coined this word [affordance] as a substitute for *values*, a term which carries an old burden of philosophical meaning. I mean simply what things furnish, for good or ill." (Gibson 1966:285) "This is a radical hypothesis, for it implies that the <values> and <meanings> of things in the environment can be directly perceived. Moreover, it would explain the sense in which values and meanings are external to the perceiver." (Gibson 1979: 127).

[9] "The psychologists assume that objects are *composed* of their qualities. But I now suggest that what we perceive when we look at objects are their affordances, not their qualities. We can discriminate the dimensions of difference if required to do so in an experiment, but what the object affords us is what we normally pay attention to. The special combination of qualities into which an object can be analysed is ordinarily not noticed. ... The affordance of an object is what the infant begins by noticing. The meaning is observed before the substance and surface, the color and form, are seen as such. An affordance is an invariant combination of variables, and one might guess that it is easier to perceive such an invariant unit." (Gibson 1979: 134)

passes cases in which the user abstains from using the tool[10]. In brief, it can be said that in his philosophy Heidegger stresses the differences between a thing and a tool. A thing is a complete, autonomous physical object, while a tool is incomplete and requires a user to be complete. It is the user who can put it in motion, and thus reveal its true nature. This property of the tool to match the shape of user's body or its parts was termed by Heidegger readiness-to-hand. The tool is not recognized on the basis of its physical features, but, rather, its readiness-to-hand. When we perceive a tool we look for features that allow us to connect it with the body and make it possible to put the tool into motion by body movements. It explains why our perception focuses on finding handles, buttons, seats, pedals, etc., because these are essential parts of tools that decide on their readiness-to-hand Gibson's affordance is a counterpart of Heidegger's readiness-to-hand. It is also a feature of an incomplete object that needs to be complemented by its user. The concepts of readiness-to-hand and affordance differ in that the former refers to the objective correspondence between the tool and the user, while the latter refers to the information (message) about the tool's readiness-to-hand.

The conception of affordances, which is the most original theoretical contribution of Gibson, was initially met with moderate enthusiasm of other psychologists, attributable to the aloofness of psychological mainstream towards his ecological psychology[11]. Their coldness was caused not only by the revolutionary character of his doctrine, which entailed the rejection of previous theoretical order, but also by the vagueness of its basic concepts, starting with affordance, and a lack of clear instructions on how to test this conception empirically. The situation changed when Gibson's ideas became an inspiration for researchers working in the fields of cognitive sciences, artificial intelligence, robotics and cognitive neuroscience. As these researchers have belonged neither to psychological mainstream nor to the inner circle of ecological psychologists, they approached Gibson's works broadly, adapting his ideas to their own needs. It has turned out that after such remodeling, Gibson's theory could have been reconciled with approaches that he originally ignored or criticized. It has been demonstrated that his ideas can be expressed in the language of computational theory (Marr 1982, Wells 2002), as well as reconciled with the theory of two visual systems (Norman 2002), or with situation semantics of Barwise and Perry (Chemero 2003). In this circular manner, the theory of affordances has returned to psychology, where it acts like yeast and continues to stimulate new empirical research as well as new theoretical proposal, as the articles published in the present volume demonstrate.

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[10] According to Heidegger, cognition in general and perception in particular are forms of being alongside things (*Sein bei*). To perceive effectively and accurately we should refrain from taking any actions towards the object. Actions produce disturbances, while refraining from action allows for an undisturbed insight into the features of the perceived object. These cognitive attitudes significantly differ from the standard, instrumental relations to objects (things). "In refraining from all production, manipulation and so on, taking care of things places itself in the only mode of being-in which is left over, in the mode of simply lingering with... *On the basis* of this kind of being toward the world solely in their mere *outward appearance* (eidos), and *as* a mode of this kind of being, looking explicitly at something thus encountered is possible." (Heidegger 1996: 57)

[11] A radical criticism of the Gibsonian framing of perception was presented e.g. by Richard L. Gregory (1974). He compared widely known conceptions of perception and deemed Gibson's ecological psychology to be the worst of them.

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# Anthony P. Chemero

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Privately: *There isn't any particular kind of music I like to listen to. I'm just as likely to have Ornette Coleman on as Killer Mike or the Liars. I suppose my favorites are the experimental end of pop and rock, and post-1960s jazz. (I'm listening to the new Ravi Coltrane album as I type this.) For literature, I tend to like longer challenging novels--Pynchon, Nabokov, David Foster Wallace, Murakami, Ishiguro are among my favorites. I even have a Pynchon tattoo. Since having children, I rarely have time to watch movies.* [A. Chemero]

Webpage: *https://edisk.fandm.edu/tony.chemero/*

prepared by Witold Wachowski
picture source: A.P. Chemero's archives

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# With the most profound misgivings
## Interview with Anthony P. Chemero

Dawid Lubiszewski, Witold Wachowski

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**What is a thing? Is this something that contains and discloses truth about our environment, or rather truth about us, observers?**

**"An affordance cuts across the dichotomy of subjective-objective and helps us understand its inadequacy"[12]. It seemed to be a somewhat unclear but significant statement. Does it still?**

I'm not sure I have a general answer to the question about what a thing is. It seems to me that there are many different kinds of things, and there can be a different answer for each of them. I have written mostly about a specific variety of things, what Gibson called 'affordances'. Affordances are the entities that animals perceive and act upon. I agree with Gibson that affordances cut across the subjective-objective distinction, and also that they cut across the mental-physical distinction. This is because affordances are intelligible only in terms of both the abilities of animals, including perceptual abilities, and the features of the environment. So, they really are both mental and physical. So, as you put it, the theory of affordances discloses truths about both observers and environments. The theory of affordances, I also believe, fails to disclose truths about either observers or environments separately. Gibson (and later Gibsonian psychologists) have put this by saying that psychology studies animal-environment systems.

What about other sorts of things? I should point out here that the conception of affordances is (non-viciously) circular. Affordances are only intelligible in terms of abilities, and vice versa. So abilities also cut across this subjective-objective dichotomy. One could also define all sorts of other things in terms of affordances, thereby making affordances some sort of ontological primitive. Doing so would make every kind of thing disclosing of truths about both observers and environments. I have, for example, written about defining perceivable events in terms of affordances. Beyond that,

[12] (Gibson 1979: 129)

however, I'm not sure that generalizing from the funny subjective-objective character of affordances is a good idea. That is, I doubt that everything is both subjective and objective like affordances are.

Certainly, it was not Gibson's intention to claim that everything was an affordance. Rather, he wanted to argue that affordances existed in the environment along with the trees and monkeys. That is the sense in which Gibson was a realist. The things we perceive are not constructed in our heads, and projected onto the world; instead, the things we perceive exist in the world. I feel confident that he was right about this.

**For the short answer: Why is Gibsonian theory the best theory of the nature of animal environment systems for radical embodied cognitive science?**

**Philosopher Martin Heidegger in your account seems to be very… ecological-psychology-hungry. ("Heidegger in the Lab: When tools break down" – an excellent title!) What has inspired you in his work most of all?**

I got very interested in Gibson via reading phenomenology, really. I happened to be reading both Merleau-Ponty's *The Phenomenology of Perception* and Gibson's *The Ecological Approach to Visual Perception* at the same time while I was in grad school. I was struck by the similarities. Prior to that, I had been hanging around Tim van Gelder, and reading lots of Heidegger, along with lots of robotics and dynamical modeling. The set of ideas that I eventually started calling 'radical embodied cognitive science' (that name is stolen from Andy Clark, by the way) started there. When I read Gibson, I thought that he had outlined the way that phenomenologists should do scientific psychology. Reading work by later Gibsonians, especially Mike Turvey and Bill Warren, only made me more convinced of this. Radical embodied cognitive science is, in large part, a philosophy of science for the science that folks like Turvey and Warren do.

That was a genealogical answer to a conceptual question. So here is the conceptual answer, which I wrote about in one of my first publications. When people talk about mental representation, they typically have in mind an organism confronting a independent, structured environment, and the organism having some structures on its inside that stand in for the structures of the environment. So, if you want to be an anti-representationalist, you can say that there is nothing on the inside of the organism that is the right sort of structure. Alternatively, you could say that the environmental structures are not independent of the organism in the right way. It seemed to me that the Gibson was making both of these claims: representations inside of organisms are the wrong way to understand perception, and that the things that organisms perceive are not fully independent of the organisms themselves. Furthermore, Gibson's ideas—unlike Heidegger's and Merleau-Ponty's—have already been the engine for lots of scientific discovery.

**To what extent have you retired from Gibson?**

Over the last several years, Rob Withagen and I[13] have been developing some of Gibson's ideas in ways that most ecological psychologists do not like, precisely because they contradict some of the things Gibson wrote. This gets pretty technical, but the main area of disagreement is over what is required for some pattern in light (or vibrating air, etc.) to carry information about the environment. Basically, Gibson and later Gibsonians (especially, Mike Turvey, Bill Mace, and Bob Shaw) argued that in order to carry information, a pattern in light (etc.) has to be 1:1 correlated with a particular environmental event. Rob and I think this is far too strict, and that we guide our behavior by lots of patterns in light (etc.) that are only probabilistically related to environmental events. Here's an example. Suppose you are in your office with the light on. I could, from outside the building, see that your light is on (a particular pattern) and guide my behavior as if you are in your office. We would say that the light being on carries information about your presence in your office. But, sometimes your office light is on when you are not, in fact, in your office. Gibson (and later Gibsonians) would say that the possibility that you are not in your office when the light is on means that the light being on can never carry information that you are in your office. Rob and I disagree.

This molehill can be made into a mountain, because it implies different conceptions of affordances and direct perception, which Rob and I have also developed. In doing so, though, we don't think of ourselves as abandoning Gibson's ideas, but as fine-tuning them. Many Gibsonians think we are abandoning Gibson.

**There are many definitions of emergence and self-organization. Both phenomena are connected especially in ecological approach. Furthermore, it is not so clear what lower(micro) level and higher(global) level are. How do you understand it?**

I typically try very hard not to use the 'E-word' because it has so many connotations, and is taken by some people to signal fuzzy headedness. I have, however, tried to say in some detail what self-organization is[14], mostly in work collaborating with Mike Turvey. Before I talk about that, I should say something about why ecological psychologists care so much about self-organization, because it's not necessarily obvious why they would. In the ecological approach, perception of the world is not a matter of adding information to sensory representations; instead it is a matter of keeping in touch with the environment. Because perception doesn't end with representations of the environment, action cannot come from developing plans by manipulating representations. So the ecological approach requires an understanding of action that doesn't require plans. As Gibson put it, action needs to be regular without being regulated. There is no internal agency doing the driving. Scott Kelso, Peter Kugler, and Mike Turvey realized that a good way to think about action as regular but not regulated is to

[13] (Withagen & Chemero 2009, 2012)

[14] (Chemero 2008)

think of it as self-organizing. That was more than 30 years ago, and there is now lots and lots of evidence that they were right.

OK, now to the actual questions being asked: what is the right way to understand self-organization, and how should one understand the relation between levels? Unfortunately, there are lots of names for self-organization, even though they all point to the same basic phenomenon, and most descriptions of that phenomenon are highly technical and/or mathematical. The basic idea is pretty simple, though. You can see self-organization every time you flush your toilet, in the whirlpool that forms as the water flows out. This whirlpool is a self-organizing pattern of activity. It is made up of a constantly changing collection of water molecules. When the water molecules are in the whirlpool, their activity is constrained by their being in the whirlpool. Although whirlpools and other instances of self-organization are ubiquitous in nature, they look very strange from the point of view of physicalism and mechanism. The whirlpool is not identical to any collection of water molecules, but it is not something in addition to the water molecules. The whirlpool, which is made up of water molecules, changes the behavior of the water molecules. So, if we want to put this in terms of levels, we have a macro-level whirlpool shaping the behavior of the micro-level molecules that make it—the macro-level whirlpool—up. Notice that this is a pretty dramatic departure from the ways philosophers of mind and philosophers of science typically discuss levels, in that the macro-levels cannot supervene on the micro-levels because they causally interact with them. That is, we have "downward causation" in exactly the sense that is supposedly impossible according to mechanism and physicalism. Michael Silberstein[15], a frequent collaborator, often puts this by saying that physicalism isn't even true in physics. And Michael and I[16] have been writing a lot about consciousness in terms of the sort of micro-macro relations we see in self-organizing systems. (See below.)

**The theory of self-organization and emergence is important in ecological psychology because some of its important aspects like affordances are described in terms originating from those theories. It is said that affordances are emergent results of self-organized interaction between agent and environment. It also means that behaviour is such an emergent property. In many studies scientist are looking on how certain behaviours like grasping or walking emerge. One object of such studies may be robots. Do you think that other, more sophisticated behaviours like moral actions will emerge in robots the same way as walking and grasping?**

I can't make a good prediction about whether moral behavior will be emergent in robots. For now, I must admit, though, that I'm not confident that it will. Mostly, I believe this because I think that there is too much at stake with the sort of robots that are on the horizon (i.e., military robots) to allow ethical rules to be anything but explicitly built in by engineers, under guidance from policy makers.

[15] (e.g. : Silberstein 2002)

[16] (e. g. & Chemero & Silberstein 2008)

My shaky predictions about the future of robot moral behavior do not reflect my views about moral behavior in animals. I believe that moral actions in animals work in exactly the same way as other actions: they are emergent in animal-environment systems. A few years ago, I published a paper about this with Eranda Jayawickreme[17], a former student who now teaches at Wake Forest University. Eranda and I argued that moral virtues should be understood as a kind of ability to act. Just as abilities to walk and grasp are only intelligible in relation to affordances for walking and grasping, we argued that virtues are only intelligible in terms of moral affordances, i.e., opportunities to act morally. If this is right, moral action would emerge in animal-environment systems in exactly the way that walking and grasping do. The paper was just a sketch, and there is a lot more work to do on it. I haven't had time, unfortunately, but Eranda has carried this a little further. He's been using the ideas of virtues as abilities and moral affordances to understand heroism.

**"There is no need to posit representations of the environment inside the animal (or computations thereupon) because animals and environments are taken, both in theory and models, to be coupled"; "Dynamical systems theory can also provide nonrepresentational explanations of internal brain processes"[18]. We have an untoward inquiry: To what extent can one talk reasonably about consciousness and mind without relating to neuroscience, head, brain in a vat?**

I don't think we can explain consciousness without neuroscience, and things in the head will surely be part of the explanation of consciousness. But I feel very confident that we won't be able to tell the whole story about consciousness in terms of brains. Saying this alone is enough to indicate that I give absolutely no credence to the idea of consciousness in a brain in a vat. First, on the brains in vats, everyone should read the paper by Diego Cosmelli and Evan Thompson in the 2011 collection *Enaction*. They consider in some detail exactly how brains work, and in so doing pretty definitively crush the very idea of a brain in a vat. I won't spoil their punch line here. Even more strongly, though, than rejecting brains in vats, I reject the idea of neural correlates of consciousness. There are no correlates of consciousness because consciousness, like thinking more generally, happens in brain-body-environment systems. There's a small discussion of this in my book, and a longer discussion in a recent paper I've written with Michael Silberstein.

Claiming that consciousness doesn't happen in brains alone might strike many people reading this as crazy, even though similar claims have also been made by Evan Thompson and Alva Noë. In today's brain-centric intellectual climate, the claim is undeniably counterintuitive. But Michael and I argue that the current problem space for discussions of consciousness is a dead end, essentially forcing you to be a reductionist or a dualist—there really aren't other stable and convincing positions. The advantage of rejecting the idea of neural correlates of consciousness is that it gets you out of this dilemma. That is, it makes it possible to claim, to adapt a phrase from Ryle, that con-

[17] (Jayawickreme & Chemero 2008)

[18] (Chemero 2009)

sciousness is neither nothing but brain activity, nor is it something else in addition to brain activity. Michael and I argue that consciousness is best understood as the activity of nonlinearly coupled brain-body-environment systems. Maybe this *is* crazy, but at least it has the advantage of pushing us out of the current dead end arguments.

**Are there any dangers threatening cognitive science? And are there any dangers threatening ecological psychology?**

I think that both cognitive science and ecological psychology are chugging along reasonably well at the moment. To the extent that they are under threat, both are under threat from the same source: neuroscientific reductionism. There is a way of interpreting research in neuroscience, quite common among neuroscientists that I have met and often reported in the popular press, that takes neuroscience as a replacement for psychology. That is, many neuroscientists think that, eventually, the psychology and cognitive science departments will go away. At the moment, it seems to me that this sort of reductionism is winning the public relations battle. In optimistic moods, I think that this is a temporary phase.

**When you commented on "Information, Perception, and Action" by Michaels[19], you pointed out her misinterpretation of empirical evidences[20]. Do you see many examples of misinterpretation of empirical studies?**

Let me start by saying that I don't think that Claire Michaels *mis*interpreted her evidence in that study. The main thing I did was to suggest an alternative interpretation, one which seemed to me to have happier consequences than her interpretation.

OK, that aside, I would say that there is always selective interpretation of data. This is inevitable. People are bound to design experiments and interpret findings in light of their theoretical assumptions. And sometimes, those theoretical assumptions will make them blind to important causal factors. I've been thinking a lot about this lately, actually, inspired by the work of philosophers of science such as Bas van Fraassen and Isabelle Peschard.

Peschard looks at some of the experiments I've done with Charles Heyser[21]. Charles and I think that neuroscientists who use the object exploration methodology to study rodents aren't careful enough about the objects they allow animals to explore. We showed, in a series of experiments, that mice preferentially explore objects that are climbable. We concluded from this that neuroscientists need to take an explicitly embodied approach, and focus not just on neurotransmitters, but on neurotransmitters-in-brains-in-bodies-in-environments. Peschard points out that the disagreement is really over what is taken to be *relevant* in explaining the behavior. We are saying that features of bodies and the environment are not just causally active in determining

[19] (Michaels 2000)

[20] (Chemero 2001)

[21] (Chemero & Heyser 2009)

behavior, but also that they are relevant in explaining mouse behavior. No neuroscientist who uses the object exploration methodology doubts that bodies and environments are causally active in mouse behavior; they just didn't think they are relevant. They could respond to our experiments by admitting that bodies and environments are relevant, or they could take features of environments as something to be controlled for in designing experiments. That is, a neuroscientist who does not share our commitment to an embodied, ecological approach can admit that features of objects are causally active in determining behavior, but are nonetheless irrelevant in the scientific explanation of behavior.

One more example, very briefly: Another recent set of experiments I've been working on with students and former students (Dobri Dotov, Lin Nie, Kevin Wojcik) focuses on some claims derived from Heidegger's phenomenology. (Here's a link to an open-access publication: *http://www.plosone.org/article/info:doi/10.1371/journal.pone.0009433*.) In our experiments, the primary thing we measure are the hand-mouse movements of a person playing a simple video game. What we are interested in about the hand movements is their variability, and nothing about their central tendency (i.e., their average). That is, what we are interested in primarily is exactly what is generally thrown out as noise in most other experiments. From our point of view in these studies, the noise is the primary piece of data.

This difference over what is taken to be relevant is probably the biggest difference between "paradigms" in cognitive science, and the most important thing that leads to differing interpretations of data. Neuroscientists agree that the body and the environment are causally important in determining behavior; they just doubt they are relevant. Gibsonians agree that happenings in brains are causally important in determining behavior; they just doubt they are relevant.

**"I have not shown that radical embodied cognitive science is the one true story about the mind or cognition or even perception-action. No clever philosophical argument can do that"[22]. You promote the idea of explanatory pluralism. You claim that we can be both situated, embodied cognitive scientists and realists. Do you believe in these ideas gaining acceptance in academic circle?**

I have actually been surprised at the resistance to explanatory pluralism. It seems to me to be an acknowledgement of the complexity of nature, along with our limitations as investigators. Ian Hacking puts this really nicely when he says that the world is so rich and multifarious that no one story could ever be true of the world as a whole. I would say the same thing about the mind. No one theory will be able to explain perception, action, reasoning, social interactions, creativity, and so on. Many people are less modest about the theories they believe. There are two basic reasons for this. First, many people believe that it should be the goal of the sciences to eventually have unified theories. Second, many people feel that their particular theory of the mind or

[22] (Chemero 2009: 208)

explanatory style will be able account for every phenomenon. These two objections to pluralism can both be admirable. Unification is a worthwhile goal, of course, and good explanations should unify apparently disparate phenomena. It is a good thing as well for scientists to attempt to apply their theories to as many phenomena as possible, if only to determine their limits. But these admirable impulses are too often turned into pieces of metaphysical or normative dogma. For example, some mechanist philosophers of science insist that *only* mechanistic explanation is legitimate.

As for the embodiment and realism, I'm not sure many people care enough to agree or disagree. One exception, I suppose, is Tom Ziemke[23], who reviewed my book and devoted a lot of the discussion to my discussion of realism. He politely disagreed with my conclusions.

**It has not been a long time since you published your last book *Radical Embodied Cognitive Science*, but you are a very active researcher. Is there anything that you would like to change, add or remove from the book?**

**What is the most problematic question for you in your work recently?**

**If you had to attack Anthony Chemero's approach, what would you choose as a blind-spot?**

Of course, there are many, many things I wish I'd done differently in my book. There are things I should have said differently, and things I wish I had written more about. One mistake I made was to focus on Gibson and dynamics too much, and in so doing failed to reflect the strength of the influence of phenomenologists on the ideas in the book. I must admit that this was partly strategic: I thought that the ideas in the book would strike as being too strange already, and I didn't want to give mainstream cognitive scientists and philosophers of cognitive science yet another reason to dismiss the book without reading. Colin Klein, another former student, read a draft of the book and said something like "All the Gibson and William James is weird enough, but Feyerabend? That just goes too far." Imagine if I had also added lots of Heidegger and Merleau-Ponty Though it might have scared off some readers, I think that the book would have been better if I had allowed more of the phenomenology in. I make up for this in my next book, I guess, which is co-authored with my colleague Stephan Käufer and is actually about phenomenology. Stephan and I argue that the heirs of the phenomenological tradition that begins with Husserl, Heidegger, and Merleau-Ponty are scientists, not philosophers and literary theorists. In particular, we argue that beginning in the 1960s the tradition of phenomenology is taken up by several groups of cognitive scientists and neuroscientists: the practitioners of ecological psychology, enactivist cognitive science, neurodynamics, cognitive linguistics, and Heideggerian artificial intelligence and robotics. So the legacy of phenomenological philosophy is not post-modernist literary theory; rather, it is research in the cognitive sciences that attempts to explain lived, human experience.

[23] (Ziemke 2001)

The second major thing I think is really wrong with the book is in the discussion of dynamics. There are two main problems with it. The first is that I had focused too much on research closely related to the Haken-Kelso-Bunz (HKB) model. Focusing so much on HKB was rhetorically useful in a lot of ways: it is easy to understand and already familiar to a lot of readers; it allowed me to make a lot of points about how dynamical cognitive science works; and it allowed me to show how dynamical models could provide guides to discovery; how it explains both by making predictions concerning novel phenomena and by unifying apparently disparate psychological and neural phenomena; etc. But… it is hardly the state of the art in dynamical modeling, and I wish I had focused more on some newer varieties of dynamical modeling. The second problem is that I underestimated the popularity of the "dynamics doesn't explain, it only describes" objection to dynamical modeling. Much to my surprise, I hear that dismissal of dynamical cognitive science and neuroscience all the time. Had I known that *anyone* still believed that this was an effective objection, I would have addressed it in the book. Now that I realize that *lots of people,* including people I respect like Bill Bechtel, believe this objection, I am working with Michael Silberstein on a new theory of dynamical explanation. We've just submitted the first of what will be a series of papers on this.

**Preface and beginning of the first chapter of your book was a sort of manifesto for our academic association, from which the *AVANT* journal originated. Were you, so to speak, asking for trouble writing this text, or a declaration like this?**

One of the central tenets of my academic worldview is that things are supposed to be fun. It should be fun to write the things you write, and people should enjoy reading them. This is especially true in philosophy, where the stakes are pretty low. I haven't always succeeded in writing things that are fun to read, but I think that I did succeed with several chunks of my book, including the beginning. I wouldn't say, exactly, that I was asking for trouble, at least not at first. But when I saw the perplexed way some (especially, older) philosophers reacted to talks about this material, I thought it would be fun to push it farther. Let's not just compare Chomsky and Fodor to Hegel; let's also compare them to intelligent design theorists!

It is important to realize, though, that Chomsky and Fodor are among my heroes. I sent a copy of my book to Fodor. He sent me a kind letter, saying he "looked forward to reading it with the most profound misgivings." The letter is hanging on the wall of my office.

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# Understanding affordances:
## history and contemporary development of Gibson's central concept

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**Abstract**

Gibson developed the affordance concept to complement his theory of direct perception that stands in sharp contrast with the prevalent inferential theories of perception. A comparison of the two approaches shows that the distinction between them also has an ontological aspect. We trace the history and newer formalizations of the notion of affordance and discuss some competing opinions on its scope. Next, empirical work on the affordance concept is reviewed in brief and the relevance of dynamical systems theory to affordance research is demonstrated. Finally, the striking but often neglected convergence of the ideas of Gibson and those of certain Continental philosophers is discussed.

**Keywords:** affordance; Gibson; perception-action; dynamical systems theory; phenomenology.

*An affordance cuts across the dichotomy of subjective-objective and helps us to understand its inadequacy. It is equally a fact of the environment and a fact of behavior. It is both physical and psychical, yet neither. An affordance points both ways, to the environment and to the observer.*

(Gibson 1979: 129)

At the end of the 1970's, the American psychologist James J. Gibson introduced the concept 'affordance'. It was meant to help complete the ecological theory of direct perception, of which he was the main proponent. On the one hand, affordances are a very easy thing to explain; they are the possibilities for action that an environment allows to an animal. On the other hand, the affordance concept can become obscure when one tries to exactly define it along with the notion of direct perception.

On the following pages, we explain the concept in detail. In particular, we introduce it in the historical context in which it appeared. Then, subsequent attempts for formalization and extension of the concept are discussed, followed by an empirical review and a discussion of comparable notions in the work of other scholars.

## Preliminaries: two-term and three-term theories of perception

Perception has long been a central topic of study in philosophy, psychology and more recently in neuroscience—perhaps because an understanding of perception would answer not only the question of how one gets about in one's daily life, but also how the attainment of knowledge is even possible. The central assumption underlying the theoretical framework of the received view follows from the work of Herman von Helmholtz (1878/1971) but can be traced further back to Plato's ideas (see the last sections for more on the relation between Gibson and the philosophy of perception). The assumption is that perception is a *three-term relation* among a subject, an object, and something internal to the subject that stands in for the object (e.g., a representation). For example, one can construe visual perception such that the projected images on the retina play the role of that third term, and introduce a process of *unconscious inference* that disambiguates the images on the basis of previously acquired knowledge. This is necessary because the images on the retina are only deformed projections of the perceived object.

Gibson disagreed strongly with such a conception of perception. In his early work, he strove to show the weaknesses in three-term explanations. For example, how can knowledge from previous experience inform the current situation without the perceiver already having knowledge about the current situation? And why does one need to presuppose that vision is based on static images on the retina? The retina always moves (between saccades too), presumably in order to sample the optic array in its *transformation*. For that matter, static stimulation is not even defined. When the image on the retina is immobilized with a special apparatus, the study participants cannot even tell whether light is present or not. Hence, the eye is better conceived of as a 3D apparatus (two-space plus time) of the visual system, not a 2D camera. The motivation behind the snapshot (2D) model that requires a three-term theory of vision is problematic. For such reasons Gibson rejected the view that animals perceive indirectly objects in their environments by way of perceptual mediators such as retinal images that refer to these objects. He posited that perception is direct, that is, it is a two-term relation between animal and environment.

**Origin**

Gibson realized that if the notion of indirect perception is abandoned, the traditional ontology of perception needs to be altered too. Therefore, in addition to his first claim, that perception is a two-term and not a three-tern relation between animal and environmental properties, he proposed that perception is not of Lockean qualities such as the length of an object expressed as the Euclidean distance between points but of possibilities for action. Note that in traversing a gap, length itself does not qualify as a possibility for action but 'shorter than my step' does.

It is generally accepted that Gibson's two claims necessarily imply each other (but see Vicente 2003). Suppose that only the first claim is true. If *only* things such as surfaces were perceived directly – Gibson's early work focuses on the perception of surfaces (Mace 2005) – then the possibilities for action that these surfaces afford would have to be inferred. Because the main function of perception is to enable action, it would follow that most perception is indirect.

Supposing that only the second claim is true turns the affordance concept into a mere buzzword. One can study possibilities for action as conceptual possibilities in the framework of the received view (e.g., designing algorithms that infer gap length from distance cues and compare it to an internal representation of maximum step length). One can call the product of such an algorithm affordance perception, but this would not make any difference to the computational theory. In other words, the affordance concept has a special meaning and hence, is *necessary*, only in the context of Gibsonian theory.

What was Gibson's original exposition? In contrast with the ontology of the received view, e.g., "[o]rthodox psychology asserts that we perceive these objects insofar as we discriminate their properties or qualities", Gibson (1979: 134) proposed "... that what we perceive when we look at objects are their affordances, not their qualities". What are affordances? "The affordances of the environment are what it offers the animal, what it provides or furnishes, either for good or ill. I mean by it something that refers to both the environment and the animal in a way that no existing term does" (Gibson 1979: 127).

What is it about the environment that allows affordances to be perceived? "Perhaps the composition and layout of surfaces constitute what they afford. If so, to perceive them is to perceive what they afford" (Gibson 1979: 127). In order to understand what this layout might be, it is useful to refer to his earlier insights about the *occluding edge*, which follow a similar logic (Mace 2005). As I move my head leftwards, an edge (the right side of the computer screen) progressively erases a portion of the visual array (the wall that serves as background). This is enough to show in a definite way that the screen is *between* me and the wall. It is crucial to realize that the occluding edge does not only define the locations of surface A (here) and surface B (there) but also their spatial order *relative to me,* the point of observation. Later in his career, Gibson realized that the occluding edge provides even more information – it specifies to a per-

ceiver that she can reach the one surface directly and needs to make a detour to reach the other. In short, things such as occluding edges can specify not only spatial order but also affordances (e.g., *reachability*).

## Effectivities complement affordances

Gibson put enormous effort into understanding what it is about the environment that allows one to directly perceive it. Consequently, his work says much more about the environment, especially the visual environment, than about the animal. The term *effectivity* was introduced in order to help compensate for this inequality (Michaels & Carello 1981; Shaw et al. 1982). The animal's effectivities are directed to the environment in the way that the environment's affordances are directed to the animal. An affordance dispositional and an effectivity dispositional; the concepts complement each other and, thus, make a dual.

This move has attracted some criticism because one could possibly understand it as a step backwards. Gibson's affordance "*points both ways*, to the environment and to the observer" (Gibson 1979: 129, italics added); there is no need for two terms. In the "reformulation", however, affordance and effectivity point unidirectionally (Cutting 1982: 212). Accordingly, one abandons what is most important and original about affordances by dispensing with the double arrow in their formalization.

The apparent discrepancy between Gibson and Shaw-Turvey-Mace stems from the different ways in which the word affordance is used in the two formalisms. What Gibson calls an affordance is what Shaw-Turvey-Mace call an affordance-effectivity dual. Gibson's double-sided arrow is replaced with something like a field with two poles. In order to see how the two formulations are similar, consider that instead of an 'affordance-effectivity dual' one could say 'environment pole and animal pole of the affordance dual.' The latter version fits more literally with Gibson's expression and would not change the Shaw-Turvey-Mace formalism. Furthermore, dispositions necessarily come in complementary pairs. Hence, the relational character of affordances that Gibson was aiming for is also part of the definition (Turvey et al. 1981; Turvey 1992). Michaels (2003) provides an evaluation of the gains and losses related to the notion of effectivities. Shaw and colleagues (1988, 2001) aim to develop a systematic theory of effectivities.

## Formalism and ontology

Gibson's second claim, that perception of affordances is understood as *real* and not as *conceptual possibilities* for action, implies that affordances are an *ontological*, not an *epistemological* category (Shaw et al. 1982; Turvey et al. 1981; Turvey 1992). Additionally, affordances exist as properties of the environment independently of the perceiver. As long as squirrels exist, a particular tree affords climbing to a squirrel regardless of whether there is a squirrel around. Hence, affordances are prior in logic to their actualization. One way to summarize a lot of what has been said so far is through the

definition "A situation *X* affords action *Y* to an animal *Z* on occasion *O* if certain relevant compatibilities between *X* and *Z* obtain" and, analogously, "An animal *Z* can effect action *Y* on an environmental situation or event *X* if certain relevant compatibilities between *X* and *Z* obtain" (Shaw et al. 1982: 196-197).

What are these compatibilities? In any particular situation, what is dynamic and what a psychologist would focus on is not *every* potential affordance and effectivity, but the match of *particular* affordances and effectivities that got *actualized* by the experimental situation. Turvey (1992) accommodates this fact by explicitly assigning a role to the environmental properties and animal dispositions supporting an affordance. An animal *Z* with the disposition to perform an action *q* (same as *Y* above) and an environment/situation *X* with a property *p* that complements the disposition *q* form an environment-agent system $W_{pq} = j(X_p, Z_q)$. The dispositionals *p* and *q* actualize each other once being made available to each other, what Turvey calls the juxtaposition function *j*. In this manner they select each other out from the larger arrays that contain all potential properties and actions of the particular environment and agent, respectively.

Kadar and Effken (1994) develop an approach to the ontology of affordances and effectivities that is more of a critique of Turvey's formalism (1992) than a self-sufficient proposal. Specifically, they would like to replace the metaphysics that Turvey's work relies on with Heidegger's. Turvey is led to build his ontology out of things with properties. Conversely, adopting Heidegger's metaphysics, the authors argue, would lead to assigning ontological primacy to fields. The question is, which of the two metaphysics fits better with Gibson's understanding of affordance? Kadar and Effken argue that, first, Heidegger and Gibson thought the same way about many issues (see the section Relations between Gibson and some philosophers of perception) and, second, fields can be made consistent with both Gibson and Heidegger, but things cannot.

**Recent developments**

In contrast to some of the earlier formalizations (Turvey 1992; Michaels 2003) and somewhat comparable to Kadar and Effken (1994), Chemero (2003, 2009) argues against understanding affordance as a property of the environment. Instead, it is a *relation* between an animal's ability to act and aspects of the environment. For instance, the affordance "stair-climbability" *is* the relation between riser height and climbing ability of the observer, and is not in the layout of the surfaces alone. This move, Chemero argues, solves the philosophical problems associated with properties and dispositionals while keeping Gibson's approach intact (2003, 2009). The situation is similar to the explication made earlier of how both Gibson's and Shaw-Turvey-Mace's accounts fit the same abstract theoretical model.

Regardless of whether affordances are understood as duals, dispositional properties, or relations, all the formalizations listed so far treat affordances in a timeless domain. The field of ecological psychology, however, is heavily influenced by the advent of dy-

namical systems theory. Chemero (2009) therefore argues that the coupling between affordance and ability (effectivity) should be treated not only as an instantaneous match (lock and key) but as an unfolding system that in the longer time domain converges with niche construction.

Chemero's (2003, 2009) real break with traditional Gibsonian theory is with respect to the issue of specificity of information. Usually, one takes it that perception can only be direct if information exists that *specifies* what is possible; otherwise ambiguity exists, and a process of inference is necessary. However, building on the work of Barwise and Perry (1981) and Milikan (2000), Chemero argues that information need not be specific as long as there is a *constraint* that connects (non-specifying) information with what is present in the environment in a way that is reliable enough to guide behavior (see also, Withagen & Chemero 2009, 2011). For instance, to a prey animal the shadow of a flying predator informs it about the presence of a predator, but the constraint between the predator and the shadow is merely correlational. On a cloudy day, the flying predator will not produce a shadow moving across the ground. And if the shadow is present, the possibility exists that it was produced by a non-predatory animal or a flying object of a similar shape (example taken from Millikan 2000). This position thus explicitly addresses perceptual error (see Gibson 1979: 142-143 on misinformation for affordances). It also fits well with empirical findings of perceivers' use of non-specifying information, such as those by Michaels and de Vries (1998). For a critique and extension of Chemero (2009) see Withagen and van der Kamp (2010).

**The scope of the concept**

Turvey (1992) abides by Hume's touchstone; action-based affordances at a level that applies to all animals are to be taken as "propaedeutic to any extension of affordances to other domains" (Turvey 1992: 174). Conversely, Shotter (1982) emphasizes the historical character of affordances. In a human world one *must* consider the socio-cultural affordances. For Gibson, mailboxes afford sending letters to a human *encultured* in letter-writing in the same way that chairs afford sitting. Stoffregen (2004) also argues for a broader scope of affordances. This is appropriate in the context of his understanding of affordance as *emergent* properties of the animal-environment system. Heft (2001) attributes affordances to the intrinsic properties of features, objects and events that tie us together in relations. Therefore, values and motivations that are intrinsic to affordances also constitute a proper domain of study.

Equating affordances with just anything that is "meaningful" might trivialize them (Michaels 2003). Still, Gibson's theory of perception was meant to address the human world in its full complexity. For instance, the mailbox example refers to a network of human activities. A full-fledged theory of affordance should be able to take heed of the richness within any action. How to balance these requirements? Maybe affordances can be organized by taking into account the different capacities of different organisms since these, taken as effectivities, actualize an affordance.

## Classical experiments

Eleanor Gibson must be the pioneer of affordance research with her work on child development, which even preceded the formal introduction of the concept (in: Adolph & Berger 2006). Her *visual cliff* paradigm shows that human babies and self-locomoting animals perceive that the cliff affords falling and injury, whereas the non-self-locomoting ones do not until they learn to locomote. A methodologically more optimal design, however, consists of an adjustable *locomotory slope*. Instead of a cliff one uses a declining surface and as a result there is no need for a glass plate that may or may not be as transparent as the experimenter would like (Adolph & Berger, 2006). A novel finding there is the *motor-specificity* of learning. After having learned to perceive which slopes afford crawling and which do not, infants fail at the task once they start walking and, hence, have to go through the learning process again.

Warren (1984) was among the first to test the animal-relative character of affordances. He examined participants' perceived ability to step on the flight of a stairway. In particular, the transition from ability to inability was measured as a function of riser height. Participants from both the “short” and “tall” groups transitioned when the ratio of riser height to individual leg length had reached a value that was the same across groups. Such ratios are known as dimensionless π-numbers (the units cancel out because they are the same in both the numerator and denominator) and can also index critical transitions in certain purely physical systems. For instance, the Rayleigh number for a given fluid predicts heat transfer transitions from conduction to convection (as in the onset of boiling).

The hypothesis that body-scaled information specifies affordances was tested further using judgments of the passability of a vertical aperture (Warren & Wang 1987). The layout of an Ames-like room was manipulated in a way that demonstrated that participants relied on body-scaled (eye height) information and not on extrinsic cues of object size and distance. Similarly, a study of sitting and stair-climbing found that an eye-height-based π-number specifies the affordance boundaries (Mark 1987). Interestingly, participants adjust to the eye-height changes induced by platform shoes only if allowed natural posture and mobility patterns during the learning phase (Mark et al. 1990).

## Some recent studies

Empirical π-numbers for affordance boundaries such as those found in the stepping and sitting studies need theoretical justification. Otherwise, one could speculate that a higher-order homunculus is monitoring the use of information and flipping on-off switches in accord with memorized threshold values. Self-organizing systems theory naturally handles these transitions, formally called phase transitions. Nonlinear dynamical modeling extends the previously merely conceptual use of this theory by giving an explicit mathematical account of affordance transitions (Fitzpatrick et al. 1994; Frank et al., 2009; Lopresti-Goodman et al. 2011; Richardson et al. 2007).

The intrinsic metric for affordances may include variables such as *effort*. Participants tend to overestimate the slant of a perceived slope when their response is verbal or pictorial but are relatively accurate when the response is in the form of a coordination task (Proffitt et al. 1995). Importantly, the overestimation is amplified after exercise implying that participants are not merely perceiving slant but a surface to be climbed. A similar paradigm has shown that the effect of prospective effort appears if the participants actually intend to perform the action corresponding to the distance being estimated (Witt et al. 2004; but see Woods et al. 2009).

Generalizing the intrinsic metric even further, one can simply use *abilities* as the scaling factor (Chemero 2003). Fajen studies abilities in the context of visually guided actions (Fajen 2005; Bastin et al. 2011). For instance, braking behavior while driving depends on the car's maximum deceleration (Fajen 2005) and subjects turn toward a target when the ideal speed required to intercept it is less than maximum possible speed, and ahead of it when ideal speed is greater than maximum (Bastin et al. 2011). Thus, observers adjust such that the intended action is always possible within the limits of their action ability.

**Relations between Gibson and some philosophers of perception**

Ever since the classical Greek period, most philosophers and, later, psychologists would assume as a starting point in their studies of knowledge and perception what appears to be an obvious truth—here I am, a subject, looking at something over there, an object. How does a subject get to know a detached object? This question has been formalized as Cartesian skepticism. Asking this question leads to a representational account of knowledge and perception. In contrast, some of the most important 20th century philosophers (e.g., Carnap, Wittgenstein, Dewey, Davidson, Heidegger) argued against even considering the skepticism problem and instead sought to "dissolve" it (Blattner 2006: 109). Heidegger's particular strategy is illuminating; scholars need to stop relying on bare intuition to formalize their foundational problem and start using a systematic phenomenological analysis. The conclusion of his analysis is that the subject-object dichotomy only appears in what is called the present-at-hand mode. This mode exists only in isolated cases of experience that do not warrant the presumed fundamental character of the dichotomy.

The similarity between Gibson and Martin Heidegger and Maurice-Merleau-Ponty has been pointed out a number of times (Dreyfus 1996; Heft 2001; Kadar & Effken 1994). Compare their ways of bridging the subject-object divide: "An affordance is neither an objective property nor a subjective property; or it is both if you like." (Gibson 1979: 129) and "Perceivedness … is in a certain way objective, in a certain way subjective, yet neither of the two" (Heidegger 1982: 314).

What other commonalities can one obtain? First, the Heideggerian word for affordances could be *equipment* (Kadar & Effken 1994), but *ready-to-hand* is a possibility too. The latter stands for the function, the *for-what*, that a tool promises (Dreyfus

2007). Consequently, an affordance structure that defines a niche (Chemero 2009) would correspond to Heidegger's functional totality, the structure of *towards-which* or *in-order-to* relations (Blattner 2006: 59). Second, Heidegger (Dreyfus 1991) just like Gibson assigned his respective construct the status of ontological primacy, more fundamental than Lockean properties which themselves have a derived character. Third, for both scholars possibilities for action cannot be cast in terms of propositional knowledge characterized by truth conditions (Blattner 2006: 94; Turvey 1992: 176). Finally, both the concepts of equipment/ready-to-hand and affordances are fundamentally temporal. Dasein is historical and the kind of being that projects itself ahead of itself (Dreyfus 1991: 186), it is always "pressing forward into possibilities." And on Gibson's side, affordances are prospective by nature (Turvey 1992).

Gibson's own abandonment of the subject-object assumption is probably most directly influenced by the American *pragmatism* tradition of William James and John Dewey of which he was part through his advisor Edwin Holt, who was a student of James. See Dewey's famous critique of the *reflex arc* concept for an entry point in the pragmatist tradition, and Heft (2001) for a detailed exposition of this lineage.

Who else can be found in Gibson's anti-representationalist camp? According to Lombardo, Aristotle anticipates Gibson in opposing the mind-matter dualism of his mentor Plato and in arguing that the world itself and not the world of appearances is the object of perception. Additionally, Aristotle's "correlative objects" are similar to Gibson's notion of affordances as perceived opportunities for action in the environment (Lombardo 1987).

The umwelten, the perceiver-centered animal-relative worlds of von Uexküll (1957), also converge with Gibson's thinking—more specifically with the way an affordance-structure defines an animal's niche (Chemero 2009). We do not know if Gibson was familiar with von Uexküll's work but both Merleau-Ponty and Heidegger read von Uexküll, and Gibson was familiar with Merleau-Ponty (Heft 2001: 161). There are several other theorists whose ideas were closely related to Gibson's affordance concept but who were representationalists. These include Egon Brunswik and the Gestalt psychologists.

## Conclusion

What are some of the underlying themes in the sections presented above? Ecological psychology carefully examines the assumptions that sit behind research in perception-action. It has exposed fundamental problems with the subject-object dichotomy that is taken for granted by the received view. Affordance research is not just about how knowledge is acquired but about what there is to be known to begin with. It is thus applied research as much as it is theoretical. It is an active field, both in terms of theoretical development and in terms of the amount of empirical work that is being done. It has also become extremely interdisciplinary. Just in this short review we have touched upon experimental psychology, physics, dynamical systems theory, self-organization, and stuck phenomenology in between.

What are some of the outstanding challenges that affordance research faces? The concept was developed in the context of an ongoing dispute between the ecological and cognitivist traditions within psychology. Nowadays, however, the domains of perception and action are arguably dominated by the neurosciences. These retain the information processing metaphor of cognitivism (at least superficially) but are different enough to be considered a movement on its own. Thus, there is a new and very powerful player on the scene and proponents of the ecological approach should determine their stance. It would be easy for a cognitive neuroscientist to appropriate the word affordance but miss its substance by simply talking about "action representations" stored in some cortical area. Although more thoughtful attempts to develop an ecologically-motivated neuroscience do exist (see for example Cisek & Kalaska 2010), it is yet to be seen if such attempts have any prospects.

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# Gibson's ecological approach
## – a model for the benefits of a theory driven psychology

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**Abstract**

Unlike most other sciences, psychology has no true core theory to guide a coherent research programme. It does have James J Gibson's ecological approach to visual perception, however, which we suggest should serve as an example of the benefits a good theory brings to psychological research. Here we focus on an example of how the ecological approach has served as a *guide to discovery*, shaping and constraining a recent hypothesis about how humans perform coordinated rhythmic movements (Bingham 2004a, b). Early experiments on this task were framed in a dynamic pattern approach. This phenomenological, behavioural framework (e.g. Jeka & Kelso 1989) classifies the behaviour of complex action systems in terms of the key order parameters, and describes the dynamical stability of the system as it responds to perturbations. Dynamical systems, however, while a valuable toolkit, is not a theory of behaviour, and this style of research is unable to successfully predict data it is not explicitly designed to fit. More recent work by Bingham & colleagues has used dynamical systems to formalise hypotheses derived from Gibson's ecological approach to perception and action, with a particular emphasis on perceptual information. The resulting model (Bingham 2001, 2004a, b; Snapp-Childs et al. 2011) has had great success with both the phenomena it was designed to explain as well as a wide range of empirical results from a version of the task it is not specifically designed to explain (specifically, learning a novel coordination). This model and the research programme that produced it stand as an example of the value of theory driven research, and we use it to illustrate the contemporary importance the ecological approach has for psychology.

**Keywords:** Gibson; Bingham; ecological psychology; theory; coordinated rhythmic movement.

When particle physicists recently found that some neutrinos had apparently travelled faster than light (Adams et al. 2011) it never actually occurred to them that this is what had happened. On the basis of the extraordinarily well supported theory of relativity, the physics community went 'that's weird - I wonder what we did wrong?', and proceeded to use that theory to generate hypotheses they could then test. It would take a lot of fast neutrinos to disprove relativity, and even though the result turned out to be caused by a faulty cable, the robust response by physicists stands as an example of the benefits of a good theory.

Similarly, the core of modern biology is the theory of evolution. When creationists say 'we can't see how a bacterial flagellum which rotates like an outboard motor could possibly have evolved, it's irreducibly complex' (e.g. Dembski 2002), biologists are entitled to say 'we have evidence that lots and lots of other things have evolved. Let's see if we can figure out how the flagellum did it, and in the meantime, we're going to operate on the assumption that it did evolve until we have strong evidence to the contrary'. The resulting theory driven empirical work then happily led to a coherent evolutionary story for the flagellum (e.g. Musgrave 2004).

Psychology has many individual theories describing isolated phenomena but no core theory of behaviour to guide our research, no analogue to the theories of relativity or evolution. This is beginning to cost the discipline. Recently Bem (2011) published a series of experiments purporting to demonstrate evidence of precognition. Bem took several standard psychological experiments and reversed the temporal ordering of the elements. Analyses showed a series of statistically significant effects that suggested that events in the near future were affecting earlier performance. For example, he showed participants a list of words then tested their free recall. After this test, he trained the participants on a subset of the words, and showed that there was improved recall of those words, even though the training had come last. Because he followed the rules of experimental design and had statistically significant results, the Journal of Personality & Social Psychology was unable to find a reason to reject the paper. The editors only noted that "the reported findings conflict with *our own beliefs* about causality and that we find them extremely puzzling" (Judd & Gawronski 2011: 406, emphasis ours). Note that the cited conflict was with their *beliefs* about causality, and not, for example, the laws of physics and what they have to say about time travel. This should have been an opportunity for Bem to discuss problems with the standard methods and analyses that produced these physically impossible results (the approach taken in a companion paper by Wagenmakers, Wetzels, Borsboom & van der Maas 2011). Instead, his discussion was framed in terms of a loose reading of quantum physics and an appeal to psychologists to keep an open mind. The paper simply *described* what had happened, without any real attempt to *explain how* it had happened. A failure to replicate Bem's key effects has recently been published (Ritchie, Wiseman & French 2012), but this paper was also entirely empirical and descriptive in nature, with no reference to any underlying theory of how the world works.

Psychology needs a core theory in order to mature as a science. Theory serves a dual role in science. It allows the scientist to identify when a result is likely to be anomaly (e.g. faster-than-light neutrinos), and, more critically, it provides a *guide to discovery* to

structure the search for explanations of novel phenomena (e.g. the bacterial flagellum). The Bem experiments demonstrate how, without a theory, psychology is unable to deal rigorously with anomalous results. This paper will discuss how an example psychological theory (James J. Gibson's *ecological approach to visual perception*; Gibson 1966; 1979) has been able to guide discovery and explanation of new phenomena, specifically how people learn to produce a novel coordinated rhythmic movement. It has been able to do this because it is a theory of both the objects of perception and the ecological information that supports that perception. The theory can therefore be used to propose specific mechanisms to *explain* a given behaviour, rather than simply providing some terms to *describe* that behaviour. We will suggest that the successes of this approach in the area of perceptually guided action stands as a clear model of what a truly theory-driven psychology could achieve.

## The ecological approach – a brief review of some key points

Gibson famously begins his 1979 book on visual perception with an extended analysis of the environment organisms inhabit, rather than the more traditional starting point of the anatomy of the eye. The reason is simple: Gibson knows that in order to understand why the anatomy of the eye is the way it is, we need to first understand what kinds of properties it has evolved to detect. The traditional analyses note that the eye works similarly to a camera, with a lens that focuses light (presumably an image) upside down onto a pixelated retina that varies wildly in resolution and which contains an enormous blind spot. The analysis then takes this poor quality image as the basis of visual perception and begins to investigate the internal (representational) structures that are now required to enrich the image to a point where it can support the rich phenomenology of visual experience (e.g. Marr 1982; Rock 1985). Gibson's first powerful move is simply to recognise that the eye is not the starting point of the analysis. Eyes evolved under selective pressure to enable access to information in the environment that could support action and guide behaviour. The question then becomes, what is that information, and what is it about?

This move pays off immediately. If the function of vision is to support action, then vision must provide us with access to information about action relevant properties of the world. Organisms don't need to know how far away an object is; instead, we need to know whether we can reach it, and if so can we grasp it (e.g. Mon-Williams & Bingham 2011); or perhaps we need to know whether it is approaching us on an interception path, and if so do we have enough time to respond by evading or intercepting the object (e.g. Tresilian 1999). We therefore need to know about properties of objects measured according to our ability to act with respect to those properties, and how the current layout of properties is varying over time. Gibson coined the term *affordances* to describe the organism-scaled action relevant properties of the environment,[24] and changes in the layout of the organism's environment are *ecological events*.

[24] There is something of a debate in the literature about whether affordances are dispositional properties of the environment (Turvey 1992; Turvey, Shaw, Reed & Mace 1981) or whether they are relational properties of the animal-environment system (Chemero 2003, 2009; Stoffregan 2000, 2003). We favour the dispositional

Defining these ecological properties of the world is relatively straight-forward: Gibson then notes that "The central question for the theory of affordances is not whether they exist and are real, but whether information is available in the ambient light for perceiving them" (Gibson 1979: 140). Gibson's theory therefore suggests that the prime goal of any empirical investigation should be identifying the perceptual information that supports access to world properties (affordances and events) and his analysis of the nature of the information available to a visually perceiving organism, *ecological optics*, provides clear guidelines on what that information can look like[25].

Gibson's analysis of the environment identifies two key facts. First, the basis for vision is not light, per se, but structure in light. The clearest demonstration of this is the Ganzfeld experiments, in which an observer is presented with plenty of light in an entirely homogeneous light field and perceives nothing at all (Gibson & Dibble, 1952; Metzger, 1930). This is the situation faced by an observer in white-out conditions during a blizzard; there is plenty of light energy, but there is no structure, and thus nothing is perceived, often with disastrous consequences. However, light that has interacted with a surface contains structure that reflects (pun intended) that interaction and can therefore carry information about the surface. Gibson calls this structured light the *optic array* and identifying the contents of this array is the main focus of ecological psychology research.

Second, organisms are always moving throughout the environment. This motion provides us with a constantly changing sample of the optic array, and, more importantly, the changes are not random. Instead, the structure in the array will transform smoothly and in ways specific to the relation between the organism and the ecological properties of the world that caused the structure. Higher order relational structure in this *optic flow*, which is caused by these world properties, can remain *invariant over the transformation*, and these invariants are specific to the properties that caused them. These invariant features are therefore specifying information about affordances and events, and an organism that can detect the information is directly perceiving ecologically relevant properties of the world (Bingham 1995; Turvey, Shaw, Reed & Mace 1981).

The ecological approach serves contemporary psychology and cognitive science in two ways. First, while Gibson's theory is not a complete theory of behaviour, it is an excellent foundation for one, because it provides a detailed account of how we perceive the environment that changes how we treat perception. Direct perception of affordance properties changes the job description for any cognitive, post-perceptual processes, from inference about the source of the information to using that information to coordinate and control skilled action. If you have direct access to action relevant proper-

account, but the difference is not crucial for the current discussion; in both cases affordances are considered to be real and capable of creating information.

[25] Ecological optics has since been refined into the theory of *kinematic specification of dynamics* (e.g. Runeson & Frykholm 1983). Properties of the world are defined dynamically, in terms of both their motions (kinematics) and the forces that caused those motions (kinetics). Perception has access only to kinematic information (this is the *perceptual bottleneck*; Bingham 1988) but this is capable of specifying dynamical properties.

ties such as affordances, for example, there is no need for any internal process that infers the existence of the affordance from a more limited set of perceptual information. In other words, any theory about behaviours that depend on perception (i.e. all of them) should work out what Gibsonian perception has already done to allow the behaviour to emerge before placing all the responsibility in the central nervous system. In this way, the ecological approach should be treated as the starting point for *any* theory of behaviour. In particular, embodied cognition researchers are beginning to realise that if cognition is a system which spans body, brain and environment (e.g. Clark 2008) we need a way for information to flow through this system in order to softly assemble (temporarily couple) the task relevant components. The ecological approach has many of the relevant tools already in place (specifically, methods for identifying the relevant perceptual information and how action systems use this information to coordinate functional responses to a given task; Bingham 1988) and is therefore already ideally placed to support extended, embodied cognition (Barrett 2011). Chemero (2009) has comprehensively covered what a cognitive science grounded in the ecological approach would look like in his recent book. Some minor quibbles aside, we endorse his view and so won't rehash it here.

The second contribution of Gibson's theory (and the focus of this article) is how it stands as an example of what a theory driven research programme in psychology looks like and is capable of. Specifically, the theory serves as a *guide to discovery*, capable of driving forward an empirical research programme by asking the right questions and constraining what counts as a legitimate answer. Instead of simply summarising and describing what happened in an experiment (as is common throughout cognitive psychology), the ecological approach postulates a theory about the nature of perceptual information and what it means that enables researchers to explain their results and make novel predictions that go beyond the current data. In the next section, we will review a recent programme of empirical work on the identity of the information specifying the relative phase between two coordinated rhythmic movements. This work, led by Geoff Bingham in concert with a variety of collaborators, has culminated in a perception-action model of the task in which, for the first time, a specific hypothesis about information features prominently (Bingham 2001, 2004a, b; Snapp-Childs, Wilson & Bingham 2011). The model, and the strategy for assembling it stands as an exemplar of how the ecological approach can *guide* research and it clearly demonstrates the benefits of a theory driven approach to psychology. We will contrast this approach with the phenomenological (descriptive) *dynamic pattern* approach (Kelso 1995; Zanone & Kelso 1994) which focused on simply describing key experimental results and remains unable to make successful predictions about novel experimental procedures.

**Perception, action and coordinated rhythmic movement**

Coordinated rhythmic movement has been a staple of the perception-action literature since the basic task characteristics were described by Kelso (1981; see Kelso 1995 for a detailed overview). The core task is simple: take your two index fingers and move them up and down so that they do the same thing at the same time; this is 0° *mean relative phase* and is easy to produce and maintain over a wide range of frequencies. Now make your fingers alternate; this is 180° mean relative phase, and is also easy to produce and maintain, though over a smaller range of frequencies; at 3-4Hz, under a 'non-interference' instruction, 180° becomes unstable and people typically transition into 0°. Other coordinations (especially the intermediate 90° rhythm) are typically unstable without training and people cannot maintain them in the face of perturbations such as an increase in frequency.

These phenomena were described by the famous Haken-Kelso-Bunz model (Haken, Kelso & Bunz 1985). The model implements a *dynamic pattern* approach to coordination phenomena. It models an abstract dynamical process rather than the task of coordinating two limbs in particular, and the form of the model (two superimposed cosine functions) therefore makes no particular reference to anything about coordination other than being organised with respect to relative phase. Specifically, the two cosine functions describe an energy potential function; this function has local minima at 0° and 180°, with the minimum at 180° being shallower and wider. These minima represent *attractors*, locations within the model's state space that draw behaviour towards themselves, while 90° is represented as an energy maximum, i.e. the energy required to maintain the state is maximal and thus it is unstable. This basic form of the model describes the *intrinsic* dynamics of the system, the preferred state. These dynamics can be altered, however, with learning. In the dynamic pattern approach, learning is described as a phase transition, in which an imposed environmental rhythm (e.g. 90°) is incorporated into the intrinsic dynamics as a third attractor (e.g. Schöner & Kelso 1988; Zanone & Kelso 1992, 1997). This happens after a period of competition for dynamical resources between the intrinsic and imposed attractors, and the phase transition is the resolution of this competition via a reorganisation of the system as a whole.

The research strategy advocated by Kelso is behavioural and dynamical (e.g. Jeka & Kelso 1989). Researchers should identify the *order parameters* of a system along which behaviour is organised (here, relative phase) and determine the nature of the dynamics by investigating the stability of the system in response to perturbations such as frequency scaling and the imposition of a to-be-learned rhythm. Experiments taking this approach have identified critical stability fluctuations as frequency increases, fluctuations which abruptly decrease after a phase transition from, say, 180° to 0°, as well has phase resetting and an inverse frequency-amplitude relation (Kay, Kelso, Saltzman & Schöner 1987; Kay, Saltzman & Kelso 1991).

Overall these results suggest that coordinated rhythmic movements are an example of an autonomous non-linear dynamical system. However, the HKB model and this modelling strategy are entirely phenomenological: the equation is abstract and designed solely to fit the basic pattern of the data. There is no account of the *origin* of the attrac-

tors; if behaviour is organised with respect to relative phase, why are 0° and 180° so easy? Why is 90° maximally difficult? In effect, Kelso's approach provides a clear understanding of *how* rhythmic movement is organised in people, but has no account of *why* it should be this way. This weakness revealed itself when the approach was applied to a phenomenon it wasn't specifically designed to explain: trained performance at relative phases other than 90°.

### Learning (the dynamic pattern approach)

The dynamic pattern approach considers learning to be a process of competition over limited dynamical resources between the intrinsic task dynamics (the HKB model) and extrinsic task demands (an externally paced rhythm). Zanone and Kelso (1994) laid out several predictions about how this competition should unfold when learning various novel coordinations. Learning something close to 0° (e.g. 45°) would be more difficult than learning something an equal distance from 180° (e.g. 135°) because the attractor at 0° would exert a stronger pull on this unstable state and prevent people from being able to maintain it.

This has been tested twice, by Fontaine, Lee & Swinnen (1997) and Wenderoth, Bock & Krohn (2002). Both studies trained participants to perform novel coordinated rhythmic movements that varied in their distance from the two intrinsically stable states (e.g. 36°, 45° 60°). Contrary to the prediction, learning a novel coordination close to 0° was easier (faster and more stable) than learning one close to 180°. Competition from pre-existing attractors does not explain this, but Wenderoth et al suggested that some earlier work from Bingham's lab (Zaal, Bingham & Schmidt, 2000) on the visual perception of relative phase contained the answer. 0° is not stable because there is an attractor there; rather, 0° can be described as having an attractor there because it is stable, and that stability is a function of how relative phase is visually perceived. Information, not dynamics, held out the possibility of an explanation.

### The perceptual basis of coordinated rhythmic movements

Schmidt, Carello & Turvey (1990) demonstrated that the HKB phenomena are still present when the limbs being coordinated belong to different people and the coupling is visual. Bingham then ran a series of visual judgment experiments, in which he asked participants to visually evaluate displays of dots moving at some mean relative phase with varying frequency and amounts of variability in the coordination (Bingham, Schmidt & Zaal 1999; Bingham, Zaal, Schull & Collins 2001; Zaal, Bingham & Schmidt 2000). The results were quite startling. Participants could do the task on average but the variability of their judgments varied in the HKB pattern (low variability at 0°, maximum variability at 90° and intermediate variability at 180°). Thus, 0° was judged to be maximally stable, 180° stable but less so, and 90° maximally unstable even with no added noise; when phase variability was explicitly added, this was only clearly discriminated at 0°, somewhat at 180°, and not at all at 90°. Bingham had shown that the HKB pattern from the *movement* task also showed up in a purely *perceptual* task, sug-

gesting that the pattern in the movement task was being caused by how relative phase is perceived. Wilson, Bingham & Craig (2003) then replicated this result using haptic perception of relative phase.

Following on from this, Wilson, Collins & Bingham (2005a) had people move at 0°, 90° or 180° in order to produce 0°, 90° or 180°, in varying combinations. In the conditions where the mapping was not altered (e.g. moving at 0° to see 0°, or at 90° to see 90°) we saw the HKB pattern. In the transformed mapping conditions, however, movement stability followed the visual feedback. Movements at 90° and 180° were both stabilised if the display showed 0°, while movements at 0° were made less stable if the display showed 90° or 180°. The stability of coordinated rhythmic movements followed the perception of relative phase, and not the relative phase of the movement per se (see also Bogaerts, Buekers, Zaal & Swinnen 2003).

Relative phase is clearly perceivable. The question is how; what is the information for relative phase? There were hints in the literature that relative phase was perceived in terms of the *relative direction of motion*. Relative direction perfectly predicts the movement phenomena; it is maximally stable at 0°, stable but less so at 180° and maximally variable at 90°. In addition, the HKB pattern goes away when relative direction is not defined (e.g. orthogonal movements: Wilson, Collins & Bingham 2005b; Wimmers, Beek & van Wieringen 1992; transformed (Lissajous) feedback: Kovacs, Buchanan & Shea 2009a, b). Gibson made sure to emphasise, however, that it is *always* an empirical question which information people use. Wilson and Bingham (2008) systematically and selectively perturbed all the components of motion that could conceivably specify relative phase (relative speed, relative frequency and relative position). None of the perturbations had any effect other than to add a small amount of noise to judgments of 0° and 180°. In addition, it proved impossible to perturb relative direction without making the relative phase undefined. We therefore concluded that relative phase is perceived in terms of the relative direction of motion.

## A perception-action model of coordinated rhythmic movement

Bingham had now established that the movement phenomena reflect the perception of relative phase. However, remember that skilled actions are perception-action systems – perception is not the entire game (as erroneously claimed by Mechsner, Kerzel, Knoblich & Prinz 2001). Bingham therefore needed to model the full perception-action task in a manner that accurately reflected the actual *composition* and *organisation* of the task dynamic. Bingham's intention was that this model should reflect the ecological approach that had guided the discovery of the identity of the task elements. The model could not simply contain parameters designed to fit the data; everything in the model should represent an empirically identified element of the task dynamic of coordinated rhythmic movement.

The first step was a *task analysis* to identify the dynamical resources that were available to be softly assembled into a task-specific device that produces the observed behaviour (Bingham 1988). This analysis revealed that we need to model two rhythmically moving limbs which are coupled together via perceived relative phase. Rhythmically

moving limbs exhibit very specific properties; they show *limit cycle stability* (their preferred state is periodic), *phase resetting* (they respond to perturbations such as being sped up or slowed down by returning to the limit cycle at a different phase than if they had remained unperturbed), an *inverse frequency/amplitude relation* (as frequency increases amplitude goes down unless instructed otherwise) and a *direct frequency/velocity relation* (Kay, Kelso, Saltzman & Schöner 1987; Kay, Saltzman & Kelso 1991). These characteristics mean that the control of the limbs is non-linear and autonomous (i.e. they are driven as a function of their own behaviour, not of time). Bingham therefore modelled the limbs to-be-coupled as damped mass-springs (following the equilibrium point hypothesis, e.g. Feldman, Adamovich, Ostry, & Flanagan 1990).

Perception enters the model in the coupling function. Each mass-spring is driven by the perceived phase of the other limb, modified by the perception of relative phase (in terms of relative direction) and with noise proportional to the relative speed (Wilson et al. 2005b; Snapp-Childs et al. 2011). Relative direction and relative speed are both state variables and thus can be used to drive a mass-spring autonomously; they are also both kinematic and thus perceivable (Bingham 1988; Runeson & Frykholm 1983; Turvey et al. 1981). The final form of the model (Bingham 2001, 2004a, b) is therefore

$$\ddot{x}_1 + b\dot{x}_1 + kx_1 = c\sin(\Phi_2)P_{12}$$

$$\ddot{x}_2 + b\dot{x}_2 + kx_2 = c\sin(\Phi_1)P_{21}$$

where $P_{ij} = sgn(\sin(\Phi_i)\sin(\Phi_j) + \alpha(x_i - x_j)^3 N_t)$

This model is the first fully perception/action model of coordinated rhythmic movement. It explicitly models both perceptual and action components as having specific forms, and it places these components in a specific organisation with respect to each other. It also makes three specific predictions, all empirically confirmed; that the information for relative phase is relative direction (Wilson & Bingham 2008), that the movement phenomena are a function of perceptual stability (Wilson, Snapp-Childs & Bingham 2010) and that relative speed acts as a noise term (Snapp-Childs et al. 2011).

**The model as guide to future discovery – learning (the ecological approach)**

Like the HKB (and any decent cognitive model) Bingham's model is very successful in explaining the things it is designed to explain. However, it has an extra dimension that the HKB lacks. Specifically, it is the product of a theory-driven process which uniquely demands that the model be built from components that have been empirically determined to matter, and that these components must be built so as to reflect the real composition and organisation of the system at hand. The model does not yet explicitly handle trained performance at, say, 90°; however, recent learning research inspired and guided by the ecological theory the model embodies has met with great success.

The model assumes that movement stability is a function of perceptual ability. This suggests that if people became expert perceivers of 90°, they should then be able to move at 90° with no practice at the task. This is indeed the case (Wilson et al. 2010a).

The model also suggests that learning 90° is unlikely to involve simply improving your ability to detect relative direction at 90°; this variable remains intrinsically variable at 90° and thus cannot support stable action there. The model therefore suggests that people trained at 90° are likely to switch to a new variable; Experiment 2 of Wilson & Bingham (2008) perturbed the performance of the trained observers and found that they had switched to perceiving relative phase as relative position (but only at 90°). Finally, as noted above, Wenderoth et al. (2002) explained their learning rate data in terms of Bingham's perception hypothesis; if the region around 0° is clearly perceived then different states there can be clearly discriminated and learning these states is therefore easier than around 180°.

The model also predicts that there should be transfer of learning between bimanual and unimanual versions of the task because while the coupling in the unimanual case is only in one direction, it is still composed of relative motion information (Snapp-Childs et al. 2011). This is indeed the case; there is transfer, and the form of the transfer matches the predicted stability characteristics of the unimanual and bimanual versions of the task (Snapp-Childs, Wilson & Bingham, submitted). This is quite remarkable; predicting how learning will transfer is a notoriously difficult problem in motor control (Schmidt & Young 1987). The ecological approach the model embodies, with its emphasis on information, makes clear and so far successful predictions about a task it is not explicitly set up to handle because it can base these predictions on a hypothesis about the underlying mechanism.

**Summary**

Sciences need theories, to guide discovery and constrain explanation. Gibson's ecological approach has shown itself capable of supporting productive and successful empirical research across a wide range of tasks and serves as a model for what a theory-driven psychology could achieve. Reviews of other work in this vein can be found in Barrett (2011) and Chemero (2009). We have focused here on Bingham's perception-action model as an exemplar of this research and how the ecological theory underpinning the model successfully guided discovery and ruled out alternative explanations for phenomena. In addition, while the ecological approach is not a complete theory of behaviour, it is a successful theory of perception, and this must therefore be the starting point of *any* analysis of behaviour. By beginning that analysis at the right place (in the opportunities for behaviour in the environment and the information about those opportunities) the ecological approach will inform, enrich and (most importantly) constrain our explanations of behaviour in a principled manner (Chemero 2009).

Gibson's ecological approach therefore continues to have much to offer contemporary psychology, but it remains to be seen if psychologists can accept and work within the constraints of a real theory as they attempt to explain more complex cognition and behaviour. The beauty of such a period of theoretically motivated, hypothesis driven 'normal' psychological science is that if we invest some serious time pushing the theory, looking for cracks, and resisting the temptation to jump ship at the first sign of trouble, psychology will end up in a better place no matter how it pans out. If the theo-

ry breaks, it will have been broken honestly, and for good reasons. If the theory holds up, we will have achieved a lot of progress and begun to act like a real science for a change. Either way, psychology will be a stronger science for the experience.

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# On possibilities for action:
## The past, present and future of affordance research

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**Abstract**

We give a historical overview of the development of almost 50 years of empirical research on the affordances in the past and in the present. Defined by James Jerome Gibson in the early development of the Ecological Approach to Perception and Action as the prime of perception and action, affordances have become a rich topic of investigation in the fields of human movement science and experimental psychology. The methodological origins of the empirical research performed on affordances can be traced back to the mid 1980's and the works of Warren (1984, 1988) and Michaels (1988). Most of the research in Ecological Psychology performed since has focused on the actualization of discretely defined actions, the perception of action boundaries, the calculation of pi-numbers, and the measurement of response times. The research efforts have resulted in advancements in the understanding of the dynamic nature of affordances, affordances in a social context and the importance of calibration for perception of affordances. Although affordances are seen as an instrumental part of the control of action most studies investigating affordances do not pay attention to the control of the action. We conclude that affordances are still primarily treated as a utility to select behaviour, which creates a conceptual barrier that hinders deeper understanding of affordances. A focus on action-boundaries has largely prevented advancement in other aspects of affordances, most notably an integrative understanding of the role of affordances in the control of action.

**Keywords:** Ecological Psychology; Action Selection; Action Boundary; Perception-Action; Experimental Psychology.

James Jerome Gibson stressed that humans and other animals are active perceivers that live and move in a meaningful environment. The objects of perception and action, therefore, according to Gibson, are the meaningful relations between animals and their environment. Gibson called these relations *affordances*. He first introduced the term in 1966 as " (...) simply what things furnish, for good or ill" (Gibson 1966: 285) and further described the theory of affordances in his later work (Gibson 1977, 1979/1986) where affordances were defined as "a specific combination of the properties of its substance and its surfaces taken with reference to an animal"(Gibson 1977: 67)[26][27]. The aim of the current review was to give an overview of the development of almost 50 years of research on affordances.

It was not until two important papers by Bill Warren (1984, 1988) were published before experimental research on affordances could really take off. In these papers Warren defined the role of affordances in the guidance of action and argued for the following sequence in the unfolding of an action: "...the intentions of the actor select an affordance to be realized, a corresponding mode of action by which to realize it, and appropriate laws of control to visually regulate the action" (Warren 1988: 341). More specifically, Warren argued that critical points of affordances may induce phase transitions between different modes of action-- involving stopping the first mode of action and starting another one--which are governed by different *laws of control.* In other words, according to Warren action modes map one-to-one into unique sets of control laws. Warren proposed that in this process of the action guidancetwo different problems need to be distinguished: First, the "*affordance problem*" of how organisms perceive what actions the environment affords in a given situation and on the basis of which action modes are selected; and second, the "*control problem*" of how optical variables regulate actions during a specific task.

## Measuring Affordances

Gibson suggested that an affordance has to be measured relatively to the actor (Gibson 1979/1986: 127-128). That is to say, instead of using arbitrarily extrinsic units (e.g., meters) for environmental properties, affordances should be defined in terms of intrinsic metrics reflecting the animal-environment relation. Warren (1984) operationalized this idea in his study on the climbability of stairs. When taking a property of the animal as a standard for measuring an environmental property that is measured in the same arbitrarily extrinsic unit, the ratio of the two metrics is a dimensionless pi number that reflects the animal-environment relation. For instance, Warren found that

[26]Since the first introduction, different researchers and theorists have commented on and refined the definition of affordances. For instance, Turvey (1992) defined affordances as dispositional properties of the environment that are complemented by dispositional properties of animals, which he coined effectivities; Stoffregen (2003) argued that affordances should be regarded as properties of the animal-environment system rather than as dispositional properties; Michaels (2003) defined affordances as the actions permitted to an animal by environmental objects, events, places, surfaces, people etc.; Chemero (2003) proposed that affordances are relations between the abilities of animals and features of the environment.

[27]In the remainder of this paper we will focus on affordance research that follows the tradition started by Gibson (1979/1986) as the Ecological Approach (cf. Reed 1996).

independent of how tall people are, their action-boundary for the climbability of stairs (the critical height of a riser, such that lower risers are climbable whereas higher risers are not) is at a riser height of .88 times their leg length. Observers were remarkably accurate in the perception of this critical climbable riser height, and also of the optimal riser height (the riser height that could be climbed with least energy expenditure, at .26 times leg length).A biomechanical analysis showed that the number of .88 times leg length was not an arbitrary number: Warren measured the lengths of upper and lower legs, and established that upper leg length was .44 times total leg length, on average. Given this ratio, the number of .88 times leg length equals the distance from the foot to the floor when the upper leg is lifted maximally, touching the trunk. In the years following, the paradigm of looking for critical boundaries (and pi numbers) to study affordances was extended to a broad variety of tasks. To name a few examples, affordance research was performed on sitting (Mark 1987; Mark et al. 1990), passing through apertures (Warren &Whang 1987), passing under barriers (van der Meer 1997; Wagman&Malek 2008, 2009; Stefanucci et al. 2010), reaching (Carello et al. 1989; Rochat 1995), stepping over obstacles (Pufall & Dunbar 1992) and gaps(Jiang & Mark 1994) and walking-up slopes (Kinsella-Shaw et al. 1992).

**Towards An Action Scaled Approach To Affordances**

The early work on affordances was focused on, the so-called, *body scaled* affordances: affordances defined in terms of a geometric relation between the actor and the environment. However, as later researchers would argue, one's action capabilities are not merely body scale dependent, but are rather a combination of geometric, kinematic and kinetic characteristics. To address this idea, over the years, researchers have come up with experiments following an *action scaled* approach. For instance, Konzcak et al. (1992) reconsidered the situation of climbing stairs by taking into account not only leg length but also kinematic and kinetic factors such as hip flexibility and leg strength to calculate action boundaries. This action-scaled approach provided a better fit of the actual action boundary of maximal riser height than Warren's body-scaled measure. Perceived maximal riser height corresponded well with achieved maximal riser height, both in younger and older adults. The older participants were even more accurate in perceiving their action boundaries than the younger adults. Konzcak et al. (1992) argued that the reason could be that older people might have to be more accurate in order to avoid falling, while younger adults have the action capabilities to correct the results of a miscalculation.

Another example of an action-scaled approach are the studies of Pepping and Li (1997,1999, 2000) investigating the perception of action boundaries in the volleyball block. Pepping and Li showed that the action of overhead reaching and blocking in volleyball is, as in many sports situations, dependent on both geometric characteristics (reach height of the player) as well as kinetic characteristics (jumping ability of the player) and that participants accurately perceive their maximum overhead reach and block height. In order to investigate whether kinetic variables could be perceived, Pepping and Li (1999, 2000) designed a series of experiments in which they either changed the action capabilities, by giving the participants' a weighted belt to wear

which added their mass, or altered the kinetic requirements of the task by using different types of floors. The changes resulting in afforded actions led to an adaptation of the perceived action boundaries (Pepping and Li 1999, 2000).

**Calibration And The Dynamic Nature Of Affordances**

One's action capabilities can change over time and researchers have investigated whether people's perception adapts to these changing action capabilities. The process of recalibration to changed action capabilities was first shown by Mark (1987), in a study on sitting and stairs climbing. The participants of the study were wearing 10 cm blocks under their feet while judging their maximum seat height and their maximum riser height. Participants initially overestimated their action capabilities for stairs climbing and underestimated their action capabilities for sitting, consistent with the situation of not wearing blocks under their feet. After some experience with the blocks, they were able to recalibrate their perception of the new action boundaries for sitting and stair climbing, consistent with the change in their action capabilities: a process that could not be explained by a changed perception of the blocks, since participants consistently overestimated the height of the blocks.

As mentioned, Pepping and Li (1999, 2000) found that participants were able to rescale their perceived maximal reach when their action capabilities changed. Similar observations were made in the perception of affordances for standing on an inclined surface while changing the location (Malek&Wagman 2008) or the height (Regia-Corte &Wagman 2008) of the centre of mass of participants. When weights were attached to their body, participants were able to recalibrate to the changes in action capabilities, independently of whether they visually saw the surface or were able to haptically explore the inclination of the surface.

Most of the research on changed action capabilities involved familiar tasks for healthy adults. In contrast, a number of studies addressed the ability of participants to perceive affordances when using a wheelchair. The use of a wheelchair is a task most healthy adults are not familiar with (cf. Stoffregen et al. 2009). In a study by Higuchi et al. (2004) participants unaccustomed to the task of using a wheelchair judged the passability of apertures when rolling in a wheelchair. Systematic overestimation of their abilities occurred. After 8 days of practice passing through apertures the estimations improved, but the overestimation of the action capabilities did not completely disappear. The practice involved in this study only consisted of the task of passing through apertures, the task participants had to judge. Stoffregen et al. (2009) allowed some perceivers two minutes of self-controlled wheelchair motion before judging the minimum height under which they could pass in a wheelchair. Even when this practice did not involve the task of passing under, it still resulted in improved estimation of this action boundary compared to estimations of perceivers who did not practice. No improvement across trials occurredwhich implies that learning occurred prior to the judgments when allowed to practice and no learning occurred after the beginning of the judgment sessions (Stoffregen et al. 2009).

### The Role Of Exploration And Movement In The Perception Of Affordances

The previous results raised the question of how the participants recalibrated their perceptual boundaries. In order to get more insight into this process Mark et al. (1990) had participants judge their maximum seat height (with and without the 10 cm blocks, see Mark 1987) while being, more or less, restrained in visual and physical exploration. When allowed to move (even just leaning forward and sideward and rotating their head) the observers were able to improve their estimations of maximum seat height. However,when exploration was constrained, less or even no recalibration occurred. Exploration was needed both in the condition with blocks as well as in the normal standing condition in order for recalibration to occur. The results suggested that participants had to determine their action capabilities each time before performing an action, even if they were familiar with the task (Mark et al. 1990).

The necessity of exploration (see for instance Mark 1990; Stoffregen 2009) implies that at least some movement is needed in order to pick up information about action capabilities. Oudejans et al. (1996) investigated the role of movement in the perception of catchableness of fly balls. They had participants judge whether balls were catchable in two different conditions. In a “stand” condition participants were not allowed to move while making the judgment. In a “move” condition they were allowed to move for a short period of time before having to judge the catchability of the ball. Even experienced baseball players had difficulties judging the catchability when not allowed to move during the task. The results demonstrated that movement was needed in order to perceive their action capabilities (Oudejans et al. 1996; but see Fajen et al. 2011). Additionally, Pepping and Li (2008) found that haptic exploration of the ground surface influenced the perception of the action boundaries in a jump-and-reach task. Although the perception of the action boundaries was not necessarily more accurate when the participants were allowed to haptically explore the surface (a constant overestimation of different surfaces occurred), the judgments were more consistent and less variable than the judgments without the possibility to explore.

### Developmental Approaches To Affordances

In the previous part we addressed the process of recalibration of perceptual boundaries after changes in action capabilities. For example, in the studies of Mark (1987, 1990) and Pepping and Li (1999, 2000, 2008) the participants faced temporary changes in their action capabilities resulting from external factors (e.g. blocks under their shoes or increased mass of the participants). One of the more permanent changes in action capabilities is related to the process of growing older. Every day children have to adapt to changes in their action capabilities caused by development of skills and growth. Some of the classic studies on the development of visually guided locomotion in infants involved the detection of a visual cliff by infants. In these studies, infants balked at the edge of a visual cliff despite the presence of a Plexiglas table preventing them from actually falling (e.g., E. J. Gibson &Walk 1960, and Walk &E. J. Gibson 1961; for an overview of visual cliff studies, see, for instance, Adolph et al. 1993a and Adolph &Eppler 1998).

Multiple studies considered the perception of affordances of walking infants as well as crawling infants. E. J. Gibson et al. (1987) found a difference between walking and crawling infants in the perception of traversability of different surfaces. Walking infants showed longer initiation times and more exploration (both visual and haptic) when facing a deformable surface as compared to a standard surface. Crawling infants did not exhibit such differences. When given the choice between the surfaces, walking infants displayed a preference for the standard surface that afforded walking, whereas crawling infants did not exhibit a preference for one type of surface. This shows that infants detected the affordances for locomotion in a particular action mode (E. J. Gibson et al. 1987). The analogous conclusion can be drawn regarding infants' behaviour on slopes. In a series of experiments, Adolph and colleagues studied how walking and crawling infants adapted their locomotion when facing slopes of varying steepness (e.g., Adolph 1995; Adolph et al. 1993b; Eppler et al. 1996). Walking infants walked down a shallow hill of 10 degrees but slid down or avoided a risky steep hill of 36 degrees which are appropriate choices for the different degrees of the slant (Eppler et al. 1996; see also Adolph 1995). Also, Adolph's (1997) longitudinal study of infants' ability of crawling and walking on slopes showed that crawling infants gradually improved their judgments of risky slopes until these were near perfect in their last week of crawling. Next, after the transition from crawling to walking, all infants displayed a sharp decline in the accuracy of their judgments of risky slopes, after which their judgments improved again just as when they had been crawling. That is to say, infants became increasingly capable of adapting their mode of locomotion to the properties of the slant relative to their own locomotor proficiency, but this improved ability to judge slopes did not transfer from crawling to walking (Adolph 1997). Interestingly, when loading the infants with extra weight, infants changed their actions in accordance with this change in action capabilities by treating a slope that was a safe one in normal conditions as risky in the extra-weight conditions (Adolph and Avalio 2000).

Other studies focused on the influence of walking experience on the action boundaries of children. In a barrier crossing task (Schmuckler 1996) and a gap crossing task (Zwart et al. 2005) walking experience was related to the observed thresholds for barrier and gap crossing. Schmuckler (1996) suggested that skill and experience might play critical roles in tasks in which the actor is not an expert and that factors such as body size might become more important in familiar, well-practiced tasks.

Whereas many affordance studies have been performed on infants and toddlers as well as on adults, only few studies have looked at the developmental changes between infancy and adulthood. Plumert (1995) compared 6 and 8-year-old children with adults, and demonstrated differences in the accuracy between adults' and children's judgments whether they could perform different tasks (e.g. reaching and stepping tasks). Whereas adults overestimated their ability if it was just beyond their action capabilities, children also overestimated situations that were well beyond their abilities. Allowing the children to practice yielded improvements in the perception of the conditions that were well beyond their abilities for the 8-years-old, but not for the 6-years-old. Furthermore, 6-year-olds with less accurate judgments of their action capabilities had experienced more serious accidental injuries in their daily life (Plumert 1995).

### The State Of The Actor In Relation To Perceiving Affordances

In the previous parts of this review, studies investigating different factors influencing action capabilities have been discussed. In their daily life, people also have to readapt to, often subtle, internal changes caused by, for example, fatigue or injury. Pijpers et al. (2007) studied the influence of fatigue on the perception of action possibilities for climbing on a climbing wall. Their results showed that in case the exertion led to a change in the actual action capabilities, perceived maximal reaching height changed accordingly. When the perceived exertion was less, however, no changes in the actual action capabilities occurred, and in that case the perceived maximal reach did not change. This indicated that people do not base their estimates of the action capabilities on the perception of fatigue per se, but on their actual action capabilities (Pijpers et al. 2007).

Emotions are another example of a change in the internal state of the actor. Bootsma et al. (1992) studied the effects of anxiety on the perception of reachability of approaching balls. Anxiety did not influence the perception of the affordance itself. That is, the location of the action boundary for reaching remained the same. This was in line with the expectations, since anxiety did not affect the action capabilities of the participants, the judgments should not be altered. However, as expected by the authors, more variability in the perception of the action capability occurred. The conclusion of the authors was that anxiety did lead to decreased accuracy in the pickup of the information that specifies the affordance of reachability of passing balls (Bootsma et al. 1992). The notion that anxiety can change action capabilities in some situations was shown by Pijpers et al. (2006), who investigated whether actual changes in the action capabilities of climbers induced by anxiety would lead to an adaptation of their judgments of their maximal reach in a climbing task. Indeed, the decrease in actual maximal reach in the high-anxiety condition did result in lower perceived maximal reaches. Subsequently, Pijpers et al. investigated whether the anxiety would also lead to differences in realizing the action possibilities. Participants did select other actions in the high-anxiety condition, but other factors than the perceived change in action capabilities might have contributed. Another experiment in the same study revealed that narrowing of attention caused by the anxiety might have played a role as well (Pijpers et al. 2006).

### Social Affordances

Not only the perception of affordances for oneself, but more recently also the perception of affordances for others have become a point of interest. In daily life, people encounter plenty of situations that require interaction with other people. In some situations one even has to anticipate actions of others and, thus, on the action capabilities of others. Young children (three-to-five-years old) are already sensitive to the perception of action capabilities of others in the task of reaching (Rochat 1995). Rochat showed that children correctly judged the maximal reaching distance of an adult to be greater than their own maximal reaching distance. Whereas Rochat did not frame his

study in terms of action capabilities per se, Stoffregen et al. (1999) did. They considered intrinsic scales for the observed person as well as for the observer in the judgment of maximal and optimal sitting height of small and tall people. Observers in Stoffregen et al.'s study were able to base their judgments on the action capabilities of the different persons rather than on their own action capabilities. This implies that people are not only able to differentiate between the action capabilities of another person and themselves, but can also judge the difference in action capabilities between different persons (see also Mark 2007, for other examples).

As discussed before, the ability to recalibrate after a change in action capabilities is an important factor in the perception of affordances for oneself. In the case of the perception of affordances for others, the question is whether observers are able to recalibrate to the information specifying changed action capabilities. Ramenzoni et al. (2008) had people perform a reach-with-jump task to investigate recalibration in the perception of the action capabilities of other persons. The action capabilities of the observed person were changed by attaching weights around the ankles, which was either visually apparent for the observer or hidden underneath the person's clothes. Even when observers were not explicitly aware of the extra weight attached around the ankles of the person, they gave lower estimates of the maximal reach-with-jump as compared to the situation without the extra weight. Ramenzoni et al.'s results demonstrated that people are sensitive to a change in the action capabilities of others. This is impressive considering the fact that the observers did not actually see the person perform the reach-with-jump task, but only saw the person walking around.

In order to accurately judge affordances for others it matters what action the observer sees the person performing. Ramenzoni and colleagues (2010) compared the situation in which the person rotated the torso with the situation in which the person lifted a weight by squatting. The latter is an action related to the action of jumping whereas the former is not. Observing the person performing the nonrelated action did not yield an improvement in the perception of the action capabilities of the person; in contrast, observing the person performing the action related to the task to be judged did help to improve judgments.

Expertise might also play a role in the proficiency to judge affordances for others. Weast et al. (2011) addressed this issue by looking at the difference between basketball players and non-basketball players in the perception of sport-relevant (maximum standing-reach and reach-with-jump heights) and non-sport-relevant affordances (maximum sitting height). Basketball players were more accurate in the perception of the maximal jump and reach of others than the non-basketball players. Furthermore, they showed improvement in their judgments of the maximal jump and reach after exposure to kinematic information, whereas non-basketball players did not. Weast et al. suggested that this might be due to a greater sensitivity of basketball players to kinematic information reflecting the action capabilities of others.

Until now, we discussed studies on the perception of affordances for others, in which no interaction between the observers and observed people took place. Recently, also the affordances for joint action have become a point of the interest for study. For instance, the participants in the study of Richardson et al. (2007) judged whether they

would grasp planks of wood using one hand, two hands or together with another person. When comparing these judgments with actual performance, one-handed grasping, two-handed grasping, or two-person grasping could be mapped onto relevant body dimensions (i.e., the size of hands and arms). When scaling plank length, observed patterns in the transitions between using one or two hands were similar to observed patterns in the transitions between using two hands or two persons, at similar action-scaled ratios. These results show that people are not only able to perceive intrapersonal affordances but they are also capable of perceiving interpersonal affordances in grasping. Similarly, Davis et al. (2010) investigated the action boundaries of dyads walking through an aperture. For both dyads in which the observer took part as well as for dyads consisting of two other persons the observer's perceived action boundary for the dyad was smaller than the sum of the individual action boundaries. This indicates that observers were sensitive to the affordance for joint action, rather than just mentally adding the individual action boundaries.

**An Alternative Approach To Affordances**

The affordances research reviewed until here followed the approach as first introduced by Warren (1984,1988). At about the same time that this line of research originated, another approach, forwarded by Michaels (1988), combined the theory of affordances with work on stimulus-response compatibility in choice reaction times. Stimulus-response (s-r) compatibility refers to the degree to which a set of stimuli and associated responses are naturally related to each other. Michaels (1988) explored the relationship between s-r compatibility phenomena and the theory of affordances. Stimulus-response compatibility occurs when responses are faster for particular stimuli than for other stimuli. A well-known example is spatial s-r compatibility which is observed when the spatial arrangements of stimuli are responded to faster by certain spatial arrangements of responses (Fitts& Seeger 1953;or an overview of the ecological approach to s-r compatibility, see Michaels &Stins 1997). Michaels hypothesized that the detection of affordances might be manifested in the speed by which responses are made, thus "responses afforded in certain situations ought to be faster than responses not afforded" (Michaels 1988: 231-232). To test this hypothesis, a series of experiments was conducted which tested whether or not an object moving toward one hand would lead to faster responses with that hand than with the other hand, even though it might be closer to the other hand and,thus, have positional compatibility. The results revealed that participants reacted faster with the hand that could more easily intercept the object, providing support for the idea that spatial compatibility effects reflect the perception of possibilities for action. In a number of experiments, Stins and Michaels (1997, 2000) extended the approach by investigating the differences among response modes varying in level of compatibility. Different compatibility effects were found for the different response modes (pressing a button versus using a joystick: Stins& Michaels 1997; actual reaches versus button pressing: Stins& Michaels 2000).

Response times have been taken in account by other researchers as well. Fitzpatrick et al. (1994) had participants judge whether slants afford standing, and found an increase in response times and a decrease in confidence about the judgments the closer

to the action boundary. These results, together with the approach of Michaels (1988), led Smith and Pepping (2010) to consider movement initiation times, rather than judgment response times. They studied a reaching task, involving posting small balls through apertures of varying size and showed that judgment response times and reaching movement times were longer and more variable when approaching the action boundary. Since initiation time appeared to be highly sensitive to the location of the action boundary and optimal regions, Smith and Pepping argued that it offers a metric of affordance perception.

In most studies on affordances the perceived action boundaries are compared with the actual action boundaries. Pepping and Li (2005) hypothesized that the often-observed systematic errors between perceived and exact actual action capabilities might have been related to the means by which perceptual performance is measured. Therefore, in their study, both verbal as actual actions were used as response measures for judgment of overhead reachability. The results suggest that a perceptual judgment task is different than actually acting on affordances (Pepping & Li 2005).

**Concluding Remarks: The Future**

In this review we gave an overview of the affordance research in the past and in the present. The concept of affordances was introduced by J. J. Gibson, and most clearly so in his 1979 book entitled 'The Ecological Approach to Visual Perception' (1986/1979). Although research on affordances predates J.J. Gibson's conceptual work and can even be traced back to the seminal work of his wife E.J. Gibson (see E.J. Gibson 2002), the most influential empirical work on affordances can be traced to the pioneering work of Warren (1984, 1988) and Michaels (1988) Since then, the methods and approaches proposed by these studies have served as a guide to discovery. They have inspired many researchers to generate experiments in a variety of tasks, which resulted in a great amount of knowledge on different aspects of affordances. Most of the studies have continued to approach the study of affordances with similar methods -- that is, using discretely defined actions, and focusing on pi-numbers and action boundaries -- as first demonstrated in the classical studies on affordances (e.g. Warren 1984; Warren &Whang 1987; Mark 1987). Examples are the results of studies emphasizing the dynamic nature of affordances and those examining the ideas of recalibration, and perception of social affordances. The majority of studies focus on action *boundaries*. That is to say, they have aimed to establish whether some action is possible or not and whether participants are able to perceive this.

In this review we focused on what Warren called '*the affordance problem*' and we can conclude that most of the studies on affordances have followed a more or less traditional approach, using and extending the paradigms as first introduced by Warren (1984, 1988) which mainly focused on the perception of action boundaries. This focus on action boundaries leads to the impression that the primary behavioural utility for affordances is for the selection of behaviour, rather than the continuous control of action (for similar arguments see also Stoffregen 2000 and Smith 2009). This poses and important challenge to affordance research as results so far seem to emphasize a di-

chotomous view of possible and impossible actions which, obviously, is not sufficient to explain human behaviour. Given this shortcoming, what should the future of affordance research hold?

An answer to this question can possibly be found in the paper that was so instrumental in providing the methodological tools for most of the affordance research reported here. In his seminal 1988 paper Warren proposed that affordances have to be selected and that subsequently the mode of action and the laws of controls are selected. Furthermore, Warren argued that the affordance problem is directly tied to the '*control problem*' (Warren, 1988). A study on the control problem would focus on the laws of control by which our actions are being controlled. (i.e., 'information-based control'). From an information-based perspective the role of perception is to detect information in the ambient flow fields (e.g., in the optic flow) that can be used to guide movement according to a law of control (e.g., see Fajen 2007a). Examples of this information in the optic flow are the optical variable *tau* that specifies time-to-contact, and *tau-dot* (e.g., see Lee 1976, 1980)and optical acceleration for catching fly balls (e.g., see Chapman 1968; Michaels &Oudejans 1992; Todd 1981; Zaal& Michaels 2003). Historically Warren can be seen to have marked the start of separate research traditions into either the control problem or the affordance problem.

Research that links the affordance problem and the control problem as defined by Warren (1988) is scant. A noticeable exception is Fajen's (2005, 2007a) affordance-based control approach that sets out to show how action-capabilities play a role in the control of action. For instance, in the control of braking, Fajen (2007a) argued that people are sensitive to their action boundaries. This sensitivity is not captured in the typical information-based control explanation of keeping *tau dot* at a required value (e.g., see Yilmaz & Warren 1995). People are not simply using a control that can be framed in terms of (not) braking hard enough to avoid a collision, but people's control also takes into account their action. In other words, Fajen argues that the existing information-based approach fails to capture the limits of people's action capabilities. For example, the *tau-dot* model assumes that there are no limits to the amount of deceleration, but in real life actions these limits always exist (Fajen 2007a, for other examples on affordance based control see e.g. Fajen 2008, 2007b; Fajen&Devaney 2006, Bastin et al. 2010).

Fajen's work on affordance-based control seems a promising start to the unification of the affordance problem and the control problem. Note though, that also in affordance-based control the focus remains on the execution of discrete actions and on distinguishing possible and impossible actions when performing one specific task. Although the theory of affordance based control nicely shows the role affordances might play in the control of action, it remains relatively silent on the role of control in the perception of affordances. What is more, since its focus is on control, the research on affordances based control does not yet provide us with insight into how the affordance problem is solved; that is, as defined by Warren (1988), how an actor *selects* an affordance to be realized.

In summary, after J.J. Gibson's conceptualisation, Warren (1984, 1988) and Michaels (1988) have been instrumental in shaping the research on affordances. Reviewing the empirical literature, we conclude that research on affordances thus far seems to emphasise perception of action-boundaries in discrete actions which has prevented researchers from taking into account other aspects of affordances, most notably the link between affordances and the control of action. Warren's conceptualisation of the affordance problem and the control problem (Warren, 1988) has thus far not invited much research that looks at affordances and control in an integrated manner, with a notable exception of Fajen's research on affordance based control. Historically, we feel that Warren (1988) marks both the start as well as the future of research on affordances and that integrative study of affordances and control of action deserves to be put high on the future research agenda of Ecological Psychology.

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# Affordances for robots: a brief survey

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**Abstract**

In this paper, we consider the influence of Gibson's affordance theory on the design of robotic agents. Affordance theory (and the ecological approach to agent design in general) has in many cases contributed to the development of successful robotic systems; we provide a brief survey of AI research in this area. However, there remain significant issues that complicate discussions on this topic, particularly in the exchange of ideas between researchers in artificial intelligence and ecological psychology. We identify some of these issues, specifically the lack of a generally accepted definition of "affordance" and fundamental differences in the current approaches taken in AI and ecological psychology. While we consider reconciliation between these fields to be possible and mutually beneficial, it will require some flexibility on the issue of direct perception.

**Keywords:** affordance; artificial intelligence; ecological psychology; Gibson; robotics.

## 1. Introduction

An ecological approach to the design of robotic agents can hold significant appeal for researchers in the area of artificial intelligence (AI). Embodied agents situated in a physical environment have access to a wealth of information, simply by perceiving the world around them. By exploiting the relationship between the agent and its environment, designers can reduce the need for an agent to construct and maintain complex internal representations; designers can instead focus on the details of how the agent interacts directly with the environment around it. The result is more flexible agents that are better able to respond to the dynamic, real world conditions. The ecological approach thus appears well suited to the design of embodied agents, such as mobile autonomous robots, where the agent may be required to operate in complex, unstable, and real-time environments.

First proposed by psychologist J.J. Gibson (1966), the concept of affordances serves as a basis for his theories of ecological psychology. Though "affordance" is often informally described as "an opportunity for action," there is as yet no commonly accepted formal definition of the term. In *The Ecological Approach to Visual Perception*, Gibson writes:

> *The affordances of the environment are what it offers the animal, what it provides or furnishes, either for good or ill. The verb to afford is found in the dictionary, but the noun affordance is not. I have made it up. I mean by it something that refers to both the environment and the animal in a way that no existing term does. It implies the complementarity of the animal and the environment. (Gibson 1979: 127)*

Despite a lack of agreement on what exactly an affordance is, a number of attempts have been made to apply ecological concepts to the design of artificial agents. In many cases, researchers in AI have drawn direct inspiration from ecological psychology, while in other cases, they have independently arrived at approaches that, though they may differ in some respects, are in many ways compatible with Gibson's proposals.

Often, however, it is apparent that psychologists and AI researchers have very different approaches to the problem of understanding what affordances are and how they are utilized by agents, whether organic or artificial. Thus, the purpose of this article is twofold. Our first goal is to provide a brief survey of existing work in the area of artificial intelligence, for the benefit of researchers in both fields. This survey is presented in section 2. Our second goal, addressed in section 3, is to identify some of the main issues that can complicate attempts to reconcile the approaches of ecological psychology and of AI, and that may inhibit communication across the two domains – in particular, the role of Gibson's theory of direct perception. In section 4, we conclude with some speculation as to the future of affordance-based approaches in AI.

## 2. The ecological approach in AI

In designing artificial agents, several successful patterns for control and coordination of perception and action have emerged. Some of these approaches share an important characteristic - a clear emphasis on utilizing the environment, and the agent's interaction with it, to reduce the complexity of representation and reasoning. This characteristic is founded on an ecological view of the agent - an entity embodied in a world rich with observable cues that can help guide the agent's behavior. As summarized by Brooks, "the world is its own best model" (Brooks 1990: 5).

We begin with a brief overview of the AI literature, focusing on agent design paradigms that incorporate elements of the ecological approach. While researchers in AI may not always make exactly the same choices Gibson might have, there is much here that will be familiar to a reader with a background in ecological psychology.

### 2.1. Agent design paradigms

Sensing, planning (or reasoning), and acting are three major processes that an agent needs to carry out. In traditional deliberative systems (Maes 1991), these are modeled as distinct components, typically activated in cycles with a linear sense-plan-act sequence (Gat 1998). This methodology has allowed for fairly independent development of the three components, especially domain-independent planners that have been able to exploit advances in general problem-solving and formal logical reasoning (Fikes et al. 1972; Newell & Simon 1963; Sacerdoti 1974).

But such an organization has two significant implications. Firstly, decoupling of the processes creates the need for an abstracted internal representation of the environment (partial or complete) to pass information from the perceptual component to the planning system; this intermediate 'buffer' can potentially become a disconnect between the real state of the environment and the agent's beliefs. Secondly, plan failure is treated as an exception that is usually handled by explicit re-planning. With the uncertainty and unpredictability inherent in the real world, these aspects can limit the versatility of physical robots. These challenges have been addressed by researchers through refinements such as modeling uncertainty and nondeterminism (Bacchus et al. 1999), and dynamic planning (Stentz 1995; Zilberstein & Russell 1993).

The ecological view presents a fundamentally different approach to agent design, relying heavily on simple, efficient perceptual components (as opposed to complex mental constructs) and common underlying mechanisms for sensing, reasoning, and acting (Brooks 1986). Planning and execution in such systems is usually a tightly coupled process, with the agent constantly recomputing the best course of short-term action, simultaneous with the execution of the current task. This reduces dependence on a control state that keeps track of the agent's progress in a sequence of actions that might rely on potentially out-of-date information.

An ecologically-aware agent can demonstrate flexibility in the face of changing conditions, while still performing complex behaviors. Chapman (1991) demonstrates, using a simulated environment, how ecological principles can help an agent abort a routine that is no longer appropriate, re-attempt a failed action, temporarily suspend one task in favor of another, interleave tasks, and combine tasks to simultaneously achieve multiple goals. Similar characteristics have emerged in a number of physical robotic systems that follow different methodologies and design patterns, yet embody principles compatible with the ecological perspective.

Action-oriented or task-driven perception (Arkin 1990) is one approach roboticists have used to deal with inherent uncertainty in the real world. Knowledge of a robot's current situation, intended activity, and expected percepts can help introduce enough constraints to make perception tractable and accurate. Furthering this approach, Ballard (1991) argues with the Animate Vision paradigm that the ability to control visual input (specifically, gaze) enables the use of environmental context to simplify tasks such as object recognition and visual servoing. This has been reiterated by Brooks and Stein (1994) and validated by some later systems (Gould et al. 2007; Kuniyoshi et al. 1996; Scassellati, 1999).

The task-driven methodology can be generalized to include other aspects of the agent's current *situation*. Chapman (1991) and Agre (1987) illustrate how the affordances of an environment can be characterized within an overall theory of situated activity, which is one way of conceptualizing ecological elements. They also demonstrate how instructions given to artificial systems can refer to indexical functional entities, i.e. pointers to real-world objects specified directly in terms of their characteristics as relevant in the current situational context, instead of absolute identifiers. Properties of candidate objects, including their affordances, help disambiguate references present in such instructions, e.g. "it" in "pick it up" can only refer to objects that can be picked up.

Other ecological elements have also received attention in robotics. In their work on the humanoid robot Cog, Brooks et al. (1997) emphasize the need to consider bodily form when building representation and reasoning systems to control robots. In behavior-based robotics, Matarić (1994, 1997) emphasizes the learning aspect of behavior selection, and notes that this amounts to learning the preconditions for a behavior. In addition, reasoning about behaviors – especially in the context of planning – requires that behaviors be associated with properties or states of the environment. This kind of reasoning enables robots to "think the way they act" (Matarić 2002).

A number of researchers have even applied Gibson's concept of optic flow to autonomous robotic agents. For example, Duchon et al. (1998) describe the design of mobile robots that utilize optic flow techniques not only for obstacle avoidance, but to also implement predator-prey behaviors that allow one agent to chase after another as it attempts to escape.

### 2.2. Affordance-based approaches

Most of the research cited up to this point does not make direct reference to Gibsonian affordances. In this section, however, we consider examples from the AI literature where the focus is specifically on agents designed to utilize affordances. While there may be some disagreement as to how compatible the results are with the Gibsonian approach, generally speaking, the goal has been to apply concepts from ecological psychology to develop better agents.

Recent work in AI has led to the development of robots capable of exploiting affordances in support of a range of behaviors, including traversal and object avoidance (Çakmak et al. 2007; Erdemir et al. 2008a, 2008b; Murphy 1999; Şahin et al. 2007; Sun et al. 2010; Ugur et al. 2009, 2010), grasping (Cos-Aguilera et al. 2003a, 2003b, 2004; Detry et al. 2009, 2010, 2011; Kraft et al. 2009; Yürüten et al. 2012), and object manipulation, such as poking, pushing, pulling, rotating, and lifting actions (Atil et al. 2010; Dag et al. 2010; Fitzpatrick et al. 2003; Fritz et al. 2006a, 2006b; Rome et al. 2008; Ugur et al. 2011, Sun et al. 2010; Yürüten et al. 2012).

Our own interests relate primarily to the design of agents capable of utilizing the affordances of tools. Tool use is briefly considered by Gibson (1979) and by Michaels (2003), and has recently been studied by Jacquet et al. (2012), but it has received relatively little attention from ecological psychology. There is, however, a small but growing body of work on tool-related affordances in AI (e.g. Guerin et al. 2012), including studies of the affordances of tools used for remote manipulation of targets (Jain & Inamura 2011; Sinapov & Stoytchev 2007, 2008; Stoytchev 2005, 2008; Wood et al. 2005) and the use of external objects for containment (Griffith et al. 2012a, 2012b). Recent work in our own lab has focused on systems for identifying the low-level affordances that support more complex tool-using behaviors, such as the physical couplings between a screwdriver and the slot of a screw and between a wrench and the head of a bolt (Horton et al. 2008, 2011).

While most of these affordance-based systems utilize embodied agents in control of physical robots, others employ simulation environments or use simulation in addition to physical interaction (Cos-Aguilera et al. 2003a, 2003b, 2004; Erdemir et al. 2008a, 2008b; Fritz et al. 2006a, 2006b; Jain & Inamura 2011; Rome et al. 2008; Şahin et al. 2007; Sinapov & Stoytchev 2007, 2008; Ugur 2011).

As with much of the work in ecological psychology, the majority of these systems focus on visual perception, through either physical or simulated cameras. A few systems employ additional forms of input, however. For example, Atil et al. (2010), Griffith (2012a, 2012b), Murphy (1999), Şahin et al. (2007), and Ugur et al. (2009, 2010, 2011) utilize range finders for depth estimation, and the system described by Griffith (2012a, 2012b) also makes use of acoustic feedback. And in Atil et al. (2010) and Yürüten et al. (2012), the systems take labels assigned by humans to objects and actions as additional input.

Whether physical or simulated, many of these systems share a common approach in the utilization of exploratory behaviors, or "babbling" stages, in which the agent simply tests out an action without a specific goal, in order to observe the result (if any) on its environment. Through exploratory interactions, the agent is able learn the affordances of its environment largely independently. However, the affordances the agent can discover will be dependent not only on its physical and perceptual capabilities, but also on the types of exploratory behaviors with which it has been programmed (Stoytchev 2005).

Perhaps the feature most relevant in the context of this document is the almost universally shared view of affordances as internal relations between external objects and the agent's own actions. This perspective conflicts with the approach advocated by Gibson. For example, Vera and Simon (1993) suggest an interpretation of affordances that is very different from the view commonly held in ecological psychology, based on an approach of the sort Chemero and Turvey (2007) classify as "representationalist" (as opposed to "Gibsonian"). Responding to proponents of situated action, an approach to cognition and artificial intelligence with similarities to ecological psychology, Vera and Simon argue that advocates of such approaches greatly underestimate the complexity of perception. Rather, they suggest that the apparent simplicity of perception is the result of complex mechanisms for encoding complicated patterns of stimuli in the

environment. In this view, affordances are the internal functional representations that result from this encoding process; affordances are "in the head" (Vera & Simon 1993: 21).

A more recent formalization of this viewpoint is formulated by Şahin et al. (2007) and Ugur et al. (2009). They begin their formalization of affordances by observing that a specific interaction with the environment can be represented by a relation of the form (effect, (entity, behavior)), where the "entity" is the state of the environment, the "behavior" is some activity carried out by an agent in the environment, and the "effect" is the result. A single interaction leads to an instance of this relation. Multiple interactions can be generalized such that the agent becomes able to predict the effects of its behaviors on different environment entities. Thus, affordances can be considered to be generic relations with predictive abilities.

Additionally, we note that some of the systems we have mentioned are designed to explicitly assign objects and actions to categories (e.g. Sun et al. 2010). As the rejection of the need for categorization in the perception of affordances is a point emphasized by Gibson (1979), this, along with the view of affordances as internal relations, is another area that may cause conflict between the AI and ecological psychology communities.

As the research cited here illustrates, affordance-based approaches have been successfully applied to a number of problems in artificial intelligence. In doing so, however, AI researchers have often employed their own interpretations of ecological concepts like affordances – interpretations that sometimes differ significantly from those of ecological psychology.

Many possibilities remain for applying affordance-based approaches to the design of artificial agents. Thus far, many of the studied applications have been relatively basic, e.g. focusing on obstacle avoidance and pushing objects around on a surface. As more capable robotic agents are developed, able to employ tool use and other increasingly complex behaviors, we anticipate new opportunities for further exploring these approaches.

## 3. Open issues

In this section, we begin with a brief discussion of one of the first problems encountered by researchers in AI when studying the concept of affordances. Specifically, what do ecological psychologists mean by "affordance"? We then identify some of the additional issues that can arise when trying to reconcile the ecological approach with the demands of implementing an artificial agent.

### 3.1. Defining "affordance"

Informally, affordances are often described as "opportunities for action." However, even within the ecological psychology community, there seems to be little consensus on how this concept can be understood more formally. Gibson's own ideas on the subject evolved over the course of decades. For example, Jones (2003) traces the origins of the concept back to the work Gibson did in the 1930's, and argues that Gibson's thinking on the subject was still evolving at the time of his death in 1979.

Gibson's most extensive writing on the topic of affordances comes from *The Ecological Approach to Visual Perception* (1979). Here, Gibson outlines the origins of the concept and proposes multiple examples, yet fails to provide a concrete definition; rather, his explanations are often quite vague. For example, in addition to the description included in the introduction at the start of this paper, Gibson also writes:

> *An important fact about the affordances of the environment is that they are in a sense objective, real, and physical, unlike values and meanings, which are often supposed to be subjective, phenomenal, and mental. But actually, an affordance is neither an objective property nor a subjective property; or it is both if you like. An affordance cuts across the dichotomy of subjective-objective and helps us to understand its inadequacy. It is equally a fact of the environment and a fact of behavior. It is both physical and psychical, yet neither. An affordance points both ways, to the environment and to the observer.* (Gibson 1979: 129).

Despite the lack of a single clear, unifying statement, however, Gibson does make certain points that help to reveal his thinking. As summarized by McGrenere and Ho (2000), Gibson specifies three fundamental properties of an affordance: an affordance exists relative to the capabilities of a particular actor; the existence of an affordance is independent of the actor's ability to perceive it; an affordance does not change as the needs and goals of the actor change. While this summary does help to clarify Gibson's position, it still leaves much open to interpretation.

Additionally, Gibson's descriptions of affordances tend to be very broad, including such examples as food affording nutrition and cliffs affording danger, as well as more concrete and familiar examples such as a hammer affording striking. While such a general approach may be desirable in some cases (Stoffregen 2004), it makes it difficult to evaluate the concept empirically. Gibson's descriptions lack predictive power; they say little about how affordances arise from physical properties, or about how an organism might recognize affordances in order to utilize them - key issues in the development of an artificial agent that is guided by affordances.

In the decades since Gibson's death, a debate within the field of ecological psychology has been held over how best to define the concept of affordance. This debate is often complex, with different authors proposing multiple interpretations and definitions, giving rise to several major points of disagreement, such as whether affordances are properties of the environment or aspects of a combined animal-environment system, whether affordances are dispositional properties or relations, and whether af-

fordances relate to complementary "effectivities" of the organism or to its body scale. There is insufficient space here to go into detail, but see, for example, Chemero's (2003) analysis.

Additionally, Şahin et al. (2007) suggest that a further source of confusion has been the fact that affordances can be viewed from three different perspectives: the agent, the environment, or an outside observer, further complicating attempts to agree on a definition.

Unfortunately, a single, uniformly accepted formal definition of "affordance" is still missing. Attempts at a formal definition have been made (e.g. Chemero 2003; Heft 2003; Jones 2003; Michaels 2003; Stoffregen 2003), but these have only added to the debate, while consensus has remained elusive. And often, these attempts at definition suffer from the same problems as Gibson's original descriptions, being very broad and lacking in heuristic guidance (Kirlik 2004).

### 3.2. Are psychological and computational approaches compatible?

Perhaps in part due to the lack of a single accepted definition of affordance, when ecological psychologists and AI researchers talk about affordances, they may often be referring to very different things (Şahin et al. 2007). This disconnect may be the result of the differing goals between the two communities, with psychologists focusing on describing behavior and AI engineers focusing on implementing systems.[28]

There seems to be a general agreement that affordances are "relations," but here, too, psychologists and AI researchers may use the term very differently. In general, researchers in both fields seem comfortable with the notion that affordances are, in some way, relations between physical properties of the agent and the environment. Viewed this way, affordances are external relations, as opposed to internal mental constructs, and the key question is whether or not an affordance physically exists; i.e., does the environment allow the agent to act in a certain way?

In addition to the view of affordances as external relations, AI researchers also have a tendency to refer to affordances as internal mental representations (e.g. Vera & Simon 1993). This is where discussions between the two fields can become contentious. From this viewpoint, the key question is not whether or not an affordance exists in the environment, but the mechanism by which it is perceived by the agent. A physical affordance consists of a property or set of properties that can be sensed. From the common AI perspective, these percepts are associated by the agent with a particular course of action, possibly mediated by the agent's current state (e.g. its set of goals). Thus, AI researchers often refer to affordances as being the relation between the identification of a physical property and the associated response. Ecological psychologists, however, may object to the use of the word "affordance" to describe such internal representations, which were rejected by Gibson (e.g. Chemero & Turvey 2007, responding

[28] In addition, there are other usages of the term "affordance" in the areas of human factors and human computer interaction (Norman 1988, 1999), which differ significantly from the usage in both ecological psychology and AI, reflecting the priorities of practitioners in these fields.

to Şahin et al. 2007). We note that this viewpoint does not necessarily conflict with the view of affordances as physical relations; rather, it is an additional application of the term "affordance," where perhaps another choice of word might be less contentious.

### 3.3. The role of direct perception

The issue of the perception of affordances leads to another, closely related, point of controversy, the role of direct perception. Chemero and Turvey (2007) refer to affordances and direct perception as the two components that define the ecological approach.

In direct perception, affordances are perceived via "invariants" picked up directly from the optic array. Proponents of direct perception argue that there is no need for internal mental representations to mediate the process of perception. Thus, examples from AI that refer to affordances as internal representations (as above), by being incompatible with notions of direct perception, can be contentious.

A frequently cited example of direct perception is the use of optic flow for navigation. Indeed, there is strong evidence to suggest that biological organisms make use of optic flow (e.g. Srinivasan & Zhang, 2004). Additionally, there have been successful applications of optic flow to the design of artificial agents (e.g. Duchon et al. 1998).

There is, however, a significant case made in the literature that direct perception is an oversimplification of the issue. For example, Marr (1982), while praising Gibson's overall approach, argues that there are two main shortcomings to Gibson's focus on the direct perception of invariants. First, that contrary to Gibson's assertions, the detection of physical invariants *is* an information-processing problem, and second, that Gibson significantly underestimated the difficulty of such detection (Marr 1982: 29-30).

Ullman (1980) provides a lengthy critique of the theory underlying direct perception from a cognitive science perspective, arguing that the processes Gibson considers to be direct can instead be further decomposed into simpler perceptual processes, and concluding that direct explanations should be considered a "last resort." Gyr (1972) summarizes a number of empirical studies that cast doubt on direct perception's claims, emphasizing that the state of the agent plays a key role in perception, by determining what part of the optic array is relevant at a given moment and how it will be interpreted. Fodor and Pylyshyn (1981) argue that the properties available in the optic array that could potentially be directly picked up are insufficient on their own to fully explain perception without mediation by memory, inference, or some other psychological processes depending on representations. The conclusion drawn from sources such as these is that the act of perception is highly dependent upon internal mental states, representations, and computations.

This does not mean that we should abandon the goal of simplifying agent design by attempting to *minimize* the need for complex representations, but suggests that attempts to *eliminate* them entirely are unlikely to succeed. Certainly, from a practical perspective, there seems to be no obvious way to implement more complex behaviors (e.g. tool use) that does not involve some sort of representation.

It is also important to note that our goal as AI researchers is often to reproduce *behavior*, which may or may not emphasize detailed modeling of the underlying mechanisms utilized by biological systems. That is, even if biological organisms employ a form of direct perception, it may not be practical or even desirable for artificial agents to duplicate those mechanisms (consider that the underlying "hardware" differs enormously between the neurons in a biological brain and the transistors on a microchip). Ease of implementation, speed of execution, and the final performance of the system must all be considered when deciding what models to apply to the design of an artificial agent. Thus, the fidelity of the model used will depend on several factors, including how well the biological mechanisms are understood, how easily they can be replicated with the available hardware and software, and the specific goals of the research.

Nevertheless, direct perception does remain a key element of the ecological psychology perspective. Thus, the issue of direct perception may be the single most contentious point in discussions between the two fields. For example, Chemero and Turvey (2007) assert in their response to Şahin et al. (2007) that despite debates about the nature of affordances, ecological psychologists all "insist on understanding affordances so that the other main component of Gibsonian ecological psychology [direct perception] is respected" (Chemero & Turvey 2007: 474). Michaels and Carello (1981) also seem to reject any reconciliation between direct and computational/representational approaches. Indeed, at times, the ecological psychology literature can appear almost hostile to any approach that questions the role of direct perception.

## 4. Conclusion

In principle, an ecological approach frees agents from the need to maintain complex representations of the world. The agent can instead interact with the world as it is, allowing for more flexible and timelier responses in a dynamic environment, with the agent able learn to the affordances of its surroundings through first-hand exploration.

A significant body of research now exists in which ecological and affordance-based approaches have been successfully applied to solve problems faced by robotic agents. While psychologists and AI researchers may not always agree on the details of the implementations, they share the goal of better understanding agent-environment systems.

Even so, there remain significant differences that we would like to see addressed. In particular, if the issue of direct perception cannot be resolved, we believe that it may be necessary to abandon attempts to reconcile strictly Gibsonian approaches with much of the current work in AI and robotics, which depends on internal representa-

tions. In such a case, either affordances would have to be defined so narrowly as to only permit behaviors that can be based on very simple mechanisms, such as optic flow, or defined so generally as to provide little practical guidance to researchers. Despite such issues, however, we remain hopeful that the ecological approach will continue to inform the design of artificial agents, and that increased dialog between psychologists and AI engineers may contribute to progress in both fields.

We are encouraged by the appearance of an increased interest in affordance-based robotics in the recent years. Further, many of the agents being developed are moving beyond the issues of basic navigation and obstacle avoidance, with ecological approaches being applied to the design of robots capable of modifying the environment with which they interact. We anticipate that the use of affordance-based design will continue to grow alongside the development of robotic agents capable of increasingly more complex behaviors.

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# Canonical affordances in context*

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**Abstract**

James Gibson's concept of affordances was an attempt to undermine the traditional dualism of the objective and subjective. Gibson himself insisted on the continuity of "affordances in general" and those attached to human artifacts. However, a crucial distinction needs to be drawn between "affordances in general" and the "canonical affordances" that are connected primarily to artifacts. Canonical affordances are conventional and normative. It is only in such cases that it makes sense to talk of *the* affordance of the object. Chairs, for example, are *for* sitting-on, even though we may also use them in many other ways. A good deal of confusion has arisen in the discussion of affordances from (1) the failure to recognize the normative status of canonical affordances and (2) then generalizing from this special case.

**Keywords:** affordances; canonical affordances; dualism; ecological psychology; archaeology.

> *Gibson … gave us affordances as a hopelessly tangled but important attempt to account for meaning in the mutuality of the perceiver and environment.* (Cutting 1986: 252)

Within western thought a profound gulf has long been claimed to exist between the world as it really is, and the world as we perceive it, or, better, *live* in it. For many scientists, this assumption does not itself *seem* to be an assumption at all, but the inescapable implication of two supposed facts:

1. *The poverty of the stimulus*. The structure or information available to our senses is hopelessly limited and entirely insufficient to specify the properties and events of the real world.
2. *The poverty of the real world*. A very large part of what we experience and *believe* to belong to the real world is not real. It is purely subjective, a mental projection upon an inherently colourless and meaningless world.

It is this second assumption that is the really troublesome one, for as John Dewey put it, "subject and object antithetically defined can have logically no transactions with each other" (Dewey 1958 [1925]: 239).

Curiously, this assumption about the profound subjectivity of human experience is often actually relished within empirical science itself, even though it undermines the very conditions of possibility of *empirical* science. Here is the perceptual psychologist, Richard Gregory, claiming, within the space of a few lines, that we human beings are both capable of conducting objective scientific research, and, on the basis of the results of such research, that all of us – presumably scientists included – are nevertheless imprisoned within our own subjectivity:

> *... it used to be thought that perceptions, by vision and touch and so on, can give direct knowledge of objective reality. ... But, largely through the physiological study of the senses over the last two hundred years, this has become ever more difficult to defend. ...ultimately we cannot know directly what is illusion, any more than truth - for we cannot step outside perception to compare experience with objective reality.* (Gregory 1989: 94, emphasis added)

This opposition of the subjective and the objective simply does not make sense. An ideal of *mindless* objectivity – of science somehow being *done* and yet also untouched by human subjectivity – can be remarkably persuasive as long as we view science in the rear-view mirror, as a body of established facts and theories. However, as soon as we regard science as the ongoing open-ended enterprise that it actually is, it becomes clear that we urgently need an alternative conception of the objective – of the *real* – that can find a place for *us*. After all, it is only *subjects* who can be objective.

When I first encountered James Gibson's writings, as a student, I was struck straight away, given my background in physics, by the serious challenge he was posing to the dualism of the physical and the mental – of the objective and subjective.

For a long time, Gibson's work seemed to be directed exclusively against the assumption of the poverty of the *stimulus*." He argued that if we regard stimulation" as relational and transforming, rather than punctate and static, then we can begin to appreciate that it is already richly endowed. Later, he rejected the very idea of the stimulus. Information, he insisted, is obtained, not imposed.

Gibson's attack on the "poverty of *reality*" is now widely identified with his last book, *The ecological approach to perception* (1979), in connection with his concept of affordances. In fact, he had also used the term "affordances" in his 1966 book, *The senses considered as perceptual systems,* and had even anticipated the concept in several important ways in a remarkable, though largely forgotten, chapter on meaning, in his first book, *The perception of the visual world* (1950):

> *... there are all the simple use-meanings or meanings for the satisfaction of needs such as are embodied in food-objects, tool-objects, dangerous objects, and what Freud called love-objects, the parents being the first instances of the latter. For example, food looks eatable, shoes look wearable, and fire looks hot.* (Gibson 1950: 199).

Taking the example of our distant human ancestors, Gibson argued that such "use-meanings" are, from a biological perspective, more fundamental than the detailed properties of objects (and also, of course, symbolic-meanings):

> *The color, shape, motion, and distance of things [were] of no interest ... in themselves. These abstractions were merely the identifying features, often slight and subtle, of objects which invited or compelled action.* (Gibson 1950: 198).

Here is perhaps Gibson's earliest use of the actual term, "affordances":

> *I have coined this word [affordances] as a substitute for values, a term which carries an old burden of philosophical meaning. I mean simply what things furnish, for good or ill. What they afford the observer, after all, depends on their properties.* (Gibson 1966: 285)

Gibson's concept of affordances continues to give rise to a good deal of controversy and confusion. It has not helped that Gibson's presentation of the concept was itself sketchy and confused. My purpose in the rest of this article is to identify some of the main sources of this confusion – not least, Gibson's failure to distinguish properly between what I call "canonical affordances" and "affordances in general."[**]

## Affordances as relational

Gibson took care to stress that affordances are *not* animal *independent*:

> *An affordance is not what we call a "subjective" quality of a thing. But neither is it what we call an "objective" property of a thing if by that we mean that a physical object has no reference to any animal. An affordance cuts across the dichotomy of subjective-objective and helps us to understand its inadequacy. The affordances of the environment are facts of the environment, not appearances. But they are not, on the other hand, facts at the level of physics concerned only with matter and energy with animals left out.* (Gibson 1977: 69-70, emphasis added)

Initially there was a lot of resistance to acknowledging the relational status of affordances (and indeed information) because it threatens Gibson's claim to be providing us with "reasons for realism," and an escape from relativism. Even Gibson wavered on this crucial point (see Costall and Still 1989, Costall 1995). Yet it is now widely agreed that affordances are indeed relational, and, furthermore, that it is precisely

because they are relational that the concept holds out the great promise to undermine the traditional dualism of the subjective and objective. Affordances are not, however, as many commentators have claimed, relative to the animal as a mere "observer" (Gibson 1966: 285), but, rather, to the animal as *agent*:

> *... a combination of physical properties of the environment that is uniquely suited to a given animal - to his nutritive system or his action system or his locomotor system* (Gibson 1977: 79).
>
> *[The physical properties that constitute affordances] have unity relative to the posture and behavior of the animal being considered. So an affordance cannot be measured as we measure in physics."* (Gibson 1979: 127-8, emphasis added)

In my view, the concept of affordances marks a fundamental shift in Gibson's "ecological approach" from a theory of perception towards a more encompassing ecology of agency (Costall 2003). Furthermore, by undermining the traditional dualism of the subjective and objective, it takes us beyond the limited alternatives of either idealism or realism.

**Affordances as "directly perceived"**

Gibson's claim that affordances can be "directly perceived" has been the most controversial, and rightly so:

> *The theory of affordances is a radical departure from existing theories of value and meaning. It begins with a new definition of what value and meaning are. The perceiving of an affordance is not a process of perceiving a value-free physical object to which meaning is somehow added in a way that no one has been able to agree upon; it is a process of perceiving a value-rich ecological object. Any substance, any surface, any layout has some affordance for benefit or injury to someone. Physics may be value-free, but ecology is not.*

So far, so good. But Gibson then immediately went on as follows:

> *The central question for the theory of affordances is not whether they exist and are real but whether information is available in ambient light for perceiving them.* (Gibson 1979: 140)

There are three big problems here. The first is that Gibson once again frames the issue of affordances in terms of perceiving rather than acting, yet it is often only in acting upon things that we discover or reveal or even create what it is they really afford. The second problem is that over the years Gibson had come to define his concept of "direct perception" (and also his earlier term, "literal perception") by contrast to a wide diversity of examples of what he deemed to be *indirect* perception including picture perception, social stereotyping, and even looking through microscopes and telescopes (see

Costall 1988). Many of these examples of indirect perception involve *social mediation* of one kind or another. Critics of Gibson's claim that affordances can be directly perceived rightly pointed out that the affordances of human artifacts, such as their favourite example of the mail box, could not be determined by a stranger to such things by merely peering at them in sublime isolation from other people.

The third and biggest problem is that by foregrounding the issue of perception Gibson put the epistemological cart before the ontological horse: "what value and meaning *are*" (Gibson 1979: 140, emphasis in the original).The central question for the theory of affordances is precisely "whether they exist and are real." It is this claim that marks Gibson's truly radical break with the long tradition of Western thought that has held that meanings and values are purely subjective and hence *unreal*.

**Affordances are not stimuli**

As Gibson himself emphasized, affordances are not efficient causes. They do not *make* us do things:

> *The fact that a stone is a missile does not imply that it cannot be other things as well. It can be a paperweight, a bookend, a hammer, or a pendulum bob. ... The differences between them are not clear-cut, and the arbitrary names by which they are called do not count for perception. ... You do not have to classify and label things in order to perceive what they afford.* (Gibson 1979: 134, emphasis added)

Some critics have argued that if it is the case that any particular thing has a great diversity of *different* affordances, then the concept of affordances is vacuous. James Cutting gives the example of the limitless affordances of paper:

> *To be sure, it does not afford flying to Baghdad upon, but the exclusion of a large domain of behaviors does not diminish the fact that an infinity remain.* (Cutting 1982: 216)

But this is the crucial bottom line: although we can do many things with any single thing, we cannot do *anything* with any thing. We can use an apple as a missile or for archery target practice, for example, but if there is no food around we simply starve. And, for sure, we cannot fly to Baghdad on a sheet of paper (or, for that matter, on a carpet).

**Canonical affordances**

A related criticism of Gibson's treatment of affordances has come from exactly the opposite direction. John Shotter has argued that Gibson presents us with an entirely *static* account of affordances, of meanings *inhering* in objects just waiting to be "discovered." Yet, of course, in many of our spontaneous interactions with things their meanings come into being within the flow of activity:

> *... the beings in Gibson's world are depicted merely as observers, not as actors, i.e. not as beings able to provide for themselves, by their own actions, conditions appropriate to support their action's continuation. They may move about, but they do not act; thus rather than "makers", they are presented merely as "finders" of what already exists. Such a view, I would argue, fails to recognize the peculiar form-producing character of activity in a biological and social world; it fails to assign a proper role to time and to processes of growth and development.* (Shotter 1983: 20)

According to Shotter, therefore, *everything* is in flux: "an affordance is only completely specified as the affordance it is when the activity it affords is complete" (Shotter 1983: 27). In short, we can never "step" into the same flow of affordances twice.

Bill Noble has also criticized Gibson for being blatantly inconsistent in his account of affordances, at times acknowledging their fundamentally "open," relational status, and, at other times, "lodging" affordances in an "objectivated world" (Noble 1991:. 204). In my view, both Shotter and Noble were right to point out that Gibson was confused on this issue, but I think they too were confused, and for the same reason. They failed to make a crucial distinction between "affordances in general" and what I call "canonical affordances" – the conventional, normative meanings of things, notably in relation to human artifacts.[***] For example, a chair is for sitting upon, even if no one happens to be sitting upon it, or else is standing on it in order to change a light bulb. In such cases, the affordance has indeed become "objectivated" or, better, "impersonal" (Morss 1988): *one* sits on chairs. The meaning of artifacts cannot, therefore, be understood in terms of the individual-object dyad, but, rather, within a wider social framework.

Gibson was keen to insist upon a continuity between "affordances in general" and those of cultural artifacts in particular:

> *It is a mistake to separate the natural from the artificial as if there were two environments; artifacts have to be manufactured from natural substances. It is also a mistake to separate the cultural environment from the natural environment, as if there were a world of mental products distinct from the world of material products. There is only one world, however diverse, and all animals live in it, although we human animals have altered it to suit ourselves.* (Gibson 1979: 130)

Gibson's treatment of the affordances of artifacts in much the same terms as "natural" objects has led to two important problems. The first is the one identified by Shotter and Noble: the objectification of affordances *in general*. The second problem is the failure of Gibson (and, indeed, Shotter and Noble) to recognize that the meanings of things can indeed become *objectified* and *normative*. Artifacts embody human intentions. Indeed, it is through the tacit, embodied understanding of the "canonical affordances" of things, as much as through explicit representations, that young children enter our cultural world (Williams, Costall and Reddy1999). To a remarkable extent, psychological theory has treated the conventional use-meanings of things as totally transparent and given. And this includes developmental theory:

> *In the study of early infancy, it is very common for objects to be treated as natural signs [rather than conventional signs] … The object is rarely placed within a network of interpersonal relationships where its uses affect the ways subjects communicate with each other.* (Rodríguez 2007: 261)

In short, a major source of confusion concerning the concept of affordances has been (1) Gibson's failure to distinguish "canonical affordances" from "affordances in general," and (2) his unwitting generalizations *from* the very special case of "canonical affordances" to "affordances in general."

**Canonical affordancesas part of a wider "constellation"**

The real limitation of Gibson's treatment of affordances is that although it is relational, the relation is restricted to an agent-object dyad. The concept of "canonical affordances" itself alerts us to those important cases where the affordances of some thing are not simply shared between people but also normatively predefined. Yet the affordance of any artifact is not confined to that object in isolation, but depends on a "constellation" (Keller and Keller 1996) or "utensil-totality" (Gurwitsch 1979: 82-83) of not only other objects but also events. The affordances of artifacts are not usually self-contained but depend upon a wider context of other artifacts (as in the case of a toolkit) but also upon the encompassing practices in which they *go* together.

A group of archaeology students at Copenhagen University engaged in an excavation of the camp area attached to the annual rock music festival at Roskilde. They found plenty of used condoms and beer cans, a few food wrappers, and a single hash-pipe (needless to say, these items have pretty definite canonical affordances). Although they remained mute on the subject of condoms, the students suggest that future archaeologists excavating the same site in a thousand years time might well conclude that the people they were studying drank much more than they ever ate. They themselves were mainly impressed, however, by what their excavation *failed* to reveal – the event that was *holding* these various artifacts together. "We cannot see the music in the festival's soil" *("man ikkekan se musikkenifestivalensmuld")* (Skyum-Nielsen 2007: 25).

## Conclusion

I have been arguing that we need to recognize the special status of "canonical affordances," the established, widely agreed use-meanings of things. In the case of "canonical affordances," the task of the uninitiated is not typically to find their *own* meanings in the object, but to find out the intended function of the object. "Canonical affordances" have an apparent objectivity or impersonality that contrasts with the fluid and open-ended interactions with objects highlighted, for example, by John Shotter. A theoretical understanding of "canonical affordances" will not be achieved by fixation upon the object in isolation, nor the individual-object dyad. The object needs to be understood within a network of relations not only among different people, but also a "constellation" of other objects drawn into a shared practice.

In fact, this conclusion echoes the "manifesto" of a book I recently edited with Ole Dreier, based on workshops we held at the University of Copenhagen. Even "objectivated" affordances are not as static and self-contained as they might seem:

> *Things are best understood ... not as fixed and independent of people, but as themselves transformed, even coming into being, within ongoing practices, and which these objects, in turn, transform. We ... learn more about both people and things by studying them as worldly, not just as in the world, but as incorporated into practices in the world.* (Costall and Dreier 2006: 11)

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** Donald Norman's very attractive and informative book *The psychology of everyday things* (1989) was the first to bring Gibson's concept of affordances to wider attention. Unfortunately, this book was also yet another source of confusion (see Torenvliet 2003).

*** In an early article, Loveland (1991) used the term "preferred affordances" but this does not really capture the institutionalized, normative status of such affordances.

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# The concept of a structural affordance

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**Abstract**

I provide an analysis of the concept of an "affordance" that enables one to conceive of "structural affordance" as a kind of affordance relation that might hold between an agent and its body. I then review research in the science of humanoid bodily movement to indicate the empirical reality of structural affordance.

**Keywords:** Affordances, Ecological Psychology, Embodiment, Embodied Cognition, Motor Control.

One of Gibson's novel contributions to perceptual psychology is the notion of an affordance. In this article, I provide an analysis of the concept that allows one to think about a kind of affordance relation that might hold between an agent and its body, which I call "structural affordance". After a brief specification of the latter, I review research in the science of humanoid bodily movement to provide some indication of the empirical reality of structural affordances.

## Gibson's affordances

Here is the first example of an affordance that Gibson provides:

> *If a terrestrial surface is nearly horizontal (instead of slanted), nearly flat (instead of convex or concave), and sufficiently extended (relative to the size of the animal) and if its substance is rigid (relative to the weight of the animal), then the surface affords support*. (Gibson 1979/1986: 127, emphasis original)

The status of affordances is slightly ambiguous. At first blush, when Gibson says that the surface affords support, it would suggest that he wishes to pick out certain properties of the environment. Indeed, when he introduces the term itself he says that the "affordances *of the environment* are what it offers the animal" (ibid.: 127, emphasis mine). But then he says this: "The verb to afford is found in the dictionary, but the noun affordance is not. I have made it up. I mean by it something that refers to *both the animal and the environmen*t in a way that no existing term does" (ibid., emphasis mine). How could a term refer to both animal and environment? He then offers a third

option (which I think is the most sensible), saying what he wants to refer to is something that implicates their relationship, for he insists that the term "implies the *complementarity* of the animal and the environment" (ibid., emphasis mine). So we have a few options for interpreting the term. Indeed, there are two extremes. At one end, the interpretation is that "affordance" denotes a property of the environment which bears a special and particular relationship to an animal.[29] At the other end, "affordance" denotes the (special, particular) relation itself.[30] Although this is a significant matter of theoretical choice, I opt for the latter interpretation; but not entirely without motivation. Treating affordances as relations is the simplest way of capturing the reality of affordances: they are as real as their *relata*.[31]

Interpretive issues aside, there are several things about affordances that are clear. The relations are certainly conditional. They only occur within a certain range of possibility, denoted here by the explicit contrasts used (nearly horizontal vs. slanted etc.). In this case, and presumably others, multiple conditions must simultaneously obtain in a particular situation for the instantiation of a particular affordance. If a terrestrial surface were nearly horizontal over one period of time, but only sufficiently extended at another, and rigid at neither time, then there would be no time during which the affordance held at that place. And the same point holds, of course, for conditions holding in different places at the same time. It is also clear that the conditions are not necessarily binary phenomena, but are potentially graded; a surface that is nearly flat may do just as well as a surface that is completely flat. This gradation may further dictate the extent to which an affordance holds (and even in part its evaluation as positive or negative). A slippery surface, for instance, might be some grade between an affordance of support and falling; and this might be an instability to be exploited by the right morphology (cf. Pfeifer and Bongard 2007: 99).

[29] In what follows, I use the notion of a property in the general sense that objects (e.g., pears, chairs) are characterised by their properties (e.g., sweetness, rigidity). I treat them as distinct from relations for the sake of clarity; relational properties are distinct from the relations they instantiate. Most of what I say could be altered to treat relations as properties predicated of more than one individual, but with needless complication. Conversely, one might do away with properties altogether and define the notion of a structural affordance (discussed below) solely in terms of relations (see footnote 5). I will not explore the consequences of this here.

[30] There is of course the possibility that affordances could be both. Norman (1988) aims to provide a distinction between perceived and real affordances. In effect, this is a distinction between the perceived properties of a thing (*viz.* properties that bear particular relations to particular animals) and the actual relation that an animal bears to that thing. It should be noted that much theoretical literature on affordances assumes that affordances are environmental properties that bear particular relations to animals, and occasionally this assumption is made without argument (see Chemero 2009: 135 - 47 for a review). However, others have argued for the view that affordances are properties of an animal-environment system (Stoffregen 2003), or relations between the abilities of animals and features of their environments (Chemero 2003). As the reader can surmise, my preference is for the latter.

[31] I sideline the further possibility that affordances might be dispositional states (or indeed, properties) of the agent's nervous system (Ellis and Tucker 2000). I do so firstly because it is so far from Gibson's intentions; secondly, because I suspect that the truth of the proposal depends upon the explanatory scope of the notion of representation; and thirdly, because even if it were the case that affordances (in general, not merely Ellis and Tucker's micro-affordances) were representational, the question of what they represent (properties of the environment or relations between animal and environment) would still be at issue, i.e., the central issue would remain unsettled.

Identifying affordances requires identifying relational properties of the environment that are inter-dependent with relational properties of the animal. And they must be identified as such; for only this would give us a handle on the complementary relationship that is putatively at hand in the instantiation of an affordance.[32] But it certainly does not follow from the fact that a *theory* of affordances must identify certain relational properties, that they must be known to the perceiver under that description. In fact this is just what Gibson wished to avoid. If there is anything that is clear about what it means to perceive an affordance, it is that it is precisely not supposed to convey the idea that seeing a surface as affording support involves entertaining (or endorsing) a thought such as:

- The surface I see that is nearly horizontal (instead of slanted), nearly flat (instead of convex or concave), and sufficiently extended (relative to the size of my body) and its substance is rigid (relative to my body) is in such-and-such a spatiotemporal relation to me that the relation affords support.

If anything is meant to follow from Gibson's claim that "affordances *seem* to be perceived directly because they *are* perceived directly", it is that the perceiver need not be appraised of affordance relations under any description of them as such in order to perceive them. (1979/1986: 140)

Nevertheless, in order to *study* affordances one needs to identify the relational properties of the animal that are supposed to stand in an affordance relation with the environment. In the quoted case, size and weight (at least) seem to be important properties. These may be determined in a number of ways, for instance, relative to conventions of measurement designed to approximate objective physical units, or relative to a particular contextual relation with no such design. The crucial difference here is that the contextual relation is symmetric, whereas the conventional relation is asymmetric. The girl being six feet tall depends upon conventional measurements of feet and inches, but conventional measurements of feet and inches do not depend upon the height of any particular individual. There have been some elegant studies designed to demonstrate the importance of contextual (rather than conventional) relations in the instantiation of an affordance. For instance, Warren and Whang (1987) sought to investigate the extent to which an aperture affords walking from one side of a partition to the other by measuring the frequency of shoulder turning in a group of smaller-than-average participants and a group of larger-than-average participants. Unsurprisingly, a decreased frequency of shoulder turning was found to be positively correlated with an increase in aperture width. But when aperture width was determined relative to conventional physical units, the psychophysical functions of the two groups were fairly dissimilar. It was only when aperture width was determined relative to the distance between the participant's shoulders that the psychophysical functions of the two groups became comparable (indeed, strikingly similar).

[32] By this I mean that in order for a theorist to recognise that an affordance is instantiated, it needs to be identified in some way. This somewhat mundane point is certainly distinct from the claim that affordances need to be identified (and recognised) in order to be instantiated.

**Structural affordances**

I now want to argue that there is a particular kind of affordance relation pertaining to the structure of the body, one that can be distinguished from all others. The initial impetus comes from Jose Luis Bermúdez's discussion of a candidate solution to the problem of how to segment the body into parts:

> *Let me now introduce the technical concept of a hinge. The intuitive idea that I want to capture with this term is the idea of a body part that allows one to move a further body part. Examples of hinges are the neck, the jaw socket, the shoulders, the elbows, the wrists, the knuckles, the finger joints, the leg sockets, the knees, and the ankles. The distinction between moveable and immoveable body parts, together with the concept of a hinge, creates the following picture of how the human body is segmented. A relatively immoveable torso is linked by hinges to five moveable limbs (the head, two legs, and two arms), each of which is further segmented by means of further hinges.* (1998: 155)

Bermúdez suggests that his talk of hinges "provides a nonarbitrary way of segmenting the body that accords pretty closely with how we classify body parts in everyday thought and speech" (1998: 156). This is not quite right. It may be non-arbitrary, but it does not exactly capture reference to noses and ears, for example. Still, I want to extract something positive from the idea by noting another point he makes, which is that "awareness of the location of the hinges, as well as of the possibilities for movement that they afford, can plausibly be viewed as an inevitable concomitant of learning to act with one's body" (ibid.: 156). A look around his book would suggest that Bermúdez is using the notion of an affordance here in the Gibsonian sense (cf. Bermúdez 1998: 103 - 29). According to the account of affordances adopted above, affordances are relations. Here the relationship is between a bodily agent and its body. Call this kind of an affordance a ***structural affordance (SA)***, to repeat:

**SA** A structural affordance is a relation between a bodily agent and its body.

This in turn enriches the idea of an (ordinary) affordance relation between an agent and environment. On this analysis, ***agent-environmental affordances*** (leapability, malleability *etc.*) are second-order relations, prior to which are an agent's first-order SAs. Call agent-environmental affordances AEs for the sake of the distinction. One can define the relationship between SAs and AEs as follows; reading $Q$ and $R$ as specific relations, $x$ as an unspecified relation, and $b$ as an individual:

(1). If there are affordances, then there are SAs.

(2). There are affordances that are not SAs.

(3). If $x$ is an affordance in which $b$ is a *relatum*, then either $x$ is an SA relation $R$, or else $x$ is an AE relation $Q$, and $b$ is a *relatum* in $Q$ in virtue of being a relatum in $R$.

The definition of the relationship between SAs and environmental affordances is similar to a seminal definition of the notion of a ***basic action*** (cf. Danto 1965: 142). Here I will understand basic action to mean any instance of an agent trying to maintain or

change its bodily comportment. This is nevertheless meant to be along the lines of the typical understanding. A basic action can be in the service of some further (instrumental) action that an agent is trying to perform, but it need not be. Furthermore, *trying* to act requires the following condition: only when actions are initiated (and usually performed) under the impression that the actual performance is *possible* might we say that the agent *tries* or is *trying* to perform that action. This is one side of the philosophical coin paid in a trying-analysis of action, the other side of which is the metaphysical possibility of the action's total failure. O'Shaughnessy provides a description of this latter:

> *There is a perfectly genuine sense, suppressed by philosophers of a commonsensical orientation, in which no event, including intended act-events, can be foretold as an absolute certainty. That sense is this: the world is known to have harbored freak happenings; this is a permanent potential of the world, and of no situation can it be said: "This situation bears a charmed life, it is guaranteed not to harbor such a freak".* (1973: 365 - 66)

Here besides the suggestion of the metaphysical possibility of a radically indeterminate world there is the significant insight that when we try to perform some basic action such trying is always engaged in spite of the possibility that reality may not cooperate. Nevertheless, "trying entails the presumption on the agent's part that success is at least a remote possibility" (O'Shaughnessy 1973: 367). If this is correct then there ought to be some way of specifying the constraints on the action in question. One way of doing so would be to appeal to the fact that the body itself has structure. A positive feature of Bermudez's hinge analysis is that it forefronts the fact that parts of the body have the relational property of being interlocked with one another. Furthermore, by employing "the idea of a body part that allows one to move a further body part" in the analysis, Bermudez likely intends to indicate the importance of this property in this regard. The assumption being that it is in virtue of the fact that its body parts are interlocked with one another that a bodily agent is able to act. But this cannot be sufficient for the possibility of basic action. It is not so much the interlocking of parts that is important, but rather (as a consequence) the manner in which the parts causally interact, in virtue of their properties. Moreover, in absence of reasons to think otherwise, one ought to allow for the possibility of part-whole causal interaction, in addition to the possibility of part-part interaction forefronted by Bermudez's hinge analysis.

Here then is a fuller definition, unpacking the notion of a structural affordance further:

**SA*** A structural affordance is a relation between a bodily agent $b$ and the properties $I^P$ and $I^W$ of parts of its body, in virtue of which a basic action $\phi$ is possible for $b$.

- $I^P$ is the property of being causally interactive with other parts $x_1, x_2, x_3, \ldots, x_n$ in virtue of their properties $P_1, P_2, P_3, \ldots P_n$.
- $I^W$ is the property of being causally interactive with the whole body in virtue of its properties $W_1, W_2, W_3, \ldots W_n$.

This is closer to being satisfactory.[33] Now the main work is in showing that reality of structural affordances is plausible. I will do this by means of a potted review of research in motor coordination. The aim is to make some piecemeal progress by picking out at least a few candidate *P*s and *W*s (marking them as *(P)* and *(W)* as I go along). In doing so, I hope to highlight some of the causal processes on which SAs are dependent. I will also minimally address how something might count as a bodily agent, and thus how something can count as a basic action of a bodily agent (when I do that it will be obvious I am doing so). My treatment of these will be somewhat cursory, but sufficient to indicate the direction I think a treatment of SAs should take.[34]

## The empirical reality of structural affordances

A significant property of multi-jointed agents is ***motor redundancy*** (see Figure 1). Latash summarises a favourite example, which he attributes to the father of the concept, Nikolai Bernstein:

> *Touch your nose with your right index finger. Now try to move the arm without losing the contact between the fingertip and the nose. This is easy to do. This means that one can touch the nose with very many combinations of arm joint angles. Nevertheless, when the task was presented, you did it with a particular joint combination.* (Latash 2008: 35)[35]

His point is that your arm has the *(P)* property of being highly redundant in a certain sense. A glance at Figure 1 can help clarify. Assume an idealisation of your arm as having only four joints (shoulder, elbow, wrist and first knuckle), where each joint has only one degree of freedom. In order to reach endpoint E, you could assume angles a, b, c and d. But you could also reach E by assuming a', b', c' and d'. Indeed, you could also reach E by assuming a'', b'', c'', d'' *etc*.

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[33] An alternative (suggested to me by Sascha Fink) would be to describe structural affordances literally as structures with sets of relations defined upon them. This would be something like the following:

**SA**** A structural affordance is a relation between a bodily agent *b* and the relations *I* and *J* governing the set of *b's* body parts, in virtue of which a basic action $\phi$ is possible for *b*

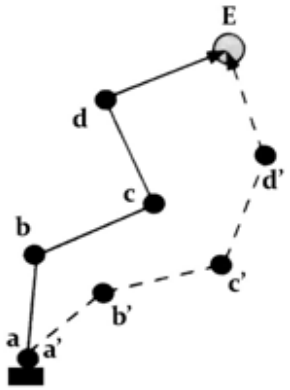
- *I* is the relation of part *x* causally interacting with other parts $x_1, x_2, x_3, \ldots, x_n$
- *J* is the relation of part *x* causally interacting with the whole body

An interesting line of future research would be to explore the implications of this and other alternative ways of defining structural affordances.

[34] There is in fact a large literature on the study of agent-environmental affordances that I leave aside for the sake of brevity (See, e.g., Ellis and Tucker 2000; McBride, Sumner, and Husain 2011). Suffice to say, if the concept of a structural affordance picks out something real, it ought to figure in a full explanation of the phenomena revealed in these studies.

[35] Latash makes something of an understatement here. He is well aware that there are not just "very many combinations of arm joint angles" that will bring the finger to the nose. As he notes later, potentially the number of combinations is infinite (Latash 2008: 36).

**Figure 1– Redundancy in a four joint limb**




See main text.
Adapted from Latash (2008: 36).

Redundancy thus described, gives rise to a very simplified instance of what is often known as ***Bernstein's problem***. As Michael Turvey notes, when considered as a *(W)* property of the whole body, motor redundancy is all the more complex:

> *As characteristic expressions of biological systems, coordinations necessarily involve bringing into proper relation multiple and different component parts (e.g. $10^{14}$ cellular units in $10^{3}$ varieties), defined over multiple scales of space and time. The challenge of properly relating many different components is readily illustrated [...] There are about 792 muscles in the human body that combine to bring about energetic changes at the skeletal joints. Suppose we conceptualize the human body as an aggregate of just hinge joints like the elbow. It would then comprise about 100 mechanical degrees of freedom each characterizable by two states, position and velocity, to yield a state space of, minimally, 200 dimensions.* (1990: 938)

Suffice to say that a 200-dimensional space of possibilities (which itself is a simplification) is of mind-boggling complexity. Biological movements regularly trace a path through the high-dimensional spaces that Bernstein's problem reveals. But, interestingly, they regularly exploit the variety of options; they trace *different* paths to reach the same goal. And this is the case even in stereotyped movements performed by highly trained individuals. As Latash conveys, Bernstein discovered this himself in a careful study of labour workers (Latash 2008: 31). He attached small light-bulbs at key points on the bodies of blacksmiths, as well as on their familiar hammer. Then he photographed their movements using an innovative high-speed shutter, whilst they performed a typical hammer-strike. What the photos revealed was that across strikes there was much less variability in the movement of the tool than in the various individual joints moving it. This suggests a simple and smart solution to Bernstein's problem: treat the system as if it had fewer degrees of freedom by lumping components together. J. A. Scott Kelso illustrates the idea nicely:

> *During a movement, the internal degrees of freedom are not controlled directly but are constrained to relate among themselves in a relatively fixed and autonomous fashion. Imagine driving a car or a truck that had a separate steering mechanism for each wheel instead of a single steering mechanism for all the wheels. Tough, to say the least! Joining the components into a collective unit, however, allows the collective to be controlled as if it had fewer degrees of freedom than make up its parts, thus greatly simplifying control.* (1995: 38)

This solution motivates a fairly autonomous conception of components of a motor task, where "elements of a system are not controlled individually, like segments of a marionette's body by attached strings, but united into task-specific [...] *structural units*" (Latash 2008: 53, emphasis original). According to this gloss, ***task-specific structural unity*** is a *(P)* property of the blacksmith's arms. Parts of the blacksmith's arm work in cooperation, stabilising and compensating for one another's variable behaviour to consistently reach the target.

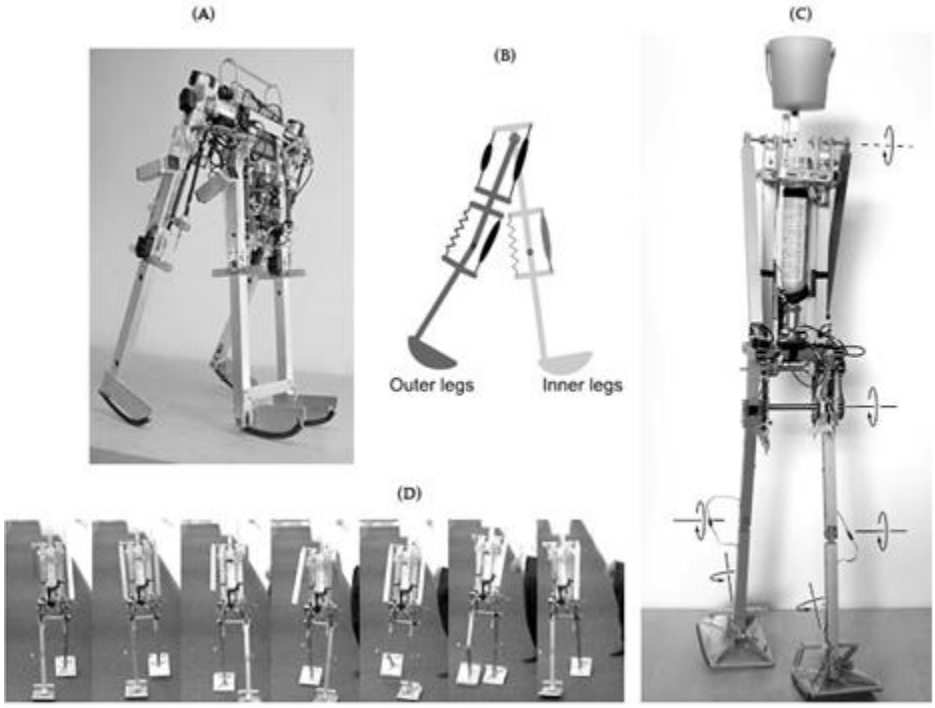
Task-specific structural unity can also be seen as a *(W)* property of the whole body in an activity like locomotion. To give an intuitive illustration, consider how one might approach the design of systems capable of human-like walking. Humans, like all three-dimensional objects, have a minimum of three degrees of freedom: a body can pitch forward and back, roll side-to-side, and turn (yaw) left or right. If a locomotive body is treated as a structural unit then it might be possible to keep the number of its degrees of freedom fairly close to three. Pitching and rolling are less technically known as leaning. Too much leaning can lead to falling over, so leaning whilst walking ought to somehow be stabilised. The problem can be conceived by imagining the walker as an inverted pendulum on either the sagittal or frontal plane. The aim is to keep the ball of the pendulum roughly perpendicular to a flat walking surface (idealized as having no incline or decline). What is required is to achieve a sustained natural pendular motion by instantiating the *(W)* property of stably coupled components. One way of doing this would be to make sure that pressure exerted upon the walking surface is kept within a safe area inside the foot edges, by making sure that the foot of the stance leg remains flat on the floor whilst the swing leg is brought forward (Wisse 2005: 113- 14).[36] But this is harder than it sounds. All potentially destabilising elements would require continuous monitoring and control, which adds considerably to the basic three-dimensions.

Enter the ***wild-walker***. The Delft Biorobotics lab have developed a simple construction that they call ***Mike***, to demonstrate that the task specified above can be more simply executed by coupling components to one another such that they collectively produce a self-stabilising behaviour. Mike has two symmetrical pairs of legs, each leg in a pair is fused to the other, one pair moves outside the other, and each pair has a knee joint (see Figure 2A). To facilitate movement on a flat (rather than a declining sloped) surface, Mike has oscillatory pneumatic actuators, called ***McKibben muscles***, either side

[36] As the expression flat-footed connotes, this is not in fact how most humans ought to place their feet. As Wisse and van Frankenhuyzen (2006: 144) point out, the shape of typical human feet actually channels the centre of pressure forwards in the progression of the step cycle. This fact has been reflected and exploited in the design of prosthetic feet, and wild-walkers since McGeer's early prototypes.

of his outer hip joints. There is also a muscle at every knee joint extending the leg, counteracted by a spring on the other side (see Figure 2B). Muscle activity is regulated by manually tuned timing and a simple switching mechanism in the inner and outer feet that antagonistically couples the hip muscles. So, for example, when the inner foot mechanism switches, the outer knee muscles are deactivated, the muscles at front of the outer hip are activated. The knee muscle is reactivated just under half a second later, and as the outer foot strikes, the mechanism switches the power from the front hip muscles to those at the back. As a result, when set to walking, Mike exhibits a robust gait, with a fairly fast leg motion of even length (see Wisse 2005: 116 - 22; Wisse and Frankenhuyzen 2006). In fact this is the key, as Mike's intrinsic dynamics essentially implement the following two principles: "You will never fall forward if you put your swing leg fast enough in front of your stance leg. In order to prevent falling backward the next step, the swing leg shouldn't be too far in front" (Wisse 2005: 122, italics removed).

**Figure 2– Humanoid walker prototypes at the Delft Technical University Biorobotics lab**




(A) Mike, a quadruped two-dimensional walker with actuated hips and knees.
(B) "McKibben muscles" situated on Mike's hips and knees (see text).
(C) Denise, a biped three-dimensional walker with unactuated knees, a passive upper body, counterswinging arms and an ankle joint modelled on a skate-board truck.
(D) Video-stills of Denise in motion (video available at *http://dbl.tudelft.nl*).

All pictures from Wisse (2005), reprinted with permission from the author.

Mike's structure presents a solution to a tendency for unstable pitching that keeps degrees of freedom low. In fact, he is built to have only three degrees of freedom, one at his hip, and one for each pair of knees. As a result, Mike only moves in the sagittal and horizontal plane. From the side, he looks like someone marching. But from the front he looks like someone shuffling on crutches, for the lateral stability inherent in his inner-outer leg design foregoes the problem of ***lateral leaning***. A slightly more humanoid solution to this would be lateral foot-placement; but a potential drawback is that reducing lateral leaning in this way could lead to an increase in pitching instability. Martijn Wisse suggests a compromise:

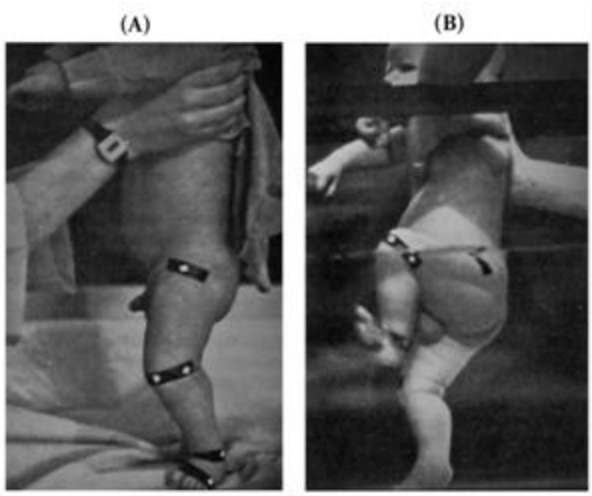
> *Similar to skateboards and bicycles, one could use steering (yaw) to stabilize lean, at least as long as the system is moving forward with sufficient velocity. The same principle is applicable to walking, and can be implemented in walking robots with an ankle joint that kinematically couples lean to yaw.* (2005: 125)

Working on these principles the Delft team built ***Denise***, a three-dimensional walker. Denise only has two feet, she also has a passive upper body, two counter-swinging arms rigidly coupled to hip angle, and a (skateboard-like) lateral leaning ankle joint, resulting in five degrees of freedom (see Figure 2C). Her ankles and knees are entirely passive, and her hip joint reciprocally couples her two legs to one another. Her hip motion is controlled in a similar manner to Mike's, but with less actuation. At each step, a switch in the striking foot activates the contralateral hip muscle, and releases a latch at the knee joint, allowing the leg to bend and then swing back to extension. Because of the ankle joint, each of Denise's steps is slightly laterally displaced. But each step drives her forward to another step: when the right foot strikes, the left knee is released, and the hip muscles start pulling the left leg forward, Denise's weight shifts forwards, the leg extends and the foot lands, repeating the process for the contralateral leg. Consequently, she swaggers along stably at around 2.5 km/h (see Figure 2D).

To the extent that Mike and Denise provide models of actual walking, they provide nice examples of whole body structural unities that could be constituent properties of an SA. But the presence or absence of an affordance for basic action can be rather labile. An interesting case of this is found in the development of infant locomotion. Most newborn infants exhibit alternate stepping movements if their upper body is supported, though these seem to "disappear" after a couple of months (Zelazo, Zelazo, and Kolb 1972). Beyond this, Esther Thelen and colleagues made several further observations. First is that these movements are "not random thrashings of the legs, but rather organized movements with a recognisable structure in time and space" (Thelen and Smith 1994: 11). Second is that these babies actually exhibit increased movement of their legs in the putative non-stepping period: they become rather fond of kicking their feet in the air as they lie on their back. These are rather striking when combined with a third: that these supine kicking movements exhibit the same kinematic profile (*viz.* structure in time and space) and a similar pattern of muscular activity to their precocial stepping movements (Thelen and Fisher 1982). From these and other data points, Thelen and Smith conclude that "what had previously been considered as distinct and separate behaviors" are in fact "manifestations of the same motor output performed in two different postures" (op. cit.: 11).

So what had changed? Why do these kids stop stepping when they can perform the movement involved perfectly well? A further line of Thelen's research indicates that the affordance fades as they lose the *(P)* property of a delicately balanced force-to-mass ratio. Newborns get fatter faster than their muscles can handle, they are simply not strong enough to keep their (perfectly healthy) chub in check. With this in mind Thelen, Fisher and Ridley-Johnson (1984) compared stepping frequency with the rate of change in newborn infants' body mass over the first month of life. They found that "infants who had gained the most stepped less" than infants who were gaining weight more slowly, suggesting that it was the *rate* at which body mass increased rather than simply body mass itself that caused the stepping reflex to recede (ibid.: 485). To further probe the ways in which muscle strength is an index of the bounds of movement, they investigated the extent to which stepping could be influenced by adding and relieving weight. And, unsurprisingly, after being loaded with the equivalent of an extra two weeks' worth of body mass, infant stepping decreased significantly in frequency and height, whilst submerging them in a bath of warm water (see Figure 3) produced the opposite effect (ibid.: 489).

**Figure 3– Stepping reflex in the human infant**

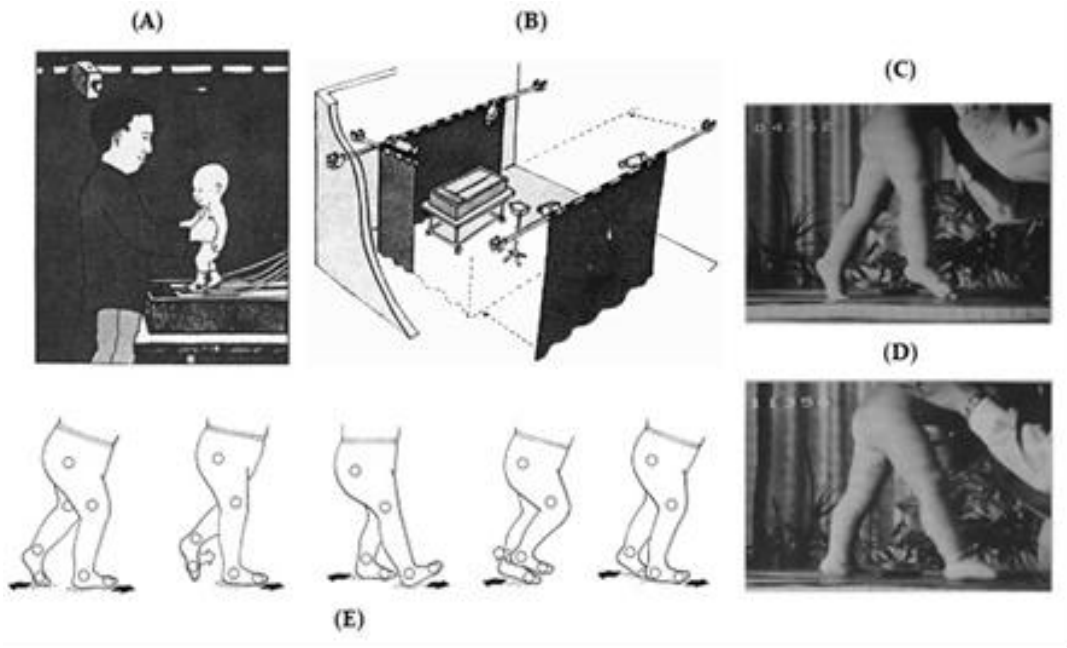



(A) 3-month old tested for stepping with feet on table; infrared light-emitting diodes are visible on the hip, knee, ankle and toe joints.
(B) Stepping reflex induced in the same 3-month old when submerged in warm water.
Both from Thelen& Smith (1994: 12).

However, if this is to be attributed as an action, it needs to exhibit the requisite task-specific flexibility. Some indication of this again comes from Thelen's work. She discovered that 7-month-old infants (*i.e.*, infants who might otherwise be thought to be at a non-stepping age), if provided with upper body support, produced pronounced and smoothly coordinated stepping movements as soon as they were placed on a treadmill (Thelen 1986). Soon after (inspired by studies on quadruped gait) Thelen, Ulrich and Niles (1987) placed infants of the same age on a split-belt treadmill (see Figure 4A & B), and observed smooth adjustments of swing and stance when the belts were run at different speeds. This work has been followed up more recently in an elegant study by

Yang, Lamont and Pang (2005). From their sample of 5-12-month old infants they report that the majority could not only rapidly adapt to belts running at 2:1 ratios, but could also produce coordinated stepping on belts running in opposite directions (see Figure 4E)!

**Figure 4– Treadmill stepping patterns in pre-walking infants**




(A) Infant being supported on a split-belt treadmill.
(B) Split-belt treadmill filmed from four angles with infrared cameras (see curtain rails) and an ordinary video camera (positioned on the floor beside the treadmill).
Both from Thelen& Ulrich (1991: 49).
(C) Toe-strike stepping.
(D) Flat-foot stepping.
Both from Ulrich (1997: 326)
(E) Bidirectional split-belt stepping: left leg steps forward, whilst right leg steps backward. Solid arrows indicate belt-motion and foot-motion during stance. Open arrows indicate foot-motion during swing.
From Yang, Lamont, & Pang(2005: 6874)

Typical adult stepping involves a rolling foot motion in which the heel strikes the ground first, moving through the flat of the foot to the toe. By contrast, neonatal stepping often involves toe-first strikes (see Figure 4C) and the occasional flat-footed contact (see Figure 4D). To investigate the intermediate stages more closely, Thelen and Ulrich (1991: 36ff.) looked for individual differences that might predict the emergence of stable treadmill stepping in the first year of life. They discovered a trend of poor treadmill performance associated with toe-first striking and/or inward foot rotation, whereas good swing and stance came with flat-footed contact. Only in the latter posture can the stance leg be pulled back far enough for a sufficient frequency of muscle spindle impulses to loop through the spine and activate the antagonists, so the leg can then swing far enough in front for a stable step. And, as Thelen and Smith explain, the

flat-footed postural arrangement is only available given the *(P)* property of well-balanced relative tension of antagonistic muscles.[37] In particular, the tension of flexors needs to be within a certain range:

> *[If] the tension [in flexors] is too loose, the treadmill will not impart sufficient stretch to overcome the inertia of the leg and it will not swing forward [alternatively, if the flexors are too tight] the treadmill will not impart enough pull to stretch it. In neither case will the stretch receptors be sufficiently activated for reciprocal phasing.* (Thelen and Smith 1994: 112)

With that I hope to have sufficiently illustrated the delicate balance of *(P)* and *(W)* properties in virtue of which basic actions are possible, and thereby given a rudimentary sense of what structural affordances actually are. This is little more than a beginning of the study of structural affordances, for although (as reviewed above) the relational properties which they typically involve are well understood, the relations themselves are rarely identified as such.

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[37] The developmental story perhaps runs more fully as follows:

*Newborn infants have a characteristic flexor bias in their limbs; legs and arms are held tightly to the body [...] probably partially as a result of the tightly packed fetal position [...] the limbs are relaxed only over many months, and indeed extensor strength in the lengths lags behind flexor strength throughout the first year.* (Thelen and Smith 1994: 112)

It is likely that it is for these reasons that “[h]ighly flexed individuals and those who did not have sufficient extensor strength” could not put their foot flat on the belt, and consequently “did less well in treadmill stepping” (ibid.: 112).

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# Complexity – emergence – ecological cognition

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**Abstract**

The present article constitutes an attempt at a review of a few selected questions related to the complexity paradigm and its implications for research on cognition, especially within the so-called ecological approach framework. I propose several theses, among others concerning the two contrary tendencies within the dominant methodology (the propensity to search for simplicity and the growing emphasis on recognizing complexity), as well as the ontological consequences of the phenomenon under discussion (ontological emergence and processual emergentism).

**Keywords:** complexity; ecological cognition; emergence; affordances; processualism.

The present article constitutes an attempt at an overview of a few questions related to the complexity paradigm and its implications for research on cognition, especially within the so-called ecological approach framework. I propose several theses, concerning, among others, the two contrary tendencies within the dominant methodology (the propensity to search for simplicity and the growing emphasis on recognizing complexity), as well as the ontological consequences of the phenomenon under discussion (ontological emergence and processual emergentism). I begin my reflections by presenting problems connected with the differentiation between an organism and a machine; I further consider to what extent the so-called cellular automata can help in studies on life; in this context, I touch upon the subject of emergence and, against this backdrop, I put forward a few remarks regarding ecological approach towards cognition in the context of the theory of complex systems. Subsequently, I refer to a number of problems/doubts that those commenting on the aforementioned research program are concerned with. The final part of the text is devoted to formulating general philosophical conclusions connected with an approach that could be described as processual emergentism. The following deliberations merely outline a research field and do not make any claims to having exhausted the subject in the case of any of the discussed issues. The majority of included comments have been based on the materials from a special section of texts devoted to the concepts analysed herein which was

published in the journal *Ecological Psychology* (Turvey 2008; McClamrock 2008; Bickhard 2008; Chemero 2008; Petrusz 2008; Juarrero 2008). Thus, the essay constitutes a kind of a broad review of critically evaluated issues and an attempt to sketch the prospects for further research.

**Complexity**

As it turns out, complexity constitutes one of the more crucial issues in both contemporary science and philosophy. Despite our looking for simple rules and longing for fulfilling the dream of completely reducing reality to its smallest, indivisible, fully defined elements, out of which everything previously dismantled can be built anew (which I would term a "block model of the world"), everything turns out to be increasingly complex. It can be said that alongside the earlier (remarkably, often successful) tendency to seek simplicity, there has appeared a new, no weaker one, connected with noticing complexity. The environment is complex, as is the organism living and exercising cognition therein; finally, cognition itself appears to be a much more complex phenomenon than it might have originally been expected. The world, as a whole, presents itself as a set of complex states, or, more precisely, processes. It is defining complexity that remains a problem, as one could inquire whether the aforementioned elements of the world fit into one qualification of complexity, or whether we should talk about different kinds of complexity (Wrześniewski 1995). There are attempts to answer this question within the framework of a philosophical-scientific reflection on the phenomenon of complexity. In my opinion, it is impossible to provide one satisfying definition here. On the contrary, one should endeavour to develop several complementary definitions which would make it possible to encompass all kinds of complexity without falling into overt simplification at the same time (Gell-Mann 1996: 55). The definitions should not, I believe, be too broad, but they cannot be too restrictive, too narrow in their scope. As the complexity of the world and its dynamics is observed, there exists a clear tendency towards at least a partial abandonment of faith in the possibility of setting unequivocal conceptual frames.

To conclude these remarks, one ought to re-emphasise the significance of the existence of two differing tendencies in the history of human reflection. We used to live in the age of simplicity, but now we are increasingly noticing the complexity of reality and of the cognitive process itself. Contending the complexity of the world requires redefining the notion of simplicity as well. Since simple laws and dependencies lead to complex effects, such as chaotic phenomena (Baranger 2011: 7), there appears a problem with separating the former from the latter; to a degree, the old demarcation lines have become blurred. All in all, one could say that complexity has taken the kingdom of simplicity by storm, and that our picture of the world will never be the same as it was.

**Organism - machine**

The development of research on artificial life, robotics, neuron nets leads to new questions regarding the nature of life and cognition. Nowadays we simulate, build copies of units that live and cognize; one could wonder whether this results in a better understanding of the aforementioned phenomena. In this case we return to the old considerations of Kant, who posed questions about the differences between a living entity and a machine[38] (Turvey 2008: 241–242; Juarrero 2008: 279). His conclusions were radical: that human and other organic creatures are not machines, that this kind of a "thing" is "[...] the reason for and the result of itself" (Kant 1986: 328). A body is, thus, an autopoietic system (Maturana & Varela 1980: 135), to use a more contemporary term. It is characterized by a particular closedness, but also openness, a relationship with the environment. This last property is nowadays the subject of research on distributed cognition, enactivism, ecological cognition or robotics. In this context there exist some constantly returning problems, which concern the nature of life, and the related question regarding the legitimacy of using the term "life" when discussing artefacts. This is significant due to the fact that within the aforementioned studies biology remains a distinguished field, as it is machines that imitate biological processes, and not the other way round. The "game of life" constitutes only a simulation of life (Lubiszewski 2011) but the situation of a robot that would be able to cooperate with the environment, learn, repair its own flaws, or procreate in some way[39] presents bigger problems still. The border between the natural and the artificial has become rather strained in the recent years. The definition of life, which one could concoct from the literature of the field, is based on several determinants:

– self-replication

– self-repair

– adaptation

– learning

– being an open system.

There is, however, no agreement as to which of these conditions should be considered necessary, and which sufficient for life to come to exist. It is a phenomenon which does not allow for an unequivocal account. Reductionist descriptions and suggestions of a biochemical definition of life do not exhaust the spectrum of the answers. Today, life is defined as a very broad notion - firstly, one ought to answer the question, what

[38] Kant's questions were primarily concerned with teleology, which contemporarily takes on a naturalized form of teleonomy.

[39] This would be the situation of a robot capable (according to the possessed algorithm, the equivalent of DNA) of constructing, using the materials available in its environment; a microrobot capable, in turn, of independent development. It may sound like a fragment of a science-fiction plot, but it does not overly exceed our imaginings regarding the future.

kind of life are we asking about? Does the question regard a virus, a cell, a simple organism, or maybe a human being? A description utilizing the abovementioned features of a living unit would look slightly different for each of the specified levels. In the context of the theory of complexity we seek such a definition of life that would extend its scope to all its manifestations. In such an approach, life is a property of dynamic open systems, which retain their identity while interacting with the environment, and are able to produce and re-produce themselves. This kind of an autopoietic system is characterized by self-organization (Petrusz 2008: 272), its "behaviour is regular without being regulated" (Gibson in: Turvey 2008: 241).

**Cellular automata and chaos**

Cellular automata are mathematical models which consist of a defined grid of cells, a given initial state and the rules of moving between subsequent states. Hence, one of the most known automata is called the "game of life": most briefly, it can be described as a machine simulating life, or, more precisely, two of its aspects: birth and death. Both in the discussed example and in others, the mathematical model is transformed into a computer program, which allows for a long-term simulation of the automaton's behaviour. The life which we manage to simulate with the use of such an automaton is not compiled, the "cells" of the board on which the "game" is taking place can be only "dead" or "alive." However, this simple phenomenon frequently leads to surprising results, such as complicated patterns which change periodically, fade after a certain time, or, theoretically, there is no end to changes in them. Visible problems arise when we try to include a more or less complex environment in this simulation, which consists in increasing and differentiating the entry population and changing the rules. Therefore, the discussed automata are very sensitive to slight changes of the initial parameters. This phenomenon is well known from chaos theory, where it occurs under the name of "the butterfly effect." We are able to observe changes in a given set, but the possibility of prediction is very limited - "the lack of explanatory power runs deep" (Stewart & Cohen 1997: 76). One could ask how legitimate is it to talk about learning the rules governing life and complexity itself in this case. A lesson taken from research on chaos theory may, again, be of help. We describe chaos as deterministic in order to emphasize that it is not the everyday meaning of chaos (a commonplace lack of order), that order can emerge out of such chaos, but, frequently, this order is only partial, and, more importantly, the behaviour of such a system is unpredictable. Determinist processes underlie both the chaotic phenomena, and the behaviour of cellular automata. However, at a certain point there may occur a qualitative change which cannot be predicted from the basic rules. We talk of emergence then. Subsequently, this leads to an important assessment - in this case, we can describe a system (e.g., a so-called route to chaos) rather than establish rules, which, as in classic science, allow us to make predictions, obviously with a margin of error. Within the framework of the theory investigating the behaviour of systems which are far from equilibrium and being non-linear, determinism is preserved only partially. Classical causal determinism should be forgotten. Here, the determining of phenomena becomes a much more complex phenomenon, and causalism does not suffice (Petrusz 2008: 275-277). There

appears the issue of interpretation, which, I believe, belongs to the field of philosophy. On the one hand, one could insist on the old image "despite it all" and claim that the image of the world resulting from the research on complexity is illusory. The impossibility of establishing initial conditions with infinite precision is then treated as a difficulty both technical and epistemological in nature, while the problems with predicting are blamed on the computer efficiency, inadequate for the scientists' needs. This resembles the situation of the proponents of classical determinism in the context of the debate on quantum mechanics. It is always possible - even despite one's experiences - to refer to "hidden variables" and insist on the falsity of the Copenhagen interpretation. In the extreme version of such an approach, Laplace's demon would still fare quite well. On the other hand, in the context of the research on complexity, a thesis is posed that the obstacles are lurking not in us, but in the very structure of reality. In one of the versions of this argument it cannot be predicted how the complex systems which are far from equilibrium may evolve as the system itself is the fastest computer capable of "calculating" all the necessary equations (Halpern 2004: 185). In order to learn what such an evolution will look like, we cannot but wait patiently and draw possible conclusions *ex post*, albeit without any guarantee that they could be reasonably related to future events. In such cases, a simulation fails from the point of view of ontology of reality as well. In order to conduct a simulation of a system in which every element might play the role of the proverbial Lorenz's butterfly, one would need to carry it out on the 1:1 scale. Simulating weather on such a scale turns out to be pointless from the very start. Simulating is an activity similar to drawing a map - what sense would it make to take a stroll though the town with a map imitating every slab of pavement?

To a large degree, living systems resemble chaotic systems; the exact level of aptness of this comparison is unknown, but the similarities do not seem accidental. Thus, life could be termed a chaotic, nonlinear property. The analogy between chaos and complexity turns out to be extremely important[40]. The very assumptions underlying the so-called complexity sciences deny the possibility of discovering the laws of complexity similar to the old, classic ones. Nonlinear processes, deterministic chaos, prediction problems, explanation being just a description -- all these phenomena result in scepticism regarding the possibility of establishing unequivocal rules. However, I believe one could talk about a new kind of science, a new approach to explanations. Just as after the quantum revolution the classical science turned out to be a useful approximation on a scale of a certain size, the awareness of the nonlinear character of complexity does not eliminate the old description. Still, one ought to realize how incomplete the classical descriptions are. As it is usual in such cases, the problem of reduction returns. In the methodology of research on cellular automata there can be found the conviction about the possibility of achieving reduction - reducing complexity to a few simple issues. From here, we are only a step away from deciding that there occurs a certain continuum from the physical and chemical phenomena to the biological ones. Since in chemistry we deal with the phenomena of self-organisation (e.g. the

[40] However, one ought to remember that complexity is a broader concept. A common feature of chaos and complexity can be found in non-linearity. Complex systems display chaotic behaviour, but chaos can also appear in simple systems (Baranger 2001: 10).

Belousov–Zhabotinsky reaction), we can assume that even the behaviour of an organism could be ultimately explained by studying the patterns of behaviour of such simple systems. Said tendency to notice complexity very clearly meets here the desire to return to the reductionism of a few simple rules. However, I think that this might be only another dream about reduction, and that the truth lies somewhere in between. Complexity is specific to various levels of reality, and although some generalizations can be provided, we are far from creating a science of complexity, within which, regardless of the scale, we would be dealing with the same general pattern of causality - as e.g. Chemero (2008) would want it. Still, this leads to forgetting about the problems regarding the nature of such potential laws. As it happens in the case of chaos, searching for the laws of complexity is encumbered with numerous stipulations. The reductionism we might talk about in such a case has little in common with its classic version, to which we are so used. In its classic version, reduction *de facto* boiled down to relating mechanical causes; our picture of the cause and effect relationship has not changed much. However, nowadays we are aware that the problem of determination is more complex (Bunge 1968: 30-38). In the world, there cooperate different kinds of phenomena determination, which cannot be reduced to simple causes and results found in unequivocal relationships (Petrusz 2008, Juarrero 2008: 278-280) . At this point we touch upon the aforementioned extremely crucial issue of how new things appear (emerge) in the world, that is, the issue of emergence. The difficulty in pointing to simple cause-and-effect dependencies has resulted in an attempt to admit that a radical novelty is a natural property in the world. Where we have a cause (an earlier, less complex stage) and a result (a later, more complex stage which has new properties, behaviour or structure), the element that would connect both states is still lacking. In my opinion, this “something” is ontological emergence, which refutes the idea of a universal microreduction (Silberstein, McGeever 1999).

**Emergence - naturalization and cognition**

Nowadays emergence has apparently become “disenchanted” as a result of naturalization. Scholars presenting a scientistic approach hold that emergence will soon become an element of a highly formalized and mathematized science concerning complexity. From a philosophical point of view, these demands can be read as a result of the tendency to look for simplicity and reduction. However, I think that such an attitude towards the matter does not solve the problem. Without prejudging the development of science, we now rather ought to agree on a version of ontological emergence according to which we abandon the hard reduction postulate even in its oblique forms - emergent properties are, on their own, natural, emergence does not require naturalization, but rather simply accepting it into our framework of perceiving the world. The problem lies in finding the middle ground between the tendency to reduce phenomena and irreducibility as an important characteristic of emergence. These tendencies can be seen as contradictory, but at the same time they seem to lead to the most adequate description of reality. While talking about ontology, one cannot however forget epistemology - traditionally, epistemological emergence is juxtaposed with the ontological (metaphysical) kind. The epistemological version can be termed “ostensible” - it

is a result of the imperfection of our cognitive apparatus, a function of the state of knowledge at the given moment. What today is described as 'emergent' does not have to be so in a hundred years. This is relative emergence, which will be explained and ultimately "disenchanted." Another epistemological issue is the question of emergence in the very cognitive process. Perception, thinking, experiencing emotions might be described as emergent phenomena. Emergentism is, therefore, an approach useful in the analyses of cognition itself as well. If we consider the cognizing subject in all its relationships with the environment, retaining at the same time the memory of its complexity as an organism, undoubtedly we arrive at the necessity of developing such a concept that would connect into a whole the theory of complexity and emergence in the context of ontology and cognition theory. In this case, the concept of so-called ecological cognition, originally developed by James Gibson, appears useful.

**Ecological cognition, affordances and hypersets**

In the context of the theory of complexity, cognition is described as a dynamic system, engaging the subject and the environment. Perception, as well as the mental "handling" of its effects, constitutes a process wherein, on various stages and levels, one can talk about emergent phenomena. Research on cognition is problematic due to several reasons. Perhaps the most important of these is the one which stems from the fact that, as Gibson put it, "All forms of cognitive processing imply cognition so as to account for cognition" (in: Turvey 2008: 241). This state of things constitutes a serious challenge, as it implies a variation on the old problem of the subject - object relationship, wherein we study a tool with the use of this tool. Cognition is never a given, it is a process which we try to grip by the means of a process that is numerically different, but, ultimately, qualitatively identical. Thus, we arrive upon a specific kind of an aporia - there appears a question: how can one reduce cognitive phenomena, is it not so that a dissection into primary factors results in an irretrievable loss of the subject of the inquiry? To what extent should it be a description of a dynamic system, retaining its identity and integrity, and to what - a dissection into primary factors? Can the diachronic and synchronic perspectives be reconciled? Can the dynamism characteristic for cognition be described in static terms? We can find here clear signs of dualism, which makes a fully unitary description impossible. Once again, as in the discussion about the issue of the essence of life, one ought to answer a key question: how precise should the description be, which determining factors should be taken into the account? It seems that it is easier to further multiply the question marks, rather than provide any constructive answers.

However, there exists quite a broad trend in the contemporary research on cognition, wherein scholars make attempts to answer the question posed above. Herein one can point towards the enactivist approach, the embodied mind concept, studies on distributed cognition or precisely the ecological approach. All these concepts offer answers to basic questions, such as: Where does cognition begin and end? What is the cognizing subject? What is the very nature of cognition? which are different from the classical ones (that is those Cartesian in spirit).

The subject seems to lose its former distinctness, separateness, it is fixed, embedded (immersed, anchored) in the environment, its borders become blurred (Bickhard 2008: 254-255). To put it differently, the inner/outer categories gain a new articulation. Within the ecological approach, one attempts in this way to understand the concept of affordances, which is key to this perspective. This issue has been taken up in his account by Chemero (2008). The author refers to two possible ways of understanding affordances: as dispositional properties of animals complemented by the dispositions which are contained in the elements of the environment (Turvey 1992) and, according to his own, previously proposed perspective, as higher-order relational properties of animal - environment systems (Chemero 2003; 2008: 263; Chemero & Turvey 2007: 31)[41]. Ultimately, Chemero deems both approaches to affordances as close to each other, insufficient and overly static. As he claims, there a theory of "affordances 2.0" is what is really needed (Chemero 2008: 265). They are to be more mutable over time, individual, based on a constant interaction of elements. The relation connecting all the elements of the system wherein cognition occurs ought to be re-formulated in order to emphasise the circularity of determination - every element (function, process) of a system is, at the same time, its own cause and effect (Chemero 2008: 259; Turvey 2008: 242; Petrusz 2008: 271). Such a system is closed to outside causative reasons: this feature is defined as autonomy. What Chemero has aimed to do is to describe affordances as a complex, autonomic system. Moreover, he attempts to delineate dependencies within the subject - environment system with the aid of the set and hyper set theory. The latter are better suited to outline the interesting relationship due to a specific property - they are their own elements (Chemero 2008: 257)[42]. That last property allows, in turn, to put the property of being a complex autonomic system into graphs of a circular structure (Chemero 2008: 258). In this context, complexity has been defined as a property of the system whose hypersets have loops on the graphs that present them. In this case, one can refer to a specific self-referentiality of the elements of a complex system.

However, the project of formalizing the subject - environment dependencies has not been finalized. The author uses examples to show how the subsequent layers of reality can be described as complex and autonomous systems - chemical reactions, cells and organisms (it could probably be shown that such a description fits the microworld as well). The problems begin at the point where the affordances, that is environment - animal relations, are described. The graph proposed by the author in this case depicts a system which is complex, but not autonomous according to his understanding (Chemero 2008: 264). Perhaps for presenting such a complex system a more advanced

[41]In others words, affordances are described as relations between the properties of environmental situations and skills of the animals. To be more precise, one should add that there exists at least one more meaning of the term "affordance," which is however connected with the notion of mental representations, and thus incompatible with Gibson's own ideas, as he is an anti-representationalist (Chemero & Turvey 2007: 31).

[42]The hyper set theory is based on the set theory suggested in 1908 by Ernst Zermelo, and completed by Abraham Fraenkel (Zermelo-Fraenkel's axioms). Within the hyper set theory one of these axioms is negated, which is the axiom of regularity (or of foundation): "Every non-empty set X contains an element disjointed with X" (see Murawski 2001: 189). Hypersets do not contain such an element, all the elements of a given set, and the set itself, are elements of the same set, that is the set is its own element (X Є X) (Chemero 2008: 257). In this case we talk about unfounded sets.

means od depiction should be used - e.g. computer graphics, where the dependencies could be pictured as more dynamic and more closely related to reality. It is not only the affordances themselves, but also the graphs which should be upgraded to a 2.0 version. However, in my opinion, the difficulty lies not in the methodological problems (the graphs become less legible for very complex systems) as much as in ontological and linguistic issues - not all elements of the system turn out to be autonomous according to the author's understanding, and the description of dynamic dependencies in the technical language of sets comes off as very static.[43] The author himself notices the indicated difficulties, but he believes that the chosen way of formalization is the right one, that the description of the affordances should be more dynamic, suited to an individual situation, that it needs to make use of the enactive approach to cognition. Ultimately, he suggests a rather complicated graph (Chemero 268), containing three elements: affordances, abilities denoted by sensory-motor coupling, and the nervous system. The entire relation is to be a combination of two macrofactors - the "perception - action" system and the autopoietic system. Only all those elements together are to present the animal - environment system, characterized by autonomy and self-organisation. However, it has to be noted that such a picture connects so many heterogenic components that it seems valid to ask about its adequacy. When it comes to the purposefulness of using graphs in describing complex phenomena, it is possible to raise a much more basic objection. And so Chemero describes the situations that interest him via graphs, but a question can be asked, does adding a "loop" to the graphs (complex system) explain the relation itself? In this context there appears again a need for a theory of emergency, as all "this" which happens between particular vertices of the graphs is precisely emergent phenomena. The discussion above can be summarized by saying that affordances are emergent states of things (situations), which engage different elements of the environment and the subject, related to the overriding cognitive relation. As such, it is with great difficulty (if at all) that they submit to our descriptive treatment, in any form.

### "Worries" and problems connected with the ecological-complex approach

The issues discussed above can be grouped into several larger wholes. In his article, Ron McClamrock (2008) presents such a juxtaposition. He talks about "worries," dividing them into "conservative" and "progressive" ones. The former are related to the still vital reductionism and physicalism, and simple darwinism; they are "some kind of metatheoretical or even metaphysical roadblock" (McClamrock 2008: 245). The latter can be associated with the discovery of chaotic phenomena, the search for the theory of everything, and the description of the self as connected with the outside world.

[43] It can be added that the postulated closure, self-sufficiency of such a system is a relative property. It is balanced by the openness of the system, as interactions with the environment result in an inability to isolate living systems. The terms suggested by Chemero seem too scanty. In my opinion, this is the source of the author's problems with recognizing affordances themselves as complex, and, at the same time, autonomous systems.

Among the conservative worries the author emphasises, above all, those that touch upon the basic issues for the contemporary philosophy of science. These worries concern, first of all, the issues of causality and reduction. As McClamrock notices, despite the emergence of weakened versions of materialism (token, nonreductive), the tendencies towards rejecting contextual causality or macrocausality remain very strong (McClamrock 2008: 245). In this case, materialism is connected with physicalism, leading to the conviction that the properties (objects) deemed material may be described only in physical terms, whereas every other kind of description is supposed to lead to their being deprived of their causal ability, and discarded outside the borders of science. This attitude is echoed by methodological and metaphysical individualism (localism), within which attempts are made to avoid explaining phenomena by referring to the context (environment). Explanations are supposed to be based on the principle of microdetermination, and looking only for the closest causes. Fundamental questions immediately come to mind, such as: what does "physical" mean? When using this predicate, do we mean an object unambiguously located in space? Maybe one that possesses mass? Do simple physical entities occur in nature? These are the questions which microphysics attempts to answer with great difficulty. Today it is frequently physics which is distant from the "classical" language of physicalism and reductionism, used only to describe a certain section of reality - the world between the description of quantum mechanics and the aforementioned macrodetermination phenomena - and even this with numerous idealizing assumptions. Simplicity as a determinant of a "good" theory of explanation has ceased to suffice. Rather, it should be assumed that reductionist explanation does not come into conflict with other kinds thereof. Methodological individualism (localism) can be thus reconciled with contextualism and macrodeterminism.

The ideas described above have a wide range of influence, affecting also the sphere of the philosophy of the mind, and, more precisely, the issues of internalism and phenomenology (meaning here the first person descriptions of mental states). In this case there still lingers a post-Cartesian outlook on cognition, consisting in a belief in the possibility of locating precisely mental states and their identity, when the structural identity of brains occurs (McClamrock 2008: 247). That last conviction leads to the thesis regarding supervenience, which in this context frequently approximates very much the classical reduction. Additionally, the idea of the "Cartesian theatre" comes into play, that is the approach of strong internalism, according to which that which is mental becomes identified with that which pertains to the brain, which is enclosed within the skull. All the aforementioned beliefs have their basis in the already discussed assumptions of reductionism; similarly, when it comes to causality, all the distant causal relations should be explained via well-localized and close causes (the physical brain is in this case the best explanation for experience). McClamrock attempts to show that in this case the ecological approach together with the idea of a brain / organism system connected with the environment is much more adequate, as it is frees one from seeking the "closest," simplest causes. Cognition, as the subject itself, takes on a very clear processual shape, the mind is no longer a decision centre established once and for all; in a certain way it becomes "separated" from the brain: the borders of the skull are no longer its borders, the body and the environment acquire meaning as a

no-less-important part of the coupled cognitive system. Does it then lead to an invalidation of the classically understood subject? I believe that this would be a far-fetched conclusion. The first person perspective remains our original, inalienable experience (*empiria*). Again, as in the previous cases, the tendencies towards one type of explaining should be met with recognition of the necessity of the existence of a different one - in this case, involving emergent macroproperties, such as the first-person experience or intentionality.

Apart from these problems, McClamrock discusses also the aforementioned "progressive worries" - the first of these being chaotic emergence, which is the basis for describing the systems characterised by nonlinear dynamics. As it turns out, the difficulty lies in reconciling the descriptions of the chaotic level and the one that is superstructured over it (McClamrock 2008: 248); it is not easy to achieve a model within which different levels interact as determining each other. Our being accustomed to the classically understood causality can have some bearing on this matter. Another obstacle lies in separating actual causality from what only impresses us as exhibiting such. In other words, one can ask: how to differentiate between actual causes and the illusory ones? Which regularities are important for an adequate description of the discussed phenomena, and which seem to be only an artefact of analysis? In accordance with the author's conclusions, it ought to be emphasized that there is no one good answer; first of all, we need to agree on what we are looking for, and how accurate the description should be in order to suffice. Moreover, a more basic question needs to be answered: is the theory of chaos itself enough to explain to what degree it corresponds to reality? There is no agreement today even with regard to that last issue (Poznański 2003: 13-14). It is not far from there to another question about the point of looking for "the theory of everything." According to the author, macroreductionism (which can be defined as reductionism *à rebours*) shares with the former, reductionist paradigm, the same dream about a complete theory of cognition and complexity. Such demands are, however, excessively ambitious, and the impossibility of fulfilling those expectations occurs already at the starting point. Thus, we ought to abandon such a quest and, instead, focus on the analysis of specific cases, "get dirty into detail" (McClamrock 2008: 249). One can notice that from this point of view we return again to the model of classical science. The author recommends a far-reaching caution; however, I believe that the issue can be dealt with differently and that more general inquiries do not have to be dropped. I return to this issue in the following paragraph.

That last issue discussed by McClamrock touches upon vital notions, which are not only ontological or epistemological, but, above all, ethical in nature. It is a vision of a self that is radically scattered and "world-permeated." This self becomes "thrust back into the world" (McClamrock 2008: 250), and, in a sense, distilled within it, which, in turn, results in the necessity of posing a question about the existence of a specific "decision centre" which would possess ethical implications. The existence of such a "centre" is sanctioned by a long tradition of a self-determining self guaranteeing responsibility for one's own deeds, the existence of an autonomous subject as the bearer of rules and obligations. Personal identity, which has also been subject to certain erosion, constitutes the ontological basis in this case. Within this new view of ethics and morality, a larger emphasis is put on environmental activity and the slow working out of

appropriate dispositions as a part of interacting with other subjects. One can, then, talk about a renaissance of the virtue ethics, dating back to Aristotle and competing against the heretofore dominant vision of a world of ideas that is transcendent to an individual (Juarrero 2008: 281-282). Such a vision results in, among others, the issue of a larger responsibility in the context of a simultaneous larger uncertainty regarding foreseeing the consequences of one's own actions. The subject is stripped of the support provided by the classical paradigm, according to which one can make pronouncements based on imperturbable instances. Thus, the complexity of the world, contextualism and processualism give rise to new questions of ethical nature, to which there are no simple, unambiguous answers.

**Philosophical conclusions - processual emergentism**

In my opinion, the issues discussed here result from the aforementioned problems connected with the new ontological paradigm and the contradictory tendencies within contemporary science. I think that solving these problems would require not only a change of language, but of the entire paradigm of both science and philosophy. It appears problematic to dismiss the picture we possess and to understand that even if a description is pragmatically effective, it might be ontologically wrong (in the past we would have said it does not reach the "essence")[44]. Philosophy, which draws on science, does not have to limit itself to methodological issues; it should suggest a broader picture of the reality, which would not be philosophy of science as much as philosophy of nature. Obviously, in this case a certain "work at the foundation," the analysis of concepts and specific assertions, also counts, but it is likewise important to build an overall vision of the reality (picture of the world). I think this is the role of philosophy. It is not a science in the meaning of empirical or axiomatic sciences. Perhaps there is no chance for a "philosophical theory of everything," but philosophy can suggest metaphysical hypotheses regarding the phenomena described by science, not necessarily falling into some kind of cheap mysticism. In this sense it can propose a "theory of everything," albeit with serious stipulations regarding its perfection and absoluteness. It will never be able to fulfill the severities of a classical scientific knowledge paradigm; it has no such ambitions (Lemańska 1998).

The new outlook at the structure of reality and of cognition itself provides an opportunity to draw from this state of things far-reaching philosophical conclusions. One of the crucial elements of such a picture is the idea of emergence, formation of creative novelty; in this context, as Robert B. Laughlin (2006: 208) notes, one can pose a thesis regarding the transition from an era of reduction to an era of emergence. The philosophy of process might provide useful context in this case, as this discipline seems predisposed for this role due to its being oriented towards describing a dynamically

[44] Hard science and philosophy differ fundamentally in the way they describe reality and in the standards they impose upon themselves. In science, a reductionist description, which unambiguously localizes properties and entities in space and time, can be enough, but it can be insufficient in the field of philosophy - in the philosophical context it can be the same thing as "errors." What can be an inessential problem for a scientist - e.g. the impossibility of performing an infinitely precise measurement or the incompleteness of induction - can constitute a real challenge and a source of radical claims regarding the world for a philosopher.

changing reality. Within the processual paradigm, new life is given to the old Heraclitean variabilism, and it is stability, not change, which is treated as something fundamentally mysterious, requiring an explanation (Bickhard 2008: 254). Therefore, the philosophy of process attacks our pre-judgments regarding the nature of reality, resulting from a culture based on stability. Because of this, as in the case of ethics, a change in thinking encounters large difficulties; frequently, we do not fully realize how deeply certain views are rooted, and how they work by means of a certain inertia (Bickhard 2008: 252). However, I believe that the attempt to deem variability or process to be a basic property of the world does not solve ontological issues; there is still a long way to go before understanding the nature of complex dynamical processes. In other words, I am convinced that while it is necessary to increase our appreciation of the processual vision, both stability and variability remain equally mysterious[45]. Both of these poles demand being described and accounted for in the structure of reality. As in the discussion between the emergentists and reductionists, in the dispute between the followers of Parmenides and of Heraclitus it is advisable to retain moderation and look for a consensus. To put it somewhat simplistically, I am convinced that "the truth lies in the middle" and that it is possible to construct a precise picture of the world even from the apparently inconsistent elements. At this point it is more important to shift the emphasis and appreciate the dynamic - holistic elements. Obviously, the world is a dynamic process, but there are structures emerging from it (Metallmann 1933), which are characterized by a relative stability. Philosophy may help in reconciling different descriptions of reality - monism and dualism, diachronics and synchronics, reductionism and holism do not necessarily be treated as opposites. We might refer again to our attachment to a "block" description of reality, while a process often connects within itself heterogenous elements. One should always remember about contextualism and perspectivism within the theory of cognition. The lack of radically unambiguous answers within contemporary ontology does not have to be a disadvantage, quite the opposite - it is a result of the relationship between the world and the cognizing subject. And this is precisely the conclusion to which research on complexity, emergence and ecological cognition leads.

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# Multimodal dynamics of coordination,
## or Michael Turvey and psychology according to engineers (not only for engineers). Excerpts[46]

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**Abstract**

A major part of Michael T. Turvey's contribution to ecological psychology are reflections on the coordination of movement, in particular with regard to the environment. This suggestion plays an important role in the beginning of the debate on motor control, whose today's meaning is far broader than the original one, including the field of cognitive science. An overview of the progress of the debate has been presented, from the beginning to the present day, with an indication towards its diversity and the role of what Michael T. Turvey suggested 30 years ago.

**Keywords:** Michael T. Turvey; ecological psychology; coordination of movement; motor control; dynamics and self-organizing systems theory; MOSAIC, joint action.

A natural point of reference for Michael Turvey's body of work, especially keeping in mind the entire period of his academic activity, is ecological psychology. In a chapter on ecological psychology he co-wrote with Michael J. Richardson, Kevin Shockley, Brett R. Fajen and Michael A. Riley for *Handbook of Cognitive Science. An Embodied Approach*, Turvey treated the subject in view of six rules: 1) the proper unit of analysis is the organism-environment system; 2) environmental reality should be defined on an ecological scale; 3) behaviour is emergent and self-organized 4) perception and actions are continuous and cyclic; 5) information is specificational and 6) perception is of affordances. Within this set of rules, the rules 3 & 4 seem especially interesting, as while they retain a basic relationship with ecological psychology, they point to a connection with the concept of Bernstein, to whose body of work Turvey attached particular sig-

[46] Excerpts from the entire Polish version (Komendziński 2012).

nificance. This paradigm is connected with the concept of coordination. Bernstein's model evokes another notion important in this context, a notion whose weight I would like to point out, namely, anticipation.

We can refer to five main routes of development characteristic for that time: 1) towards a extension of synergetic modeling (identification of new parameters of control); 2) towards connecting levels of analysis; 3) towards temporal patterning and variability of rhythmic coordination; 4) towards a network of oscillators for perceptive and motor synchronization of complex musical rhythms; and 5) towards synergy as spinal modules or pattern generators with a point attractor or limited cycle dynamics. These two decades of development constitute search within the theory of metastability, and thus, a departure from multistabilities discovered at various levels (see Dagmar Sternad).

At the end of the previous decade (in 1998 and 1999), there appeared works authored by Daniel Wolpert and Matsuo Kawato, which suggested a modular approach that would refer to internal model for motor control. A developed model of motor control, known as MOSAIC (Modular Selection and Identification for Control) was presented by Haruno, Wolpert and Kawato in the article "MOSAIC Model for Sensorimotor Learning and Control" in 2001. In the same year, Wolpert together with Randall Flanagan present in *Current Biology* the work "Motor Prediction." Motor prediction is one of the problems very interesting for us in the context of reflection over coordination and motor control (prediction, applying the model to social interactions).

In the last decade, in the context of discoveries within neuroscience, the possibility of monitoring of brain activity by the means of modern tools and an especially fast development of the concept of embodied cognition, there has been developing research on joint action. According to what the titles of works published by Gunther Knoblich, Natalie Sebanz and their collaborators claim, coordination is therein transferred to whole bodies. These texts show that our minds are to a large degree joint with motility and the body on the one hand, while our bodies become harmonized with other bodies. We achieve simultaneously the embodied and the social dimension of joint action. In this case, coordination begins to have its social dimension. Starting with Michael Turvey three decades ago, the research conducted within the paradigm of behavioral dynamics of coordination is an important component of the joint action perspective, at least within the scope of time coordination. The ecological perspective, represented by Turvey, meets studies over cooperation conducted within joint action. Coordination of movement ceases to be simply a way of organizing movement, human motility becomes an experience. In further research over coordination it turns out that humans not only coordinate their movements with the rhythms coming from the environment, but, importantly, they do not do this within one modality, but, rather, in a multimodal way.

The research of this kind is important for situations from our everyday life, as it allows us to understand what coordinating movement with visual and auditory rhythms of the environment consists in. This allows for a transition from the issue of motor coordination in the perspective of its dynamic interpretation to another question of current importance, that is sensory multimodality and intermodal integration.

As a conclusion of this route of development of the notion of motor coordination in the perspective of the theory of dynamic systems we may take a look at a chapter from the book *Progress in Motor Control* (2008, edited by Sternad), written by Michael Turvey and Sergio Fonseca, entitled "Nature of Motor Control: Perspectives and Issues". This chapter points to four approaches which the authors see as their sources of inspiration: neuroanatomy, robotics, self-organization and ecological reality. This suggestion should be analyzed more carefully, due to the fact that, as Nigel Stepp, Anthony Chemero, and our main character, Michael T. Turvey, demonstrate in the article "Philosophy for the Rest of Cognitive Science", it is a proposition of a paradigm for cognitive sciences, and especially for the integration of cognitive sciences within the scope designated by the research in the perspective of the theory of dynamic systems, the concept of motor ontrol and the complementary account of embodied cognition, and particularly in the context of anticipation and prediction. This may be the future of cognitive sciences, the road towards which was indicated over 30 years ago by Michael T. Turvey, as based on the earlier indications by Bernstein and Gibson.

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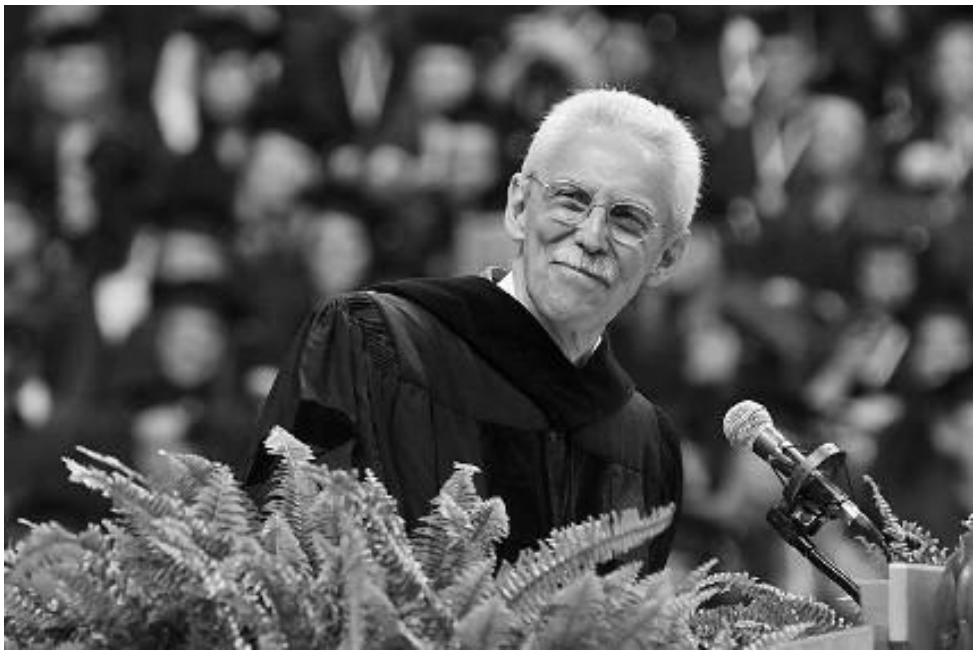
# Michael T. Turvey



Michael T. Turvey received his Ph.D. from Ohio State University in 1967. He joined the University of Connecticut in 1967 and the Haskins Laboratories in 1970. His awards include a Guggenheim Fellow, the American Psychological Association (APA) Early Career Award, Fellow at the Center for Advanced Study in the Behavioral Sciences, Cattell Fellow, Honorary Doctorates from Free University of Amsterdam and Florida Atlantic University, APA Distinguished Scientist Lecturer, Board of Trustees Distinguished Professor (University of Connecticut), Fellow of the Society of Experimental Psychologists, American Psychological Foundation F. J. McGuigan Lecturer, Fellow Japan Society for the Promotion of Science, Ohio State University Distinguished Alumnus, President of the International Society of Motor Control, 2009 Bernstein Prize in Motor Control, the Society of Experimental Psychologists 2011 Lifetime Achievement Award, and the 2012 Glushko & Samuelson Distinguished Cognitive Scientist Award.

He has published over 380 scientific articles, produced more than 40 Ph.D.s, and taught more than 27, 000 undergraduates. His research on perception and action and their inter-relation follows James Gibson and Nicholai Bernstein in emphasizing the search for general laws and principles. His research on visual word recognition pursues the key role of phonology in reading identified by Alvin and Isabelle Liberman. He became Professor Emeritus in 2008.

Privately:

He reads science (widely), philosophy, and newspapers. He listens to classical music and opera but cannot imagine going to a theater or concert. He watches sport on television (primarily, football with the round ball) and rarely anything else. He likes to pub, and has an English pub in his home. It is used frequently by faculty, students, friends, and visiting scholars.

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prepared by Witold Wachowski
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# From Physical Education to Physical Intelligence:
## 50 years of Perception-Action by Michael T. Turvey

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**Editorial Abstract**

Author comments on the changes in his approach to questions concerning action and perception, current and future status of ecological psychology, as well as specificity of human nature.

**Keywords:** Michael T. Turvey; perception; action; ecological psychology; affordance.

## From "A note on the relation between action and perception" to...

From "A note on the relation between action and perception" to "Ecological perspective on perception-action: What kind of science does it entail?" what has changed in Michael T. Turvey's approach to questions concerning action and perception?

I am happy to be asked this question. It gives me an opportunity to reminisce (more than a little) and to underscore what I see as the broad theoretical significance of the ecological perspective for psychology and philosophy, but, perhaps more importantly, for science in the large.

At the meeting of the North American Society for the Psychology of Sport and Physical Activity (NASPSPA) held at the University of Illinois in May 14-16, 1973, I presented a short paper that focused on the potentially deep similarities between perceiving a letter of the alphabet (e.g., A) and the act of writing that same letter. That one could perceive as "same" the indefinitely many variants (in sizes, orientation, and script) of the letter A, and that one could write the "same" variant (more or less) of the letter A using indefinitely many combinations of muscles, muscular contractions, joints, and joint motions, suggested that the principles of A-perception and A-action were (a) extremely abstract, and (b) of like kind. The published paper (Turvey, 1974, *A note on the relation between action and perception*) promoted the idea of mathematical symmetry groups related through an isomorphism.

The 1974 paper was not strictly ecological. It was my first foray into what I then termed "action theory." It was an opportunity to begin thinking about action in ways that I had been exploring perception, ways that were motivated by the style of inquiry expressed in James Gibson's writings, most notably (for me) his 1959 chapter *Perception as a function of stimulation* and his 1966 book *The senses considered as perceptual systems*. Shortly after the NASPSPA meeting I took advantage of a Guggenheim Fellowship (1973-1974) to fully acquaint myself with the Russian literature on movement published in the journal *Biofizika* (translated and published in English as *Biophysics*), particularly the literature inspired by the ideas of Nicolai Bernstein. His primary works had been made available in English in the 1967 publication of *The coordination and regulation of movements*. In significant degree, it was Bernstein's emphasis upon the topological rather than the metrical properties of movements that had encouraged me to think more abstractly about the grounding of the human ability to write the letter A.

At this juncture I should make clear the origins of my interests in perception-action. My undergraduate and Master's degrees were in physical education. I often puzzled over matters such as how to teach a 14-year-old the technique of discus throwing. Or how is it possible for a midfielder in football (I am English born) to hit a 40 m pass on the run to the right winger who, in seemingly one motion, chests the ball down to his feet and sends the ball on an inward curving trajectory to the far post where it is intercepted by the on-rushing striker who directs it by his head into the goal?

My Ph.D. degree (received from Ohio State University in 1967) is in Experimental and Physiological Psychology. As a doctoral student I investigated the pre-perceptual visual information store (later called iconic memory) and short-term verbal memory, and I examined the effects of cortical and limbic system lesions on learning and memory functions in rats. My course work was heavy on learning theory, sensory systems, higher brain functions, embryo- and neurogenesis, and comparative psychology. In the latter course I encountered the concept of "higher-order stimulus" and the name of its author, James Gibson. I was intrigued. That encounter, though fleeting, was pivotal in my career. Some months after, while studying in the stacks at the main library of Ohio State University, and seeking a brief respite from assigned readings, the Sigmund Koch volumes on *Psychology: A study of a science* caught my eye, particularly the volume entitled *Perception*. What next caught my eye was the name James Gibson in the list of contributing authors. I read his chapter (already identified above): *Perception as a function of stimulation*. I read it there and then. My immediate reaction: So that's *why it is possible* for football players to do what they do!

I should also remark on the significance of my study of embryo- and neurogenesis. That material introduced me to Paul Weiss and the unconventional possibility that order is not an *a priori* fact of a biological system (not program-driven, or other-generated) but an *a posteriori* fact (execution-driven, or self-generated).

From 1967 to 1974, at the University of Connecticut and the Haskins Laboratories, I dedicated myself to the parallel challenges of (a) acquiring the skills of scientific experimentation, and (b) abiding Michael Faraday's admonition of "Work. Finish. Publish." I did so as a practitioner and expositor of the information processing approach while

struggling with the relentless rethinking of psychological theory demanded by Gibson's overhaul of the field's foundational concepts. My major accomplishment in this period, I should note, was a body of research on peripheral and central processes in vision (*Psychological Review* 1973) that, along with my experiments on primary and iconic memory ,earned me the American Psychology Association's Early Career Award, the first major award, I believe, to be given in (what was then) the relatively new field of Cognition.

Two papers were written during my Guggenheim year. One linked the ideas of Gibson and Bernstein (*Preliminaries to a theory of action with reference to vision*); the other contrasted Gibson's approach to vision with that of the "seeing machines" of an emerging artificial intelligence (*Perspectives in vision: Conception or perception?*). The Gibson-Bernstein paper was completed early in 1974. The book it was intended for, as a chapter, was not published until 1977. Fortunately, the paper was made publicly available two years earlier in the widely distributed *Haskins Laboratories Status Report*, the same year the conception-or-perception chapter was published. In combination, these two papers became the springboard for a radical rethinking of the scientific status of perception and action.

What was at issue? Gibson's growing insistence in the 1960s and 1970s for an objective conception of information—required in no small part by the universal success of controlled locomotion by foot, wing, and fin—placed novel demands on philosophy, biology, and physics, as duly noted by Robert Shaw (my long-term, and most important colleague), William Mace, Ed Reed, and others. Information in Gibson's specificational sense, rather than Shannon's uncertainty-based sense, is *information about*.

Claude Shannon pursued the concept of information on the working premise that "meaning is irrelevant," adjudging that the concept was more approachable if treated as a mathematical abstraction independent of meaning. It could then also be treated as independent of coding systems, since differences among codes would only be differences in number of coding elements. The successes of Shannon's formulation for machine intelligence and communication are obvious. James Gibson, in sharp contrast, pursued the concept of information on the working premise that "meaning is relevant." As the basis for steering oneself through cluttered surroundings, information must be about whether a surface affords stepping on and bounding from by you, an opening affords passage for you, a brink in a surface affords leaping over by you, and so on. Locomotion is conducted in terms of a practical semantics, in terms of meanings that are activity-relevant. In contrast to Shannon's information carried by code, Gibson sought information as carried by invariants of energy distributions (e.g., multiply reflected light, hydrodynamic flows) structured by environmental layouts and sources relative to a stationary or moving point of observation.

Information in the sense of *information about* ties down the definition of perception as *direct*: To perceive x is to detect information about x. "Perceiving x" and "detecting information about x" are simply two ways of referring to the same, single state of affairs. The identity implies that perception is resonance-like and, thereby, a matter of laws and principles. The identity also implies (as Shaw and colleagues would eventually argue in 1979 and 1982) that perception is a fact of existence: It is necessarily what

it is and not something that can be either right or wrong. A primary implication of the latter is that whatever success is achieved by the epistemic functions of organism-environment systems, it is achieved on the basis of satisfying existential criteria, not logical criteria.

All classical definitions and explanations of perception are shaped by the belief that light to the eye, sound to the ear, and so on are nonspecific (impoverished, ambiguous) in respect to the environmental states of affairs responsible for them. Accordingly, perception must be *indirect*. Alhazen in the 10th century and Helmholtz in the 19th century expressed the nature of perceiving as follows: Given a proximal stimulus (e.g., retinal image, sensations), one must both ask and answer (albeit unconsciously) "what distal stimulus would normally have produced it?"

The implied central role of inference, common to almost all past and present formulations of perception, is not assumed by the familiar modes of induction and/or deduction but, rather, by the mode of "abduction", as Charles Peirce (see Harris and Hoover 1983) chose to name it: an inference from observation to explaining hypothesis. Perception understood as indirect is the (unconscious) making of inferences from effect to cause. Unconscious inference is paradoxical. It presumes knowing (a) the causes (having mental representations of them) and (b) the relations between effects and their causes, both of which can only be acquired on the basis of unconscious inferences. As an important aside, indirectness marks the Gestalt alternative to Alhazen and Helmholtz despite its dismissal of sense data and inference. To paraphrase Koffka, the world does not look as it does because the conditions of stimulation are what they are but because the brain states are what they are. Solipsism is (at the very least) equally as unsatisfying as the paradox of unconscious inference.

Expanding upon the question above of "what was at issue?" if perception is to be understood in terms of laws and principles, then what of action? In the mid-1970s we studied action as a separate enterprise, with Bernstein's ideas as the focus. In the latter part of the 1970s the action question became more pressing to my colleagues and me as the limitations of the major approaches to the coordination and control of movement (those deriving from cybernetics, neurophysiology, information processing, and artificial intelligence) became more apparent. There was considerable intelligence borrowing conducted (a) from an *a priori* stance toward the orderliness of movement (the prescribing of causally involved architectures and algorithms), and (b) coordinate with a *sui generis* attitude to individual action phenomena (treating them as unique and not explainable through general principles).

At some juncture we realized that, for a fully consistent ecological theory of perception-action, addressing the problems of coordination and control required the kind of generality typically associated with physics. But what kind of physics might that be? It certainly could not be Newton's, the physics of machines, but it could be that which Kant (1790/2000) saw expressed in organisms, a physics of self-organization involving "nothing analogous to any causality we know" (Section 69: 279).

Two developments of the 1970s helped our quest—the awarding of the Nobel Prize in Chemistry to Ilya Prigogine for his work on nonequilibrium thermodynamics, and the rapidly developing mathematics of nonlinear, dynamical systems. Prigogine's physics underscored that both biological and nonbiological order of varied degrees of complexity are *a posteriori* facts, the lawful consequences of irreversible (dissipative) processes. The developing mathematics highlighted the evolution of stable, unstable, and metastable states shaping the trajectories of systems of high dimensionality, an evolution that followed from changes in one or a few control variables. A third development should not go unstated, a fortuitous link between ecological psychologists at the University of Connecticut and the founders of homeokinetic physics (a physics for all systems) authored by Iberall, Soodak, and Yates. By 1980 we had made sufficient progress for Peter Kugler, Scott Kelso, and me to publish seminal papers with the title *On the concept of coordinative structures as dissipative structures*.

The experimental base for the early conceptions and their evaluation has been amplified considerably in the intervening 30+ years. Perception experiments have addressed the grounding of perception in ecological optics, acoustics, and mechanics (the patterns of mechanical forces that support the multiple achievements of haptic perception). The action experiments have addressed the grounding of rhythmic limb movements, postural organization, and inter-person coordination in the principles of self-organizing systems. Underpinning the experiments in both perception and action were new procedures and analytic methods, either adopted from or based upon advances in the burgeoning physics and mathematics of complexity. Many were summarized in Warren's2006 *Psychological Review* paper on *The dynamics of perception and action*.

In order to address the next part of the Editor's charge, the transition to 2012, I need to highlight two additional publications. The book that Peter Kugler and I published in 1987 on *Information, natural law and the self-assembly of rhythmic movement* provided a primary theoretical backdrop, what might be termed a strategic physics: a universal set of organizing physical strategies, most particularly thermodynamic, that apply with equal emphasis across the various scales and disciplines of the natural sciences. The motivation was Gibson's information and Iberall's homeokinetics (Iberall and Soodak 1987). The larger purpose, one might say, was dissolving the dualism of animate and inanimate—bringing both under the purview of law in equal degree. It could be viewed as a new kind of reductionism, a strategic reductionism (to common physical strategies) instead of a morphological reductionism (to common material properties). (An immediate benefit was its use as a springboard for the conception of *Ecological mechanics: A physical geometry for intentional constraints* published by Robert Shaw and his son in 1987.)

This theorizing was taken a step further in the 1991 publication with Rod Swenson on *Thermodynamic reasons for perception-action cycles*. An argument for a direct and deep connection of perception-action to thermodynamic principles was built on the cornerstones of (1) maximum entropy production, (2) inexorability of order production (because order produces entropy faster than disorder), (3) evolution as a global phenomenon (the system "Earth" at its highest level evolves as a single global entity),

and (4) Gibson information. Perception-action cycles arise from the opportunistic coordination of (4) with self-organizing dynamics. Their significance is amplifying opportunities to produce ordered flow and consequent dissipation of potentials at a faster rate. The argument itself was that the progressive emergence of perception-action cycles (the nonergodicity of species) in Earth's evolution is a lawful consequence of opportunistic physics. It was (and is) how (1) is satisfied. In a 1995 publication, *Toward an ecological physics and a physical psychology,* my colleague Shaw and I suggested that the metaphysical hypothesis of organism-environment dualism that has tended to dominate psychological theory (implicitly or explicitly) can be, and should be, replaced by the scientific fact of organism-environment mutuality and reciprocity. This latter scientific fact nests Gibson's affordance.

So, now, how has my approach to action and perception changed between the early foray in 1974-1980 and 2012, with the latter captured in the chapter *Ecological perspective on perception-action: What kind of science does it entail?* and its companion piece (written with my wife, Claudia Carello) *On intelligence from first principles: Guidelines for inquiry into the hypothesis of physical intelligence (PI)*?

Ideally, given my remarks above, it should come as no surprise that ecological psychology can be considered as a psychology for all organisms, the 96 phyla that comprise the Five Kingdoms—Bacteria, Protoctista, Animalia, Fungi, and Plantae (Margulis and Schwartz 1982/1998). It can be considered as a psychology that aims to understand how all organisms "make their way in the world" (see Reed 1996)—how they perceive and act. It should also be evident that ecological psychology, at least as interpreted by my closest colleagues and me, pursues the desired understanding in terms of identifying conceptions, theory and methods up to the charge of delivering a law-based account of the phenomena characteristic of nature's ecological or mesoscopic scale. In this regard, note that the expansion (signaled by the colon) of *Ecological perspective on perception-action* is *What kind of science does it entail?*

The science currently in force in the study of perception-action, and cognition in general, focuses primarily on Animalia in the phylum Craniata, and on explanation derivative of nervous-system properties and expressed in a language of artifacts that compute (in language-like symbols, or neural-like states). While all members of Animalia other than sponges are endowed with nervous systems, the size of the endowment is not a straightforward index of perception-action competence (see McCrone's 2006 appraisal of the jumping spider *Portia labiata*). That nervous systems are absent in the four other kingdoms means that the vast majority of perceiving-acting systems lie outside the explanatory scope of a science that gives primacy to the nervous system.

In reviewing the lineage of *Ecological perspective on perception-action* (scheduled for a 2013 publication) I have presented both explicit examples and subtle clues as to the look of the entailed science. Here, I add (with minimal but ideally sufficient detail) two further examples, that of affordance and that of prospectivity.

A primary desideratum is ecological ontology, organism-specific descriptions of the surrounding surfaces, substances, and media that clarify how any given habitat (*where* an organism lives) is partitioned into distinct niches (*how* an organism lives). The ecological furnishings, Gibson suggested, are affordances. In respect to all five kingdoms, an affordance is an invariant combination of properties of surface and substance taken with reference to an organism and specific to an action performable by the organism. The niches of organisms comprise possibilities for action, and are perceived as such .As an organism moves (like an animal, or a bacterium), or grows (like a plant), or ramifies (like a fungus), or spreads (like a mold), relative to its surroundings, some action possibilities persist, some newly arise, and some dissolve, even though the surroundings, analyzed classically as objects in Euclidean relationships, are unchanging. Gibson in his 1979 book summarized an affordance thusly: It exists whether or not it is perceived or realized, it cuts across the subjective-objective dichotomy, and it is equally a fact of environment and behavior. What kind of science does affordance entail? The answer, I suggest (and present in *Ecological perspective on perception-action*), is a science of objective relational properties that includes among its fundamental notions *compatibility* (in the quantum sense of other relations remaining potential when one is actualized) and *impredicativity* (defining properties in terms of the system they constitute). In several publications, Anthony Chemero and I have explored the relevance to ecological psychology of non-well-founded set theory and the impredicative definitions that it supports (e.g., in *Biological Theory* 2007).

Eleanor Gibson (1994; see also Reed 1996) singled out *agency* as the core phenomenon to be explained by psychology. Its three defining properties are prospectivity, retrospectivity, and flexibility. In approximate terms, prospectivity and retrospectivity are the abilities to coordinate current behavior with emerging and prior states of affairs, respectively. In similarly approximate terms, flexibility is the ability to vary the means to achieve an end. Agency, I would argue, is characteristic of all phyla to greater or lesser degree. If such is the case, then each of the defining properties must be based in a generic principle. For example, rather than asking how the future is produced from an internal model, one should ask about the coupling (between organism and environment) that results in coordination with the future.

Prospectivity relying on systemic lawfulness can be termed *strong anticipation*, following a suggestion by Dubois in 2001. Voss (2000) has identified a general framework for the anticipation of a “master” system (e.g., light-dark cycle) by a “slave” system (e.g., organism) with delays, namely, $dx/dt = g(x)$, $dy/dt = f(y) + k(x - y_\tau)$. The term $y_\tau$ identifies a past state of $y$ delayed by $\tau$. The effect of the coupling term $k(x - y_\tau)$ is to minimize the difference between the state of $x$ at the current time, and the state of $y$ at a past time. If this difference is successfully minimized, then the difference between the present state of $y$ and the future state of $x$ is also minimized. The effect of this minimization is the synchronization of $y$ with the future of $x$ (for physical and biological examples see papers by Nigel Stepp and colleagues 2010, 2011). The basic coupling dynamics can be extended in two ways: by including multiple $x$ values delayed relative to a given $y$ value, or by including multiple $y$ values relative to which a given $x$ value is delayed. As suggested by Stepp in his 2012 dissertation, there may be a universal equation encompassing all variants of strong anticipation—all variants of lawful prospectivity.

The capstone of the line of inquiry from the 1974 paper (which first paired the terms perception and action) to the present is the focusing of my efforts and those of several of my colleagues on the so-called hypothesis of physical intelligence (PI), alias intelligence-from-first-principles. The overarching concern of avoiding loans of intelligence, or "self-actional explanatory terms," as Dewey and Bentley would have said in 1948, puts a premium on understanding the ill-defined but intuitive notion of intelligence through the strategies that collectively define the ecological approach to perception-action. In a paper in the first of several planned special issues on PI in *Ecological Psychology*, Claudia Carello and I identified 24 guidelines for seeking intelligence from first principles.

## The Present and Future of Ecological Psychology

What is the current status of ecological psychology?
What dangers or misunderstandings do you see?
What will be the role of ecological approach in the future?

The final chapter of Ed Reed's portrayal of the life and science of James Gibson summarizes the status of ecological psychology within the field *circa* 1988. I would say that much has remained the same since that summary. The mentalistic and mechanical models that we associate with Descartes and Helmholtz and Sherrington continue to dominate, buttressed by the versatile current instantiations of Turing machines and Turing's mechanization of mathematics and thinking. The contemporary satisfaction obtained from tying hypothesized mental functions to anatomical networks revealed by fMRI and other modern technologies is creating a deepening sense of comfort with the theoretical status quo. From the latter perspective, the critical reexamination of the base concepts demanded by ecological psychology is seen as largely unnecessary and irrelevant—as just so much heterodoxy.

Also conforming to Reed's 1988 summary is the continuing tendency for select theoretical and methodological advances within the ecological perspective on perception-action to be incorporated into the old language of standard theories of sensory processes and motor control. Especially bothersome is the co-opting of terms (e.g., affordance and optic flow) for uses in cognitive science, human factors, and education that are conceptually at some remove from their definitions and usage in ecological psychology.

What makes the ecological approach to perception-action challenging is that it requires honest recognition of the obvious: Physics is not done yet! In our 1995 paper *Toward an ecological physics and a physical psychology,* Shaw and I set the stage with the statement (inspired by Robert Rosen, 1991) that "Material systems that express 'knowing about' are more general in respect to the principles that underlie them than the material systems that physics currently addresses." The sections of this paper are (i) organism-environment mutuality and reciprocity, (ii) toward a functional semantics, (iii) controlled locomotion as the paradigmatic form of "knowing about," (iv) physicalizing and intentionalizing information, (v) intentional behavior as a symmetry of the ecological scale, and (vi) direct perception: symmetry again. Only by recognizing

and addressing the incompleteness of physics can we hope to reverse the historical tendency of treating perceiving, acting, and knowing as necessitating special explanation outside the purview of universal physical principles.

For an appreciation of my expectation for the future role of ecological psychology I recommend Turvey and Shaw (1995) and suggest special attention to their inserts entitled "Psychology on the cusps between the past, present and next centuries" and "Direct perception."

## On Human Nature

Does human nature pose an important challenge? Is there any mystery to it?
Are these questions too trivial in the 21st century?

My answer to this question paraphrases Reed's *Conclusion* to his 1996 book.

Ecological psychology is the study of how organisms encounter their world (precisely, their habitat—where they live, and their niche—how they live). Ecological psychology has plenty of room for appreciating the specialness of human life but as a scientific stance it does not hold human life separate from the rest of the planet's life forms nor the encounters of other life forms less real than those of humans. The specialness of human life is the richness and non-ergodic nature of human encounters (occupying niches that did not exist previously) necessitated by the great and volatile diversity of human surroundings. Its study is far from trivial, and we should expect it to place significant demands on scientific inquiry in the 21st century.

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# Selected literature on affordances and ecological psychology

***Ecological Psychology***

The most recognizable journal devoted to ecological psychology. It focuses on many problems and issues that are related to animal-environment systems. The journal presents such diverse areas as human experimental psychology, developmental/social psychology, animal behavior, human factors, fine arts, communication, computer science, philosophy, physical education and therapy, speech and hearing, vision research. Frequency of the journal is four issues every year. Publischer: Taylor & Francis.

The journal's Main Editor is William M. Mace: Professor of Psychology at Trinity College (Trinity College, Hartford, Connecticut, USA) and Executive Director of International Society for Ecological Psychology
(*http://caribou.cc.trincoll.edu/depts_ecopsyc/isep/)*

The journal's website:
*http://www.tandf.co.uk/journals/titles/10407413.asp*

Classic works by James Jerome Gibson:

**J.J Gibson. 1977. The Theory of Affordances. Eds. Robert Shaw and John Bransford. *Perceiving, acting, and knowing: toward an ecological psychology*. Hillsdale: Lawrence Erlbaum Associates. 67-82.**

In this article Gibson for the first time introduced the term "affordances".

**J.J. Gibson. 1979. *The Ecological Approach to Visual Perception*. Boston: Houghton Mifflin.**

One of the most important books about ecological psychology and affordances. It covers such topics like: complementarity of animal and environment, theory of affordances, theory of direct perception and ecological psychology. It is both: a new proposal of theory of perception and as well as critique of classic ones (like behaviourism, cognitivism). Obligatory position for everyone interested in the subject.

Other works:

**A. Chemero. 2003. An Outline of a Theory of Affordances. *Ecological Psychology*, 15(2): 181-195.**

Comprehensive overview on theory of affordances. Author describes the relations: (a) between the abilities of animals and features of the environment (b) and among affordances and niches, perceivers, and events.

**W.W. Gaver. 1993. What in the World Do We Hear?: An Ecological Approach to Auditory Event Perception. *Ecological Psychology*, 5(1): 1-29.**

Gaver takes an ecological approach to everyday listening. Everyday listening is the experience of hearing events in the world. Author develops a new framework for describing sound in terms of audible source attributes.

**E.J. Gibson & A.D. Pick. 2000. *Perceptual learning and development: An ecological approach to perceptual learning and development*. Oxford: Oxford University Press.**

It is a unique theoretical framework for the ecological approach to understanding perceptual learning and development. This book covers the development of perception in detail from birth through toddlerhood, beginning with the development of communication, going on to perceiving and acting on objects, and then to locomotion.

**H. Heft. 2001. *Ecological Psychology in Context: James Gibson, Roger Barker, and the Legacy of William James*. Mahwah: Lawrence Erlbaum Associates.**

The book is a contribution to both the history of psychology and the development of the ecological method. Heft presents many different topics from philosophy and psychology and shows connections between both.

**D. Norman. 1988. *The Psychology of Everyday Things*. New York: Basic Books.**

Author of this book studies affordances in the context of human–machine interaction. It covers large amount of topics including HCI, design practice, ergonomics and psychology.

**M.T. Turvey. 1992. Affordances and Prospective Control: An Outline of the Ontology. *Ecological Psychology*, 4(3): 173-187.**

This article is about actions and ontology. Author argue that research in the ecological approach to prospective control is ultimately the search for objective laws. Moreover affordances and the promoted ontology is materialist and dynamicist.

**W.H. Warren. 1984. Perceiving affordances: Visual guidance of stair climbing. *Journal of Experimental Psychology: Human Perception & Performance*, 10, 683-703.**

One of the most important experimental research about affordances. It is an analysis of affordances in terms of the dynamics of an animal-environment system. Warren explores relation between environmental properties and properties of animal by introducing critical points (phase transition to a new action), optimal points and more. Case study of a research is stair climbing ability.

prepared by Dawid P. Lubiszewski

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# EMBODIED INTERNIFUS

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# Sense-making with a little help from my friends:
## Introducing Ezequiel Di Paolo and Hanne De Jaegher

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**Abstract**

The work of Ezequiel Di Paolo and Hanne De Jaegher has helped to transform the enactive approach from relative obscurity into a hotly debated contender for the future science of social cognition and cognitive science more generally. In this short introduction I situate their contributions in what I see as important aspects of the bigger picture that is motivating and inspiring them as well as the rest of this young community. In particular, I sketch some of the social issues that go beyond mere academic debate, including how the methods and assumptions that inform orthodox cognitive science are intrinsically related to the critical state of affairs in our world today. I conclude with some personal recollections in order to give an idea of the context in which their ideas, and mine as well, came to fruition.

**Keywords:** enactive approach; cognitive science; social cognition; theory of mind.

I am glad that *Avant* has provided a platform for Ezequiel Di Paolo and Hanne De Jaegher, a pair of avant-garde scientists and philosophers, to air their views in the open format of an interview. Their articles are already consistently pushing the boundaries of accepted conventional wisdom within the confines of the rigid etiquettes, rules and regulations of academic publishing. But in this interview we are given the rare opportunity to hear from them more directly and personally, and we get a sense that their published output so far is only the visible tip of a deep iceberg that, like life itself, still remains to be more fully explored. There are surely still many additional ideas and surprises to be revealed that will help us to finally overturn the doomed neuro-computationalist dogma. And the sooner, the better. The most celebrated insights of current scientific thinking about what we human beings essentially are, namely, that we are nothing but selfish, genetically pre-programmed zombies, passive robots, and/or disembodied brain-minds, have already been causing damage for far too long. It's time to stop that grotesque and inhuman masquerade! There are more convincing scientific alternatives.

But the work of Ezequiel and Hanne is not just about unmasking the hubris of computationalism. Clearly, there is more at stake in their work than contesting the next big ideas in science and philosophy. And here is why: It simply cannot be denied that the subject of cognitive science (i.e. the scientific observer, usually a psychologist) and the object of cognitive science (the so-called 'subject' or, to use the politically correct term, the 'participant', who is usually a psychologist, too) are one and the same: *a human being*. Accordingly, scientific claims about life, mind and sociality cannot be divorced from how we find ourselves to be right now, and how we wish ourselves to be in the future. The human condition is not a static given; it is an open-ended process of becoming that we shape and enact with our choices and actions.

These are, therefore, not merely academic issues confined to the ivory tower; they have direct implications for politics, for personal responsibility, and for how we can and should live our existences. Despite what orthodox science dictates, the personal and professional aspect of our lives cannot be lived independently from each other. The standard practice of ignoring the evidence of one's own first-person perspective, especially when the very topic of one's professional study is the personal-level of human existence, is irresponsible and ethically indefensible. Why should our findings be considered more objective when we aim to exclude the only genuine access to the personal-level that we have, namely our own lived existence as human beings? We all know from our own personal lives that there is more to people than what is revealed by recordings of internal physiological data and measurements of external movement patterns. And given the polarity of current public debate, it needs to be pointed out that this rejection of scientific reductionist approaches to our own first-person perspective does not entail a commitment to some kind of religious totalitarianism. Whatever happened to simply acknowledging our personal existence in the here and now?

Consequently, if we want to reject scientific and religious dogma, it is also up to everyone to show in his or her own life that the mainstream theories are wrong. For example, as Ezequiel correctly points out, the individualist-computationalist paradigm is only more or less valid as long asmost of us choose to continue the social game of being 'perfect consumers' of preformed products and information. Every time we behave like a mere reactive robot, we give tacit support to the computational theory of mind, which, as the dominant paradigm in science, in turn influences the way in which we think it is possible to behave. And to some extent this unsatisfactory state of affairs is methodologically enforced in the lab. As every psychologist knows very well, those 'participants' who do not follow the given instructions of the experiment are excluded from the results and do not appear in the final analysis. The unpredictability and uncontrollability of genuine human autonomy are excluded by society and science as madness and noise, respectively. Functionalism selectively filters the facts of our existence. What we need instead is a practice and theory of mind that takes the open-endedness of human autonomy as its starting point, and as its ultimate point of return.

But the issues go even deeper than that. Because even if we happened to believe what mainstream science tries to convince us of, namely that we are isolated and independent brain-minds, we can only make sense of this belief in the context of a shared

world. Our living body, other persons and the environment are always already present before we choose to ignore them. This blindness to its own range of dependencies, whether they are biological, social, or ecological, is what makes the modernist episteme so toxic and deadly to a human future on this planet, like a cancer that is unwittingly committing suicide by blindly consuming its host organism. However, this failure of methodological individualism is not meant to imply a return to some form of totalitarian socialism. Ezequiel and Hanne are careful to emphasize that they do not want to reduce the individual to the social, or vice versa. Their stated aim is to move beyond that kind of linear-reductionist way of thinking altogether. Indeed, to think that the negation of one position must necessarily entail acceptance of its logical opposite is precisely to remain stuck in a linear mode of thinking. Why not try to change the terms of the debate altogether? Accordingly, one of the main tasks for the enactive approach is to create new conceptual tools for better grasping the complex interdependencies between life, mind and sociality, including our own intimate personal experience as well as genuine human autonomy. From this alternative perspective we can also understand why Ezequiel and Hanne's preferred point of departure is to explore this complex network of interdependent processes by focusing on the level of autonomous dynamics emerging out of social interaction between two or more people. "Participatory sense-making" offers a middle way, an intermediate level of analysis, between the two extremes of individualism and socialism.

Let me conclude this introduction with some personal recollections. I first came across initial formulations of this enactive approach by reading Ezequiel's papers on agent-based models of communication and social interaction, while I was still a student in the Department of Cybernetics at the University of Reading. It was that work which convinced me that I should do my PhD at the University of Sussex, where Ezequiel was lecturing at the time. When I arrived there at the end of 2004, I was fortunate to end up in the same research center as Hanne, who was just finishing her dissertation on autism and participatory sense-making. I still remember my 6 years at Sussex fondly. There were many creative people with various kinds of backgrounds and interests interacting in a mutually inspiring manner, and lots of free-spirited ideas were floating around the lab. Some of these were discussed more publicly in the Life and Mind seminars and can still be found online at our blog[47]. In the following interview Hanne nicely describes this kind of style of research as a "horizontal" interaction, rather than the usual dominance hierarchies that stand in the way of genuine collaboration. Horizontal interaction enables collective emergence of creativity.

In this collaborative spirit Ezequiel agreed to become my doctoral supervisor, and this turned out to be a highly productive relationship. We have been working on many projects together to push the enactive approach forward. In particular, we made a series of agent-based models, which demonstrated the enabling and constraining effects of social interaction dynamics on the behavior of individual agents. In 2008, together with Hanne, and Steve Torrance, we organized a workshop with the theme of participatory sense-making, which turned out to be a great participatory success. But even before then I was convinced that Ezequiel and Hanne's "Participatory sense-

[47] *http://lifeandmind.wordpress.com*

making" paper had hit the jackpot. I remember philosophizing with Ezequiel at one of the many get-togethers in a small bar in Brighton. I had just finished reading a draft of their paper, and I said that they had a citation classic on their hands. Ezequiel was also optimistic, but observed that the jury was still out. Now, 5 years later, and Ezequiel and Hanne's modesty notwithstanding, that particular paper has turned into one of the most hotly debated articles of the enactive approach. Moreover, that paper has managed to achieve what most other contributions to this approach have so far failed to accomplish: it has started a mutual dialog with mainstream researchers of social cognition. Now that we have this small opening of attention, it is up to enactivists everywhere to keep up the good work and to make sure that we live up to our own expectations, both personally and professionally.

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# Sense-making with Ezequiel Di Paolo[1,2*] and Hanne De Jaegher[2]

## Interview

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**A highly philosophical question: what do you think - is society a higher, better organized 'tissue' of life than a single human being?**

*Ezequiel*: What exactly is a *single* human being? Does not the idea of a person already imply belonging to a social "tissue"? We live under the contemporary mass illusion that individual people are finished wholes to themselves, that we are islands and that at most we build bridges with a few others – family, friends, colleagues – while we forget that the patterns of our thinking, our style of walking, of breathing, our physiology, our motivations, our speech and gestures are all braided from the readily available threads of our socio-cultural embedding. Understanding this mass illusion has consequences of the utmost urgency today. Not only do we miss a crucially central level of enquiry if we focus solely on the individual agent in neuroscience, psychology, psychiatry and philosophy of mind, not only do we prevent ourselves from asking the right questions about what makes a cognitive agent an individual in the first place if we miss the socio-cultural embedding as a source of active, systematic mind-shaping, but we also fail to understand values of community, solidarity, trust, networking and we fail to give our lifework to a project larger than our individual selves. These values are not the remote ideals accessible only to selfless fictional heroes; they are in the very fabric of the processes that make human beings what they are; they are at the root of us all. At some point, for diverse reasons, we have been drilled into forgetting these origins to better fit a model of selfish freedom without commitment to others, which is manifested largely in the apathetic behaviour of the perfect consumer that only wants from society the goods it has been told to crave.

This is the context that as researchers we are often disciplined to ignore. Cultural isolation operates strongly by reinforcing the separation of key conceptual elements and by stifling the development of modes of thought that we so badly require (e.g. social scientists see neuroscientists as naïve reductionists and neuroscientists see social science as so much babbling and empirically void).

Sometimes the ontological implication (our minds are social in essence) is not denied, and adopting an individualist attitude is defended as a reasonable methodology: “We must start somewhere in our enquiries and we choose the individual as a locus of explanation until we exhaust this possibility and then we shall, in the future, consider a more complex dimension involving social interaction and other socio-cultural processes”. Our work on *Participatory Sense-Making* and the *Interactive Brain Hypothesis* shows that such an attitude is methodologically misconceived. It often leads to easily dissolvable problems once they are recast in their proper social dimensions. Unnecessary effort is invested in non-issues (e.g., “How do I manage to simultaneously hold two potentially conflicting representations, my belief and yours, in my head?”). And holding fast to methodological individualism soon leads to “epi-cyclical” theorizing, abandoning Occam’s razor and scientific common sense. As we are showing in our work, the dynamics of social interactions often provide much simpler and straightforward explanations of some socio-cognitive performance if we let go of the archaic idea that everything that happens in my cognitive world must always happen first in my head.

picture source: H. De Jaegher's archives

Coming back to the ontological aspects of your question, there is indeed something unresolved in the relative autonomies of the domains of the individual mind and socio-cultural processes and their contradictions. We do believe that neither domain is fully reducible to the other. But we also believe that this does not imply their full independence, as we have shown pragmatically, but their complex inter-penetration. What does this tell us about the nature of concepts such as individuality, interaction, society, normativity, etc.? At least this: that we are dealing with phenomena that admit multiple forms of enquiry, none of which is explanatorily sufficient on its own. Those who really want to understand these phenomena must get used to thinking with the tools of more than one discipline and in collaboration. There's no other way.

*Hanne*: I agree. I think that both society and human beings (and other animals or living beings) are organized at the level that is appropriate for them and for the environment they are situated in. The organization of societies and of living beings develops in conjunction.

**There often arises a dilemma in social cognition concerning which came first: an individual or a group. What is your view on this issue?**

*H*: I think the "which came first" question is misleading. We tend to think that things are linear, but, even though it's difficult, it may be more interesting to understand them in a circular fashion. The notion of an individual does not make sense without that of a group, and vice versa. Why is the question of which came first so compelling (and is it really?)? What do we expect from an answer to this question? I'd be more interested to think about the links between an individual and a group, and I don't just mean the positive links, also the negative ones, the tensions. If you think in linear terms about these matters, it's hard to see the dynamics that give rise to growth, to development, to change.

*E*: This is an age-old dilemma. I also don't think it should be put in terms of what came first, but in terms of what is the right level of analysis given a particular set of assumptions and a particular problem (a lot of trouble is created simply by trying to answer such questions with absolute generality, but in each particular case there are often clear indicators of what counts as important and shouldn't be assumed away). Even in such terms the question remains paradoxical because we know that society, the group, is the braided pattern and material traces of the activity of individuals, and at the same time, human individual autonomy is the outcome of a social process of struggle for mutual recognition. So where to start? This is a problem only for the 19$^{th}$ century model of scientific thinking that attempts to explain everything linearly, billiard-ball style. I concur with Hanne on this point. Non-linear, dynamical ideas are the only vehicle for any real advance in the sciences of the mind, which are regrettably still dominated by linear thinking. From this perspective the back and forth epistemological shifts between parts and whole become the common currency of enactive thinking. We see this in the example of life, in our attempts to understand the dialectics between genetic regulatory networks and cellular dynamics, between individual cells and the extra-cellular matrix, between single neurons and synchronized ensembles, between

nervous system and sensorimotor coupling, and between individual affects, actions, perceptions and intentions, and social interaction. The same pattern repeats itself. We are always invited to bring two apparently incompatible perspectives into a mutual dialogue, which is never simply a dialogue about what comes first, what causes what. We want to understand the various relations of mutual constraining and relative independence of the domains we study. This is what we mean when we say that enactivism has a *parallax epistemology*, we constantly shift our vantage point and gain knowledge from the movement. But this is simply another way of saying that enactivism is inherently dialectical.

picture source: E. Di Paolo's archives

**What problems do you encounter in understanding social enactivism?**

*Both*: In general, we have found that the approach is enthusiastically received. Of course, this doesn't mean that people readily agree with the enactive take on social understanding. There are genuine unresolved questions and challenges within the approach and ongoing debates with more traditional perspectives. Sometimes, though, problems also arise out of misunderstandings. Enactive ideas are based on a specific technical vocabulary (autonomy, sense-making, and so on) and these terms are not at first easily differentiated from more common uses in philosophy and cognitive science, or indeed in everyday language. This can be a source of initial misunderstanding but it is solvable with ongoing dialogue. Because we offer a perspective that contrasts sharply with the representationalist approach (theory of mind, simulation theory, etc.), sometimes our message is interpreted in a more extreme sense than we intend (for instance, when we are called 'interactionists'). This leads to misconceptions like

that we don't take seriously the individual or sub-personal levels that affect social understanding, or that we care only about basic forms of interactive phenomena like body synchronization, but not about the so-called higher level social understanding (such as thinking about the reasons for someone's actions). This is definitely not the case.

Another possible source of misinterpretation is that we focus on a broader conception of social understanding than just mental events about other people. We include in our conception phenomena such as trust, coordinated action, negotiations, conflict, relationships, love, domination, mutual recognition, social roles, social norms, and so on. Our approach is more generally concerned with *intersubjectivity*, by which we mean the meaningful engagement between subjects. These three terms: meaning, engagement, and subject are grounded in enactive theory rather than assumed as unproblematically given. Social understanding in this sense includes social action, skilful social engagements, interaction practices as well as – sometimes – thinking about the intentions of others.

Assuming that explaining this gamut of phenomena is somehow dependent on first explaining how we figure out the intentions of others is a widespread intuition. We question this intuition.

**'Neurophenomenology: a Methodological Remedy for the Hard Problem' by Varela was a kind of a manifesto of the emerging neurophenomenology. 'A sensorimotor account of vision and visual consciousness' by O'Regan and Noë was an analogical text for the sensorimotor approach. Can your work 'Participatory Sense-making. An enactive approach to social cognition ' be treated in a similar manner?**

*H*: Phew, this I find very difficult to answer. History will tell, won't it? I at least see that paper as a basic one in my own work, I think we laid down some basics, which at the same time already seem to contain many seeds of what has already come and is still to come after. People also seem to have found it interesting and inspirational so far, so who knows? I'm in any case curious to see what will happen further with the ideas.

*E*: This is a generous comparison and of course it is not up to us to respond to this question. We are happy that our text managed to articulate a tension that was "in the air" in many domains. Many of the ideas and motivations of this text are hardly new. What's novel is the bringing together of some elements of enactive theory (autonomy, sense-making) to specifically clarify intuitive notions such as that of a social interaction (surprisingly, previous to our work we have found no clear, non-circular scientific definition of the term) or what it means to participate in each other's sense-making. The resonances we have encountered in various disciplines (psychiatry, psychopathology, different forms of movement therapy, conversation analysis, dialogical self theories, infant intersubjectivity, musical studies, ethics, interaction studies, robotics, etc.) speak as much of the *Zeitgeist* as of the text itself.

**'Participatory sense-making ...' was supposed to be the first attempt at an enactivist account of social cognition. Is this work sufficiently grounded in research?**

*Both*: Our work is primarily conceptual. It follows Deleuze's description of the work of the philosopher as an "inventor" of concepts. Concepts are tools. The test of the tool is whether it is useful. Participatory sense-making, as we said, has already been taken up as a concept and applied to the formulation of enactive hypotheses in developmental psychology, neuroscience, human-robot interactions, animal ethology, evolutionary robotics, ethics, psychiatry, literature studies, and other fields. People have used it to discuss and further embodied accounts of individual and political affect and part of our current work is looking at the relations of individual and social institutions using this concept. In our view, this is a good test of whether the concept is grounded in research: not merely whether its origins are solid, but whether it is useful.

As to the research strands that ground this concept, they're too numerous to mention: the enactive concepts of autonomy and sense-making, the dynamical systems concept of coordination, developmental evidence of various forms of co-regulation of movement and affect, phenomenological grounding of the experience of engaging in interaction, experimental and modeling evidence such as the double TV-monitor and perceptual crossing studies, evidence from gesture and conversation analysis, and from autism research. Not to mention a tradition of non-individualist thinking going back to Hegel, Dewey, Mead, Cooley, Vygotsky, Elias, Goffman, Mauss, Merleau-Ponty and Bourdieu.

**Have you identified any issues in cognitive science that cannot be explained outside the concept of participatory sense-making?**

*Both*: This is a bit of an odd question. We do not try to explain everything, just those things that involve a social element and these are likely to imply some element of participatory sense-making, either contemporaneously or developmentally. Having said that, we think human minds, human personhood and human autonomy are largely social, so PSM will probably have something to say about many aspects of the human mind, even some that do not immediately seem social, like the perception of abstract object shapes.

**How important is the idea of participatory sense-making in the research on autism?**

*H*: The currently dominant psychological explanations of autism (in the Anglo-Saxon research world, that is) are cognitivist in the main. Theory of mind theory, weak central coherence theory, and executive function seem to have carved up the problem space nicely, and each of them deals with an aspect of the disorder: Theory of mind with the problems of social interaction and communication, weak central coherence with people with autism's preference for piece-meal information processing, and their difficulties with seeing the context and the gist of an events or object, and executive function with repetitive behaviours and the strong need for sameness and structure.

These approaches have some internal problems, one of which is that they see cognition as a matter of information processing, and autism, consequently, as faulty information processing. This makes it inherently difficult to integrate the affective aspects of autism, like the anxiety that many people experience. Another problem is that there is no principled way in which to combine these different theories. They seem to be dealing with aspects that are not intrinsically related. What connection could there be between the social information processing problems and the need for a structured daily schedule, for instance? The big autism researchers, like Uta Frith, are aware of this problem, and have not proposed an adequate solution.

I think enaction – participatory sense-making – can offer something here. It would make sense-making the basis for an integrative explanation of autism. The main and starting question would be "what does this or that mean for the person with autism?" Take echolalic behaviours (this is repetitive speech, e.g. literally repeating what someone else has said, often many times), why does a child with autism do that? In most behavioural treatments, things like this are simply seen as unfitting behaviours, and often treated with the intention of getting rid of them, because they are socially inappropriate. They are seen negatively, as symptoms, and not as positive components of alternative forms of sense-making. This is a functionalist reasoning: the behaviour does not fit in the overall configuration – which is a non-autistic world. But if we look at these behaviours from the point of view of the person with autism, we can ask why they do it. Researchers have found that in the interactive context, echolalia can make sense as a way of expressing affect in response to the actions of others. Another example, when a person with autism looks at something while quickly moving his fingers in front of his eyes, it may be that this makes it easier to see or even look at the thing. This has also been researched, people with autism seem to have a different 'embodiment,' their ways of moving and perceiving are different. For instance, they have difficulty perceiving fast movement. Enaction connects this directly to their sense-making, and says that different saliencies exist in the world for people with autism. Thus, what we could call 'autistic embodiment' and 'autistic sense-making' are intrinsically intertwined.

**Your work fits into a larger contemporary trend of abandoning a passive-contemplative outlook on the nature of social cognition (which used to be focused on explaining "higher" cognitive activities, including mindreading) and attempting to change it into an approach that would be more participatory-interactive. Still, we would like to know your exact position regarding mindreading as such. Do you think that third-person mental state attribution is simply a much rarer phenomenon than most researchers used to think (e.g. it rarely happens during everyday interactions, but it is sometimes used when passively interpreting fictional narratives that resemble classical false beliefs tasks, for instance while watching Scorsese's "The Departed") or do you take a more radical position and claim that mindreading it is not a real phenomenon at all? If the former is true, then what is your preferred explanation of mindreading abilities? Would it be possible to provide such an explanation within your non-representational, inter-**

**active theoretical framework? Or do you think that knowing how individual, internal cognitive mechanisms operate would prove to be necessary in the case of mindreading?**

*E*: What is normally described as mindreading – cogitating about the mental states of others through the use of inferences or simulation – is something we indeed do occasionally. Our view is that these acts, however, are not the basis of social cognition, which is demonstrably the position that has dominated the field to date (see our *Interactive Brain Hypothesis* paper for further discussion). We also believe that such cognitive acts are likely rather late developments. And that they require more basic and direct forms of embodied social understanding to be in place both developmentally and contemporaneously. As Shaun Gallagher says, even young infants unable to pass false-belief tests have no trouble interacting with the experimenter and following their instructions. Social understanding is a set of embodied skills, like riding a bicycle, and we become proficient at skills through experience. In the case of social understanding skills, this experience is primarily interactive and participatory, and to a lesser degree observational. So essentially, putting mindreading first is very much analogous to the General Problem Solver strategy in AI in the 1970s. It's upside down. And such an assumption faces almost the exact same problems.

Several interesting and related questions arise if you consider the alternative picture we propose. For instance, we may ask in what situations does mindreading become a useful strategy? If you think it underlies all social understanding, you are blind to this question. How do we achieve this cognitive feat? If you assume that personal mindreading is supported by subpersonal "inferences" or "simulations" – whatever that means – then the resulting explanation has a very shallow gradient, like that of the effect of opium and its soporific properties. What other forms of social understanding are in place when we don't use mindreading? And how do they relate at the level of performance and at the level of mechanisms with mindreading? We believe these are genuine and interesting questions that we would tend to ignore were we to assume the priority of mindreading.

**What do you think constitutes the difference between human social cognition and the social cognition that other animals (such as birds or primates) are capable of? Do you think it is possible to completely account for this difference using your preferred enactive, interaction-centered approach?**

*Both*: We would prefer not to start from the differences but to explore the continuum first. Then within the continuum we can start finding, rather than imposing or assuming, the differences. They may be on a gradient or there may indeed be qualitative jumps between different social capacities. We do believe that in humans there are elements that are qualitatively quite different from those found in other animals and that must be explained. They will generally involve sophisticated capacities for reflexivity and for reflection, the influence of socio-cultural normativity and social institutions, and the development of different levels of human identity through mutual recognition.

**Recently, you have presented a hypothesis of 'interactive brain', joining the discussion on basic cognitive function of the nervous system. Do you see any possible relationship between your ideas and such concepts as (1) *brain free-energy principle* by Karl Friston and others or (2) *neural re-use* by Michael Anderson?**

*Both*: It's not impossible. What's different perhaps about our proposal is the shift in how we conceive of the brain in the first place. No longer as the seat of the mind, the controller of the body, the place where "everything happens", but as a mediator, a coordinator, playing the role that is not altogether different (conceptually) from the role played by muscles in skillful movement, such as a dance. Muscles enable performance and they are shaped by it in turn. Similarly for the brain, and there is a vast amount of evidence that this is the case. What has happened is that different theories of brain function have so far remained "within" the brain. They downplay the formative role of external whole-agent engagement with the world and with others to that of "context", i.e. something that at most stimulates brain activity, gets it started, and overall must be "understood" by it, and represented. They also downplay the role of the body not only in constraining and enabling cognition and affect but as that of being the very matrix that makes our relation to the world a significant one. Our living bodies are the very reason why we are concernful beings, the key definitional aspect of the mind according to enactivism.

Our interactive brain hypothesis simply applies these insights of enactive theory to the specific case of social neuroscience. We ask how we should re-conceive the brain once we accept that social engagements often enable and even constitute, along with other factors, socio-cognitive performance. It may be that some ideas in other theories of brain function are useful for answering this kind of question. But as theories of brain function, they are not quite enactive until the shift is made in the manner of conceiving the brain as an "organ of mediation" to use Thomas Fuchs' happy phrase.

**Sometimes you refer to the distinction between personal and sub-personal processes. However, isn't this division problematic for enactivism? Do you think enactivism is a concept which covers mostly the personal level?**

*E*: Not at all. As a way of thinking about cognition, enactivism has not abandoned the aims of cognitive science, viz. to explain what the mind is and how it works. Therefore, it needs to keep a fluid dialogue between subpersonal, personal and interpersonal levels of analysis. If anything, enactivism takes the meaning of *subpersonal* very seriously and enactive researchers are very careful not to smuggle personal level notions into subpersonal processes (unlike people who speak of representations, simulations, predictions, inferencing, etc, when they speak about the brain or about neurons). That's how seriously we take this distinction. It has been a problematic one for mainstream cognitive science, not for enactivism.

How this works in practice connects to our previous comment about the shifts required by non-linear thinking. Enactivism studies the mutual constraining and relative autonomy of the different "levels" of interest that have been identified to affect a

given phenomenon. In the case of cognition, emotion, action, etc. these involve physiological, neural, environmental, personal and interpersonal aspects as a rule.

**Is the term 'social interaction' not too general a concept? It seems that the notion of interaction involves a diversity of topics and aspects of social life. From passing each other in the corridor (which you mention) to intimate relationships between people representing different cultures, different social strata, different religions, or experiences and ways of development. Can the reference to 'social interaction' better explain many social properties, such as reputation?**

*H*: I understand what you mean, but I think this criticism is slightly unjustified. The notion of social interaction may indeed include diverse phenomena but it is strictly defined in our approach, and it is operational, unlike in traditional views. Because of this, it can tell us something about what is common to these phenomena. One of the functions of the definition is as a focusing tool, with which to look at the social domain. We use the idea of social interaction to investigate the social realm in a certain way, from a certain viewpoint. Social interaction processes play a basic role in how we understand each other, how we think, what we believe and how we develop. Integral to our work, though this is not often recognized, is a particular approach to individuals. Participatory sense-making is a whole package, it's not just a focus on interactions. Its assumptions concern the interaction, but also the individuals in their autonomy, and their context, indeed also their societal, cultural, historical, and biological context, and their experience, their phenomenology.

**Don't you think that enactivism requires further clarification and separation from the currents and positions with which it only has certain points in common? There are published anthologies, but no good, comprehensive overview of enactivism has been released so far (unless there is one we are unaware of?).**

*E*: Yes, perhaps. Enactivism, in its life-mind continuity, Varela-style is becoming a solid theoretical framework. Much work remains to be done, but different people are now engaged in furthering this work. In part, the lack of a single comprehensive exposition of this framework as a whole reflects the ongoing nature of this work. Concepts like the interactive brain hypothesis, or *readiness-to-interact*, and even other ideas like an enactive approach to co-speech gesture, to the human self and to locked-in syndrome, are very much still under development (part of this work is being undertaken by researchers of the Marie-Curie Initial Training Network *TESIS: Towards an Embodied Science of InterSubjectivity*). There are, however, very good expositions of central enactive ideas, like Evan Thompson's book *Mind in Life*, as well as original motivations and inspirations in the 1991 classic *The Embodied Mind*, by Varela, Thompson and Rosch. The 2010 MIT Press collection *Enaction* shows mainly some of the different disciplinary tributaries to the flow of enactive thinking. It has only become clear relatively recently (at least to us) that enactivism is a non-functionalist approach to the mind. The very questions we want to answer – What makes us agents? Why do we care about anything? What is autonomy? – are the questions that functionalism has failed

to answer. In order to work, functionalism (this includes newer versions like the extended mind), must assume that these questions do not require further explaining; these issues are given and axiomatic, or they are never properly explained and used like a blank check whose value is not hard cash in the bank, but the hope that you can re-use it without questions asked. Enactivism is asking the questions. These relatively recent clarifications about where we stand are still happening and this is one reason why there are no comprehensive single expositions of *all* of enactivism. In a way that's good. A textbook could have the effect of freezing this process. But in any case, expect something along these lines in the near future!

*H*: I think it'd be great if Ezequiel wrote a basic book on enaction! In our work, we are always very careful to clarify and separate enaction from loosely affiliated ideas that may at first sight have elements in common, but which would dilute the strongest enactive ideas if they were uncritically assimilated. I think that this work is very important indeed. What you do get, then, is that people will find the work too radical, and feel compelled to react to that. This does not often lead to interesting criticisms or discussions, because the reaction is to the felt radicalness, and can therefore be quite reactionary. It can also make people talk past each other, because they each want to convince the other that the very basics of their approach are wrong, when it doesn't happen often that people will change their basic ideas about something. Therefore, indeed, I think it would be great to have a kind of basic book on enaction, which lays bare and shows, utterly clearly, its roots, so that we can point to that when there are such misunderstandings again. The debates between representational and functionalist cognitive science on the one hand, and enaction on the other, are good to have, but they are not the ones I am most interested in. They can keep you from doing the advanced work on your terrain. As for books, of course there are some important ones out there – like Ezequiel mentioned, *The Embodied Mind*, Evan Thompson's *Mind in Life*, and the MIT Press *Enaction* book are standard works. But we could do with a state-of-the-art text that is very clear about the fundamentals too.

**How would you describe your cooperation with such experienced researchers as Shaun Gallagher, and with younger ones – for instance - with Tom Froese?**

*H*: What I find amazing is how much you can learn from other people. I am always amazed at other people's knowledge and styles. One thing I find myself very interested in these days is the methods of work that other people use. Then I don't mean just learning about phenomenology, or neuroscience, or sociology, I also mean their methods of studying, of reading, of writing, of presenting. I find that this is something hardly ever talked about, and I miss that. One can sometimes feel alone in figuring out how to do the day-to-day work. The only way to learn about this is by working closely with someone, actually seeing them at work on a daily basis. This happens too little, in my view. Though right now, in San Sebastián and in the *TESIS* network, we have a close-knit group of researchers, where we also seem to be able to talk about these aspects of the work.

What I also like about collaborating with many different people is that the interactions are always so different, and that they all somehow work, but that you need to do different work in each case to make it all run smoothly. I also love it when the collaborations do not just happen over email, but in actual, live, meetings. Then you so often see participatory sense-making happen! That excites me. The notion that you can hold on to an idea, take it to be yours and yours alone then so quickly fades away. And it's ok, because understanding something with someone else, in a conversation, in working it out together, can lead to so much more insight. It's also better for the development of knowledge and insight more generally. One difference that I think you'd hope for there to be between younger and older researchers is that the more senior ones might be more relaxed about their CV's, and can focus more on the ideas. But in having talked to some of the professors, I find that they also are still struggling for recognition. This I think is a pity. It's a pity for young people to have to be so career-driven, but when you notice that even the big professors seem to still need to do that... That gives little hope for ever focusing in a relaxed manner on the real, important stuff of research.

I also like learning about collaboration, and research culture. Recently I learnt a lot about this from psychologist Vasu Reddy and my visits to her department in Portsmouth. The openness of that place is amazing, and so inspiring. Everybody will talk to you when you're a new face, and people are often up for thinking about your ideas with you, maybe setting up a collaboration. I think this is the way to go. Many of the young people in enactive research think like this too, *horizontally*, in terms of collaboration and sharing, and this is good.

**Are you satisfied with the new generation of scholars interested in enactivist ideas?**

*E*: Very much so. It's just amazing to see so much energy and creativity. Much of this work is still largely unknown. Some of these young researchers are still preparing their work, finishing their PhDs, getting their first publications out. (We've already mentioned the ongoing *TESIS* network). Expect some very interesting surprises over the coming years. What I find most encouraging is how much the young enactivists are ready to embrace interdisciplinarity and to listen and learn from other ways of thinking. There isn't a single researcher that could be described as enactive that does not combine in their work at least two traditionally independent strands of learning: philosophy and neuroscience, biology and computer science, psychology and sociology, psychopathology and phenomenology, systems thinking and critical theory, etc. Not one.

*H*: Satisfied seems to me a word that sounds like the collection of new researchers is full already. I hope there are many more to come! I do love people's enthusiasm, and it is very nice to come together and do real, constructive work. By constructive I mean, work on a basis of shared ideas and vision, and within that shared framework, to work out concepts, methods, experiments, in a critical manner. Many of the people I know are very, very capable and enthusiastic. It's a pleasure to work with them.

**How do you spend your free time? Are you as sociable as your research idea?**

*H*: Haha, this is a funny question. I like it. I'm not sure the research is actually so very sociable. One thing that tends to be ignored when we stress the role of the interaction in intersubjectivity, is that the *subject* is very important in our work too, the individual. Both sides — interaction and individual — are autonomous in our approach, and are dependent on each other for their autonomous organization. In my free time, I like to be with people, for sure. Though living far from my family and many of my friends can make contact somewhat restricted. I don't see some of my best friends very often. This can be hard. I do think I depend a lot on deep connection with others for my well-being. But I also like, and need, to spend time alone. I spend a lot of time just thinking, I think. But maybe you're asking what activities I like to do? I love driving around the country, exploring it. Just recently, I saw some beautiful mountain tops and passes in the Pyrenees, not too far from here. I most love to do this together with someone. I also love dancing, swimming in the sea, and cycling. They give me a feeling of freedom. For me, living life and investigating it are not very far from each other. I like to be connected in general, and one thing that can really bring that about for me is art.

*E*: I'm pretty much of a hermit and slightly anti-social! I tend to cringe away from small-talk but very much value deep connections to others. I spend most of my time reading and trying to understand life. They say that sometimes we need perspective to truly understand something. I get it from contact with nature, the gaze of some animals and the way art can sometimes change you.

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# I told no one. Introduction

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Frederique de Vignemont's works can be divided (in a manner independent from thematic distinctions) into theoretical and experimental ones. On the one hand, she is the author of one of the more curious conceptualizations of the role of body in cognition (a subject much exploited experimentally, but neglected philosophically). On the other hand, she is the co-author of interesting experimental works, which frequently complement and support her inquiries.

She is a CNRS researcher at the Jean Nicod Institute (Paris). Main aim of her research is to provide a systematic account of bodily awareness. Her research on bodily awareness include the phenomenology of bodily awareness, body representations (body schema and body image), the body and the space and more. De Vignemont also wrote several experimental papers related to one of the most famous experiment in the field - the rubber hand illusion. Besides her scientific research interests are in the field of self-consciousness, social cognition and psychopathology.

Her studies are interesting not only due to their scientific value, but also because of their interdisciplinary nature. She has published about 50 papers. She has worked with many philosophers (Alvin Goldman, Pierre Jacob, Adrian Alsmith) and cognitive scientists (Marc Jennerod, Patrick Haggard, and many others). She is currently working on a monograph entitled *Mind the Gap*, and – as we learn from the interview published in the issue – has a certain penchant for reading novels.

In the interview with Frederique de Vignemont we focus on the author's two main research areas: body awareness and the cognition of others. The remarks regarding the book she is currently working on (*Mind the Gap*) constitute an introduction to these questions. We have enhanced the interview with questions about enactivism, interdisciplinarity of her research, as well as about the place of a philosopher in contemporary cognitive sciences.

Stanisław Lem in his book entitled *Highcastle* reminisces about his youthful years in Lwów. The author portrays almost fantastical experiences – which are, however, strikingly similar to the migraine-induced experiences of disturbances of the body image, as described by Lunn. Not only the following quote from Lem's book, but also *Roly Poly* (filmed by Andrzej Wajda), *Peace on Earth* or the author's other works may interestingly complement de Vignemont's last statement in the interview:

> *Sometimes, when I was sick, but also when I was perfectly healthy, I would have strange sensations called, as I learned thirty years later, disturbances of the body-image. I lay in bed with my hands on my chest, and suddenly they would start growing, while under their incredible mass I became smaller and smaller. This happened several times, and definitely when I was awake. My hands grew to mountainous size, the fingers turning into enormous closed arches in their monstrous elephantiasis. I was frightened, but only a little, it was so strange—and told no one.* [Stanisław Lem *Highcastle*: 42[48]]

[48] Lem S. 1997. *Highcastle*. translated by M. Kandel, Mariner Books.

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# How many bodies we can find in one mind… and the other stories

## Interview with Frederique de Vignemont[1]

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### THE BOOK

**We know that you are in the process of finishing your monograph. Let us then begin with the question: why "Mind the Body"?**

While more and more people make every possible use of the notion of embodiment within the growing Embodied Cognition research program, the body itself remains an object rarely investigated explicitly. The embodied approach claims to return the mind to the body. In this book, my objective is to return the body to the mind. The aim of my book is to develop a naturalistic approach to bodily self-awareness and its underlying body representations, combining philosophical analysis with recent experimental results from cognitive neuroscience, neuropsychology, and psychiatry.

**To continue the subject of your new book: your research interests centre around body awareness and the cognition of others. Will the book bring a synthesis of these two domains (e.g. a continuation of what you cooperated on with Alvin Goldman)?**

Most of the book is dedicated to self-awareness, and, more particularly, to the sense of body ownership (i.e. the awareness of the body as one's own). In order to do so, I need to single out representations of the body that are self-specific for them to be susceptible to ground body ownership. However, recent evidence from the mirroring literature as well as mirror-tactile synaesthesia seems to indicate that some body representations at least are shared between self and others. In Chapter 5 of the body, I thus analyse to what extent body representations can be shared and their implication for the theory of ownership that I defend.

**Although, as we have mentioned, your research centers around two notions, is there any subject your haven't published about yet, but which intrigues you and which you are planning to undertake?**

I am more and more interested in the wide range of bodily sensations beyond touch and proprioception, and, in particular, in pain and in interoception. Bodily sensations have been exploited in the philosophical literature as paradigmatic examples of qualia, thus neglecting their specificity. They remain rarely, if ever, analysed for their own sake. Little is known about how these sensations fit into the architecture of mind. Collectively, they resist analysis as either perceptual or affective states. The variety of bodily sensations is so wide that they may seem to form a mongrel category. What qualifies such a variety of experiences as bodily sensations? What do they have in common? Can one provide a unified account of bodily sensations despite their dissimilarities? I would like to work on these questions in the future once the book is done.

picture source: F. de Vignemont's archives

## THE BODY

**The body, or, more precisely, bodily awareness and bodily self-knowledge, frequently appear in your work in the spirit of Evans' works, as do references to Frege. Where does your interest in these authors stem from? And as concerns the latter of these authors, do you think that the categories present in Frege's research can be successfully used in studies on the sense of ownership, or the representations of the body?**

I was philosophically brought up with Evans, so to speak. The Jean-Nicod Institute, where I did my PhD, is strongly influenced by the Fregean tradition, and by Evans. I was actually surprised the first time I visited another philosophy department in the States that Evans was not the key reference there. There are many aspects of Evans' theory that I appreciate. For instance, his paper on Molyneux's problem is the best I've read on the topic. Another example is his treatment of the notion of immunity to error through misidentification. He was the first to acknowledge the importance of the ways of gaining self-information, and, in particular, of body senses.

**In your aforementioned research there appear references to Gibson's conception. What do you think about his notion of a hand as a subjective object?**

Gibson is, of course, very important if one wants to work on topics such as the self, action and the body. But I prefer to avoid slogans like "subjective object," because you can put everything and nothing behind. I do believe that the hand has a special status in comparison to other parts of the body: we often see our hands, most of our actions are performed by them and their tactile sensitivity is very high. They are thus especially interesting. What is problematic, however, is that most experiments on bodily awareness investigate only the representation of the hand and it is not obvious that we can always generalize results on the hand to the other body parts, let alone to the body as a whole.

**In your work you try to retain and defend the division into body scheme and body image, although you formulate a more functional definition thereof (than, for instance, Gallagher). Do you reject the existence of three distinct kinds of body representations (entailing structural representation of the body) only on economic grounds? On the other hand, are you perhaps planning (or working on) a continuation of your research from "The weight of representing the body...", which was a certain alternative for the divisions present in literature of the subject?**

The more I work on the notions of body schema and body image, the less I want to keep the terminology. There is too much confusion and too little evidence in favour of the dual distinction. Yet, I do not think that the threefold distinction suffices to account for the complexity of the field. I now make two orthogonal distinctions which I call 'cold' and 'hot' body representations and long-term and short-term body representations. The latter distinction is quite standard and comes from O'Shaughnessy. For the former distinction, I use Ruth Millikan's theory about pushmipulliu representations. Cold body representations are purely descriptive. They represent various types of bodily properties, as they can be experienced in bodily awareness. Hot body representations are both descriptive and directive. They have two directions of fit, world-to-mind and mind-to-world. They represent bodily affordances for action. Now, one can ask how many cold and hot body representations there are. But I think that the Bayesian approach makes this question meaningless.

**Sometimes in your works there appears the idea of body space: maybe this concept would be more satisfying than earlier distinctions, especially if we remember how important a role space took e.g. in Kant's philosophy (being aware of the existing differences between these two, obviously)?**

Starting from the intuition that the body where I feel bodily sensations is the body that I experience as mine, I analyse in the book how the mind builds up representations of bodily space: a theory of embodiment requires a theory of spatial ascription of bodily experiences. Spatial ascription of bodily experiences, however, displays puzzling features, which do not seem to fit with what we know of spatiality. It becomes then questionable whether bodily sensations themselves are intrinsically spatial, as it has been suggested by the Local sign theory in the 19$^{th}$ century. If the Local sign theory were true, then one would expect to experience floating sensations with no spatial ascription, and exosomesthesia, that is, sensations experienced beyond the felt boundaries of one's body. But there are no such things (for more details, see the chapter *The mark of touch*, co-written with Olivier Massin for the *Oxford Handbook of philosophy of perception*). I also criticize the sensorimotor approach to bodily experiences, showing that it faces a dilemma. As I argue in my Mosquito paper, if the enactive account is cast in terms of spatial know-how (how to reach and move the body part that is touched), then it is false, because spatial know-how is separable from bodily experiences. If the enactive account is not cast in terms of spatial know-how, then it is unclear what type of sensorimotor expectations could provide the spatial content of any kind of tactile experiences, including instantaneous passive touch. Rather, what I defend is what I call the Body Map theory, partly inspired by O'Shaughnessy's theory. In my view, somatosensory information does not suffice to account for the spatial content of bodily experiences. It needs to be structured by what I call the body map, that is, the representation of the spatial configuration of the body. Furthermore, I argue that the body map is multimodal, that it is dynamic, and that it can be either purely descriptive or both descriptive and directive.

**You devote little attention to the idea of the pre-reflexiveness of bodily consciousness. Asking in the spirit of one of Adrian Alsmith's papers, do you think there exists such a thing as pre-reflexive consciousness? Is it something more than marginal consciousness (referring to your own work)?**

I think this may be more a terminological debate than a real disagreement. The notion of pre-reflexive consciousness comes from the phenomenological tradition. But it is not clear to me what exactly is meant by pre-reflexivity, and this is why I avoid it. Is it merely a matter of attention or introspection? But then I agree that we do not attend to most of our bodily experiences. Or is it a matter of conceptualization? But then I agree that bodily experiences have non-conceptual content. Or is it something else, but then what?

**This is our last question regarding the subject of the body: from the studies on the rubber hand illusion there appears to emerge a rather paradoxical picture of our embodiment, as it seems that it is sight which plays a key role in our feeling of being corporeal, especially in the context of the recently published results showing the lack of RHI in visually impaired people. What are your thoughts on this issue?**

I was actually very happy when I found out about this recent study by Henrik Ehrsson with blind people. It is not so often when experimental data confirm your theories… I strongly believe that vision plays an important role for bodily awareness. This is well accepted for action, why not for the body? For too long, philosophers have emphasized the importance of proprioception. However, recent literature about multisensory interaction shows that bodily experiences are not exclusively based on somatosensory information. They are rather multimodal, and even constitutively so, as I defend in a paper currently under revision. I argue that blind people experience their body differently that sighted people, as this is well illustrated by this study. I do not see why this raises difficulty for the problem of ownership. Actually in a chapter for a collected volume on immunity to error through misidentification edited by Recanati and Prosser, I argue that vision can ground bodily judgments that are immune to error in some circumstances and even when it does not, multimodality is not an obstacle for immunity.

**Could you comment on the opinion below?**

> ***Frederique de Vignemont recently co-authored an article with Alvin Goldman on embodied accounts of social cognition in the journal Trends in Cognitive Sciences. What they called embodiment was anything but embodiment. (…) So their question comes to this: how does a body, without a brain, isolated from its environment (including the social environment), and unable to perceive the bodily behaviors of others, discover the mental states of others? Their answer, what they call the best (or 'most promising') candidate for an embodied account, paradoxically, is that social cognition depends on body representations in the brain – paradoxically, because they ruled out appeal to the brain in any true embodied account. In effect, what they call the best candidate for an embodied account is an account that excludes any contribution from the body. Obviously, if this is considered an embodied account, there is a problem.***[49]

Most evidence in favour of the so-called embodied approach to social cognition comes from the discovery of mirror systems, and nobody can deny that they are in the brain. One can then say that Gallese and all the others are wrong in taking these results into account to defend an embodied view. Or one can try to determine the peculiarities of this specific brain activity in comparison with other brain processes. That's what we did with Alvin by proposing the notion of bodily code or format. It is interesting to

[49] Shaun Gallagher. 2011. Interview. *AVANT*, 2/2012: 81-82.

note that Gallese and Sinigaglia have taken over this notion in their own TICS paper. You could then say that they do not defend an embodied account of social cognition. But if not them, who does?

## OTHER MINDS

**The other large subject in your work are other minds. Although you do not specify it this way, if your works can be considered to conceptualize embodied cognition, would that be embodied cognition of other minds?**

I am interested in embodied cognition, but I would not qualify myself as an embodied theorist. For instance, my view on empathy is not really embodied. With Tania Singer and with Pierre Jacob, we stress the importance of cognitive appraisal, as shown by modulation of the activity of the pain network by many factors, some of them being high-level. In addition, there is a variety of questions about social cognition I am interested in beyond the implications of the discovery of mirror systems. For instance, I recently co-wrote a paper with Hugo Mercier on egocentric and altercentric biases, and their consequences for the Simulation approach. Recent evidence by Kovacs and by Samson indicates that we can be influenced by what we believe other people believe although it is pointless or even detrimental. Since the egocentric bias is taken as evidence in favour of the Simulation approach, we analysed whether the altercentric bias was a counterargument against the Simulation theory.

**On the other hand, there exist numerous indications towards the social nature of bodily representations themselves (a tradition dating back at least to Marcel Mauss's works). Do you think that knowledge about one's own body can be explained without referencing social factors?**

Can Robinson Crusoe move on his desert island? Obviously, yes. And to do so, he needs knowledge about his body. One may reply that he spent all his childhood surrounded by people. Maybe then other people would be important for the acquisition of body representations. But again, if we look at animals that are not social, it is hardly controversial to claim that they have a kind of representation of their body that they use for action. Social factors can influence body knowledge, but I do not think that they constitute a necessary condition. What is more interesting is whether they are necessary for the feeling of ownership. In other words, if you are not aware that there are other bodies, do you feel your body as your own?

**While we are going to return to enactivist concepts, could you share with us your opinion regarding enactivist, direct concepts of cognition of other minds?**

If you mean the theory of direct perception as defended by Zahavi, then I have difficulties in understanding it both for conceptual and empirical reasons. It seems sometimes that it falls into behaviorism, as if mental states exclusively consisted in public behaviours that can be perceived. It is also difficult to accommodate with the evidence

of modulation in empathy, which shows that there is nothing direct. And if by direct, they merely mean the absence of conscious inferences, then it becomes hard to see their disagreement with other theories. Advocates of theory theory and simulation theory have never assumed that mindreading processes are conscious.

**The discussion regarding the nature of mechanisms underlying mindreading has for a long time been structured by the famous "theory theory/simulation theory" distinction. Some currently argue that it is high time we abandoned this opposition as no longer useful and looked for completely new concepts and theories that could explain the ability to attribute mental states to others (perhaps ones directly inspired by neuroscience rather than philosophy). Would you agree with this assessment, or do you think that theory theory or simulation theory (or some combination thereof) could still prove useful? If the latter is true, then which of those theories do you think is on point, or at least closer to the truth? If the former is true, then what do you think a new, alternative explanation of mindreading might look like?**

It is true that several hybrid views have been proposed. As argued by Goldman (2006), simulation and theorizing need not be in competition. Rather, they may cooperate (if for instance, a theory is used to select the pretend inputs). Furthermore, some instantiations of mindreading may result from simulation only, whereas others may result from theorizing only. With the new hybrid views emerges what we might call the multiple routes hypothesis. On this view, there is more than one route leading to mindreading. The multiple routes hypothesis may then be in a position to account for recent results both in neuroscience with the discovery of mirror systems and in developmental psychology with the new implicit versions of the false-belief task, which are successfully passed by infants as young as 13-month old. The multiple routes hypothesis can be declined in many ways: low-level and high-level simulation (Goldman 2006), implicit and explicit mindreading (Frith & Frith 2008), minimal and full-blown theory of mind (Apperly & Butterfill 2012), system 1 and system 2 (Evans 2008), and so forth. But these distinctions raise a number of questions. In particular, along what dimension(s) should mindreading processes be distinguished? Their automaticity? Their availability to consciousness? Their efficiency? Their flexibility? The conceptual apparatus they require? Or the types of mental states they target?

**Pierre Jacob - a philosopher with whom you have cooperated - proposed several years ago that neural structures commonly dubbed "mirror systems" may in fact underlie conceptual representations of certain mental states. From this point of view, structures that are responsible for experiencing some mental states (e.g. disgust) are also used to represent those states conceptually. This proposition appears close to neo-empirical approaches to the issue of encoding notions in the brain. What do you think of such an idea? Do you see any empirical basis that might support it? Do you see it as a potentially attractive way of "embodying" social cognition? Would you consider the neo-empirical approach to notions as such (not necessarily mental notions) close to your research?**

Pierre Jacob was my master and my PhD superviser, so we've had many discussions on mirror systems together. I think he has done a great job in inviting neuroscientists to be more careful with the use of the notion of mirroring. According to Evans' principle of generality, I master the concept 'to grasp' if I am able to use it to represent both that I grasp a peanut/a glass of water and that John grasps a peanut/a glass of water (x grasps y). Such definition of concepts meets the requirement of mirror systems. And indeed, mirror systems have been said to encode a "motor vocabulary" shared between self and others (Rizzolatti et al. 1988). However, we should not neglect also the specificity of mirror systems, namely, the fact that they are pragmatic representations, to borrow Jeannerod's terms. They represent action from the first-person point of view of the agent in interaction with the world. This is well illustrated by Calvo-Merino and coll. (2006)'s study on ballet dancers: dancers have visual familiarity for all the visually presented movements, but only motor familiarity for movements of their own gender. Mirror activity was found only in the latter case. I would like also to add that with Pierre, we both agree that it is misleading to qualify as mirroring shared brain activity in the domains of emotion and bodily sensation. We believe that it deserves a different account. For instance, in a joint paper on empathy for pain, we have defended the view that shared brain activity in the pain network should be understood in terms of enactment imagination.

## INTERDISCIPLINARITY

**It is very interesting to us (primarily as philosophers) how you manage to connect your philosophical works with those strictly experimental? Is it difficult, or more of a natural task? How do you see the role of the philosopher in experimental work?**

Bringing together different approaches is never easy and it is especially hard to find a good balance between theoretical discussions and experimental results. In particular, one danger is to believe that empirical data provide the answers to all the philosophical problems. Empirical data provide only partial or indirect answers to questions more fully expressed in a philosophical context. Philosophical theories and conceptual tools are thus needed for the perspicuous interpretation of empirical data and their systematization. Another danger is to use selectively empirical evidence, taking what confirms the view that one wants to defend, and leaving out the other results that do not fit so well. For example, in the last ten years, research in cognitive science has yielded a vast array of exciting discoveries and provocative hypotheses about bodily awareness. It is only if one takes the time to study systematically the very rich and complex recent empirical literature that one can offer a full-fleshed theory of bodily awareness. I was lucky enough to be trained both in philosophy and in cognitive science. In particular, I learnt a lot during my post-doc with Pr. Patrick Haggard, during which I actually designed and ran experiments. Since then, I've been able to collaborate with several psychologists, and my role has been limited to design experiments and discuss results. I think that philosophers can be helpful at both stages. But even without directly collaborating with psychologists, I think that the interaction between

the experimental and the philosophical perspectives is essential. Through confrontation with empirical findings, one can shed new light on long-standing conceptual issues, hone new conceptual frameworks, and explore and resolve a new range of puzzles revealed by cognitive science.

**You have cooperated with many leading scholars, both scientists (Marc Jeannerod, Uta Frith, Patrick Haggard, Tania Singer) and philosophers (Pierre Jacob, Alvin Goldman). Which of these collaborations do you find most memorable?**

That's a tricky question. I've been very lucky through my research career, I've had the opportunity to meet amazing people both at the professional level and at the personal level. I have also been able recently to collaborate with great pleasure with Alessandro Farnè, Marjolein Kammers, Adrian Alsmith, Olivier Massin and Hugo Mercier. I've learnt a lot with all the people I have worked with. But more than collaborators, they are friends with whom I share a lot more than just papers.

**When you connect philosophical and experimental works, don't you feel as if you were "living on no man's land," between science and philosophy?**

Or on everybody's land I hope, where we can all meet and discuss.

**To close this subject – you practise interdisciplinarity. Do you have a theoretical vision of what this interdisciplinarity entails?**

In all my work, I defend a naturalist approach to the mind. By definition, this involves interdisciplinarity. I do not believe that one can achieve a real understanding of the mind without looking at results coming from cognitive science. My work is at the meeting point between a bottom up approach, based on the analysis of empirical evidence, and a top down approach, based on conceptual analysis. Those two approaches are complementary and both necessary.

## ENACTIVISM

**It seems that over the course of its development enactivism has become more differentiated rather than more defined. It can be treated as a certain frame of conceptualization or a research platform, allowing to coordinate divergent views. How would you respond to the objection that in the otherwise excellent "A mosquito bite…" you have somewhat concocted the enanctivist perspective with which you argue? According to one group, you have dealt with the enactivist approach to bodily experiences; according to another, you help to make them more precise.**

It may be because I share the enactivist intuition that action must play a role in self-awareness that I feel so frustrated when I cannot find a clear articulation of what this role is. It is very difficult to criticize the enactivist view because its proponents can always say that it is not exactly what they meant. But I presented the Mosquito paper in front of Alva Noë and Kevin O'Regan, and they took the objections that I offered seriously. That's a beginning. Furthermore, with Adrian Alsmith, we have recently published a special issue on embodied cognition and body representation in the *Review of Philosophy and Psychology*. Our aim was to clarify the field and to bring together people that do not agree. For instance, there is a discussion between Ned Block and Kevin O'Regan and another between Shaun Gallagher and Daniel Povinelli. They do not agree in the end, but, at least, the nature and the extent of their disagreement is clearer.

**How would you react to an opinion like this: The article "Habeas Corpus..." provides, among others, reasons for enactive grounding for a sense of self, own body, ownership of one's own body in action? Much like e.g. Dana Ballard, never mentioning enactivism or embodiment in his studies, which are described in a rather computational manner, he arrives at the conclusion *You see what you need*, unintentionally supporting enactivism with his research.**

I am aware of this apparent paradox in my theory. Since the Habeas Corpus paper, I have somehow refined my view. In particular, if one wants to give a theory of body ownership, one needs to account for the following puzzles: why do we feel ownership for a rubber hand although action is immune to the Rubber Hand Illusion? And why do we feel no ownership for tools although we can feel sensations there? I still want to keep the view that I defend in the Habeas Corpus article: I feel ownership towards a body part that is represented in a specific type of representation of the bodily space for action. But more work is needed to determine the content of this representation. And we need finer-grained concepts that the body schema.

**You write that "action is immune to the Rubber Hand Illusion", but Zopf et al. show something different, that action is affected. Maybe the issue is that (1) we look at the wrong 'part' of action; (2) in other research on RHI and action, one's own body is the goal of action, but in Zopf et al. there are nonbodily goals (as in one of Kammers at al.'s papers, with similar results as in Zopf's paper), maybe the goal of action makes a difference?).**

I haven't read this new study yet (but it's on my reading list...). However, there is already a reply based on another study by Marjolein Kammers and her colleagues (2010). They found that in some circumstances action could be sensitive to the RHI. In this experiment, the participant's hand and the rubber hand were shaped as if they were ready to grasp an object, with congruent or incongruent width of grip aperture. Both the index fingers and the thumbs of the real hand and of the rubber hand were stroked. After the stroking phase, participants were asked to mimic the perceived grip aperture using their non-stimulated hand or to grasp an object with their stimulated

hand. This time, it was found that both the perceptual and the motor responses were sensitive to the illusion. The representation of the hand configuration did not seem to differ between action and perception. One can find explanations for the differences between the two RHI studies. For example, one appeals to local stimulation of the index finger, whereas the other appeals to a more holistic stimulation of both index finger and thumb. Moreover, one is exclusively about the body, whereas the other is about the relation between the body and an external object. But all these possible explanations merely show that when it comes to empirically dissociating different types of body representations, there are many factors that may interfere. This is one reason among others why I think we should stop counting body representations.

**What is your attitude towards such concepts as situated and dispersed cognition, and thus – to the work of such scholars as Edwin Hutchins or David Kirsh? We are asking here about your approach to studying relations between individuals and their environment (including inanimate environment): can they to any degree have importance for your perspective?**

I haven't really worked on that. The only way I take into account the environment in my theory is in terms of peripersonal space, which I think, is fascinating. But that's far from the issues raised by situated cognition.

## OTHER ISSUES

**Would you agree with J. Kevin O'Regan on the subject of benefits that can be drawn for the knowledge of cognition and consciousness from the study of aesthetic experience? This scholar – as he has put it – expresses doubts whether studying one mysterious thing can distinctly help in understanding another.**

I am really not an expert there. I agree with Kevin that for now we know little about aesthetic experience. But that's precisely why we need to study it.

**Very frequently we find in your work quotations from literature, such as from Gogol's *Nose* or Edmond About's book. Is literature your second passion, after philosophy? If not, then what is?**

Yes, indeed. You might even say that it is my first passion. Some people are addicted to tobacco or coffee. My addiction has always been literature, since I was a child. And you should see me when I have no novel left at home, I can be in a very bad mood. Any suggestion, by the way, about Polish authors I should read?...

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# BOOK REVIEWS

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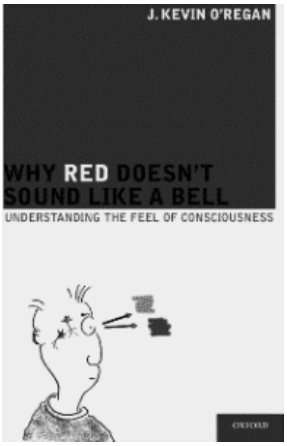
# The redness of red

## a overview of: Why Red Doesn't Sound Like a Bell. Understanding the feel of consciousness

Author: Kevin O'Regan
Publisher: Oxford University Press
Release date: 2011
Number of pages: 224

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Consciousness, the mind and the self have for many years constituted "hot" topics of debate whether among philosophers or scientists. Even though a number of those contributing to such debates attempt to convince the world that they possess the knowledge on the subjects concerning these phenomena, baffling as they are for an average person, only few of the books devoted to the subject available on the market appear to cover it in a competent manner. Doubtlessly the works of contemporary cognitive scientists, that is researchers professionally involved in the subject of cognition, can be counted among these. These works definitely include the book by John Kevin O'Regan, *Why Red Doesn't Sound Like a Bell. Understanding the feel of consciousness*. O'Regan is a renowned experimental psychologist, researcher into perception (among other things he has conducted research into change blindness) and one of the authors of the sensorimotor theory. His interests also include other cognitive phenomena, such as the rubber hand illusion or illusory pain. He works in the French *Laboratoire Psychologie de la Perception*, where he conducts his research.

In his book O'Regan continues and develops the proposal of sensorimotor approach which he has put forward in the articles co-written with Alva Noë[50] (cf. the introduction to the interview with the author in this issue of *Avant*). The book is comprised of two sections: "The feel of seeing" and "The feel of consciousness." The former is devoted to the subject of sight and contains five subchapters, whereas the latter covers the subject of consciousness and contains as many as fifteen subchapters. The book is written in a readable style, although it is addressed primarily to readers familiar with the publications of cognitive science. Nevertheless, even a beginner in the field of cognitive studies should not have major problems with following most of the presented examples, since they do not require specialist knowledge.

As has already been mentioned, in his book O'Regan adopts the sensorimotor approach, which is linked with the recently popular embodied and enactive approaches, and draws upon the ecological psychology of James J. Gibson for inspiration. For this researcher, seeing is not a passive extraction of stimuli from the surroundings but its *active exploration*, which means that it constitutes a form of action. The author wonders what it is that humans possess that enables them to experience various things: the smell of flowers, pain, or amazement over a rainbow or music on the radio. For a long time cognitivists were convinced that it is only the brain that creates such experiences and human consciousness. O'Regan suggests that consciousness and all experiences take place as a result of interactions between the body and the surrounding environment. These interactions are reciprocal and dynamic. We see the external world not because we patch together various pictures representing it in our brains, but because of the aforementioned interactions. Thus, when asking about the experience of a flower's smell or the firmness of an object, the author claims that this is something that we do, and not something that happens to us. Therefore, he poses quite a radical thesis which stands in direct opposition to many claims of heretofore conducted research into perception.

In the first part of the book the author focuses on the visual perception. To begin with, he presents the first important research into sight, performed by Johannes Kepler, as well as the issues connected therewith. Kepler discovered that the image produced by the retina is real, reversed and reduced in size. This, in turn, led Descartes to pose the thesis, which the contemporary philosopher and cognitivist Daniel Dennett has called "the Cartesian theatre." That is, Descartes suggested that there are events taking place in our heads that resemble those performed in a theatre; obviously, in the case of the process of seeing we neither "watch" nor analyze any performance, but reversed images appearing on the retina. This notion resulted in the acceptance of the existence of a homunculus: a "small human" who watches the images in our head. While O'Regan counts among the many contemporary researchers who dismiss this hypothesis, he points to the fact that the spirit of this "small human" still haunts many researchers into human perception.

[50] E.g. the already famous article by J.K. O'Regan and A. Noë 2001. A sensorimotor account of vision and visual consciousness. *Behavioral and Brain Sciences*, 24: 939-1031.

In the following part, O'Regan freely moves onto characterizing the structure of the eye. Rather than bombard the reader with Latin names and anatomical lectures, he presents a handful of interesting facts on the subject of the blind spot (the place on the retina devoid of photoreceptor cells that would enable seeing), as well as several simple experiments connected therewith and presenting its influence over perception. He subsequently introduces to the reader the notions of other impairments limiting vision. However, these impairments should not be mistaken for the "usual" visual impairments, with which we go to an ophthalmologist. Indeed, O'Regan showcases certain natural imperfections of our visual apparatus. The experiments presented in this part of the book can be conducted at home without much problem, which is undoubtedly to their advantage. This can be exemplified by the experiment concerning the peripheral perception of colours, thanks to which the readers may see for themselves the truthfulness of the thesis of the human eye's very poor ability to perceive colour.

Towards the end of the first subchapter the author describes one of the first experiments concerned with change blindness, a phenomenon which constitutes one of O'Regans primary interests. In the second subchapter the tenets of the sensorimotor theory are explained in an accessible manner. O'Regan begins with a simple experiment and its variations, in the course of which the reader is to imagine the act of recognizing objects hidden in a sack through touch alone. This serves as a subtle way to introduce the thesis that our perception is in fact an action, an active exploration of surroundings, rather than a passive process of creating its representations in the mind - which are rejected in the sensorimotor theory. The author explains it in a simple way: we never see the whole picture. Our eyes are constantly in movement, continually going through the environment and extracting data out of it. Seeing, as O'Regan puts it, is a process of continuous engagement in asking questions and seeking answers regarding "the things in front of us." Similarly as in the previous part, various experiments are described, such as an experiment with reversed image, namely a situation when the tested subjects put on glasses which cause them to see everything upside-down. Furthermore, the chapter contains description of the problems encountered by people taking part in this experiment, for instance the difficulty related to attempting to shake someone's hand.

The next part of the book presents a popular view, rejected by O'Regan, regarding "seeing everything." The fourth chapter, entitled "The Illusion of Seeing Everything," contains examples demonstrating the fact that in reality we see far less than we imagine. The author presents among others the famous experiment with the gorilla dancing in the centre of a stage, which is overlooked by the viewers. The chapter also presents a number of other examples of inattentional blindness and change blindness, most of which can be found on O'Regan's website (http://nivea.psycho.univ-paris5.fr/). The first section ends with a discussion about the notion of representation, wherein O'Regan attempts to defend the theory that the human brain does not hold a representation of the outside world.

In the second part of the book the author goes on to characterise other senses: hearing, touch, taste and smell, and, most importantly, moves on to the eponymous question of consciousness. This begins with a description of one of O'Regan's childhood dreams, namely building a conscious robot. This reference is not coincidental, as O'Regan briefly presents the history of Artificial Intelligence and robotics, as well as the challenges opening before these fields. The attempt at creating an intelligent robot also bears significance to the field of studies over cognition, since there arises the issue of the robot's consciousness. By constructing a conscious robot, we would learn something about the consciousness in general.

In the following chapter, having reviewed both the scientific and the popular ways of understanding consciousness, O'Regan conducts an extended discussion of phenomenal consciousness: the most "raw" and conscious feel. This is illustrated by the example of the pain resulting from an injection. One can imagine various things regarding this procedure, and the very thought may cause goosebumps. During the procedure, the body also begins to react appropriately: the hand might jerk when the needle breaks the skin, one may feel faint, and so on. However, all these aforementioned phenomena exist above (and beyond) what O'Regan calls the raw feel of pain resulting from an injection - as he writes, when we put aside everything we have imagined with regards to an injection and all physical effects of the stinging, then what remains is the "raw feel" of pain resulting from an injection. Subsequently, O'Regan looks for a place or mechanisms causing these raw feels, having concluded that they cannot be equated to brain functions.

Having analyzed the popular cognitive approaches to the issue of such experiences, the author moves on to present this notion from the sensorimotor perspective, which he develops himself. The discussion does not lack an example already classic for O'Regan, namely driving a porsche. The question of what makes driving a porsche special touches specifically upon the matter of raw feel. O'Regan argues that these experiences are generated not only by the brain, which doubtlessly participates in these processes, but by our sensorimotor apparatus and interactions with specific objects. The subsequent sections of the text contain references to problems which may be identified a belonging to the area of the philosophy of the mind.

One of them is the question of animal and child consciousness. The author approaches the subject by asking about the impression of the unremitting experience of sensations, that is the feeling that it is a continuous process. The following chapter continues with the subject of raw feels, this time regarding the perception of colours. Moreover, O'Regan discusses a very interesting experiment, which can actually be conducted at home - provided one has enough free time. For three weeks, the subject of this experiment wears appropriately coloured glasses (both of which are half-coloured: one is half-blue, and the other half-yellow). When one moves his or her head while wearing such glasses, he or she sees everything slightly tainted with blue or yellow. As it turns out, the effect remains when after three weeks the subject takes off the glasses and starts to move his or her head. However, this ceases quite soon. As this experiment confirms, besides the processes taking place in the brain, our sensorimotorics also takes a vital part in experiencing colours. The next chapter showcases the ex-

tremely interesting issue of "sense substitution". In other words: is it possible to see with one's ears or with the aid of touch?

When replying to that question, O'Regan analyses, among others, the famous experiment carried out by Paul Bach-y-Rita[51], as well as one of the latest variations on this experiment. It basically consists in covering the subject's eyes, the function of which is supposed to be taken over by one of the other senses. A video camera records image, which is then transformed into e.g. sound, heard by the subject. During these experiments it turns out that to a certain degree, some senses can play the role of others, if the latter do not function properly. For example, Bach-y-Rita's report suggests that people who were born blind reacted emotionally to erotic pictures or photos of people they loved if these images were put under a special camera, the transformed signal from which would reach the sense of hearing or touch.

In the subsequent part of the book the author demonstrates how the research conducted thus far regarding sensory substitution and the theories explaining this phenomenon connect with the sensorimotor theory. The following part presents the issue of touch from the sensorimotor perspective. The chapter, besides containing dry theory, acquaints the readers with various experiments, at least one of which can be easily done at home, namely, the illusion of the rubber hand. The chapter explains other illusions related to bodily experience as well. The author, among other things, explains how contemporary science accounts for the out of body experience. Besides describing classic phenomena of cognition, emotions, thirst, itching and pain are also briefly described.

O'Regan finishes his book with a very optimistic and, at the same time, daring declaration of faith in that the sensorimotor theory, as developed by him, can face up to the problems encountered by the traditional theories, which make consciousness into a phenomenon impossible to produce by biological entities. An interesting reference to this issue can be found in the interview with this researcher, as published in the present issue of *Avant*.

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# Complexity is around us

## a review of Complexity: A Guided Tour

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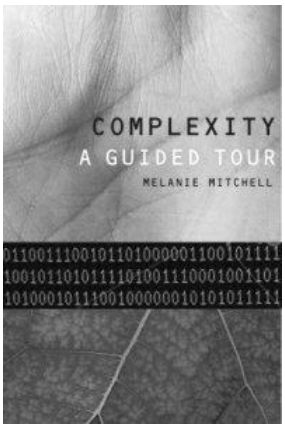


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What is complexity? Why are some things complex and others are not? Why cannot complex things be captured by methods used by classical reductionism, and why does the understanding of complexity matter today? If these questions are too general, then perhaps: how does an ant colony organize itself? How does the immune system work? What is a logistic map and fractal dimension?

These are some of the questions that Melanie Mitchell tries to answer in her guidebook to the phenomenon of complexity. Does she succeed? Well, in my opinion she does because it is not just a simple book about complexity. It is a book written with passion, where a large number of topics are related to biographic stories. It is a fascinating introduction to many interdisciplinary issues that have one word in common - complexity.

The title of the book suggests that it is addressed to newcomers and certain explanations are simplified as much as possible, as they might be in a book for high school students. However, there are also topics that can attract scientists who are already familiar with the field of complexity. The last chapter concerns the current state of researches about complexity and includes a discussion about whether complexity sciences (if they even exist) may be interesting for scientists. I believe that this book could be easily understood by anyone who is interested in computer science, mathematics or genetics. Mitchell manages to write and explain important aspects with clar-

ity without becoming too technical and claims that no special background is required. However, those without any basic knowledge of computer science or sciences of complexity may nevertheless find some parts of the book difficult, even though Mitchell tries to avoid mathematical equations whenever possible. And this is the first thing that makes my opinion about the book mixed. In general, there are some topics in complex sciences that, in order to be understood, require mathematical equations to be a part of their explanation and one simply cannot avoid them. Then again, *Complexity: A Guided Tour* covers a large number of issues arising from different areas of science, so there will always be some readers for whom certain topics are going to be more difficult to understand than others.

The other misgiving I have experienced is related to the missing feeling of being, so to speak, guided. The author at times provides too many details. While some items are presented in a technical way I felt on numerous occasions that some connection between different subjects was missing and one could easily forget what was the relevance of presented issues. Different aspects of the subject were not navigated properly, and I did not experience the "eureka" feeling when I completed the final chapter. As for the guide, I would rather know where I am at the moment, why I am there and where I am going. It was hard to sense that guidance while reading this book.

What are the advantages of this book? The first one is history. Mitchell explains very well what the pre-twentieth-century scientists thought about the universe and how the discoveries made in the twentieth century reshaped our science. She also does a good job in showing why a phenomenon of complexity is now one of the most challenging subjects for contemporary researchers.

The second one concerns the large number of issues that are covered, ranging from the purely physical to the biological and the social ones. Mitchell provides accessible and clear explanations for such topics as: dynamical systems theory, chaotic dynamical systems, information, information processing in living systems, fractals, computation, computer modelling, networks, scaling relationships and power laws, cellular automata, genetic algorithms, evolution, and molecular genetics. Nevertheless, I wish Mitchell had gone into some more detail on the notion of emergence (its history, philosophical connections and current problems).

The most compelling part of the book occurs when Mitchell explains the theory of information and computation and ties this theory with evolution. She shows how genetic algorithms and other computer-based mechanisms (such as cellular automata) are able to evolve, and can solve certain problems. But that is hardly surprising, since she is a professor of computer science and her major work concerns the areas of genetic algorithms and cellular automata.

Other advantages of the book include the critique and challenges it posed to well-known theories. The author offers an alternative views on Wolfram's New Kind of Science (NKS) and game theory. These parts are also fascinating because - for some researchers - NKS is essential to understanding complex systems.

As for the structure, the book is divided into five parts. Part one is an introduction to complexity, chaos, information, evolution, and is supported with background history. Part two moves to the topic of life and evolution in computers. However, some interesting examples of artificial life (such as: boids, Langton's ant) are never mentioned. Part three is devoted to computation and here Mitchell provides the alternative view on Wolfram's work. Part four explores networks and issues that exist on the social level. Finally, the last (fifth) part concerns the past and the future of the sciences of complexity. Every chapter is also supplemented with pictures of the problems under discussion and photographs of the individuals who contributed to certain theories and discoveries.

Melanie Mitchell has provided a valuable overview of complexity for newcomers (if you do not know what complexity is) and people from the field (if you want to challenge some popular views, such as NKS or the existence of science of complexity). I enjoyed this book and can recommend it for everyone who is not afraid of entering the complex world of complexity.

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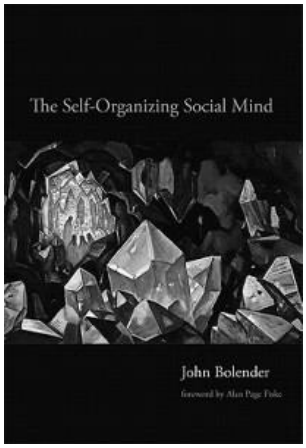
# Bringing sociality into the realm of the brain physics

## a review of *The Self-Organizing Social Mind*

Author: John Bolender
Publisher: A Bradford Book
Release date: 2010
Number of pages: 208

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How can basic features of social structures be explained? How such structures are conceived and perceived? In his fascinating, new book, John Bolender attempts to find answers to these questions and claims, along the way, that the explanation of the phenomenon of sociability – one of the human's most amazing capability - is findable through the investigation of the physical account of symmetries and symmetry breaking.

### 1. The Idea

Pythagoras was one of the first of researches, who claimed that physics could shape our understanding, reasoning and behaviour. Since Bolender claims, that the core of social-relation is shaped by symmetries and symmetry breaking it makes him a natural descendant of that line of thought. Let us find out why.

Contemporary physics does vast amount of research. For Bolender's approach, however, the most crucial area of research is the dipping into the very existence of necessities, particularly, the deep ones. Such a notion could be defined as the simplest arrangement possible; an arrangement, which is necessary and which just could be different in no other way. For example, this basic equation of 2+2 = 4 is deeply necessary and so is the fact that light travels along the shortest of paths. Therefore, inevitability and simplicity are the two intrinsic features, which we might find when we examined the above phenomenon.

As the author puts it, *there is a reason to suspect that there are deep necessities in how humans mentally represent social relations* (Bolender 2010 : 1). He is also right concluding that symmetries are the essentials of such necessities. Symmetries - Bolender says - are the transformations where originals and images are identical in every way possible.

For physicists, the existence of the deep necessities and symmetries in numerous physical phenomena is the *beauty*. For philosophers and, I hope, for these who will go into details of the book, *the beauty* will emerge from the core of sociality and from the way we think about social relations and perceive other individuals. To reflect upon this claim and to defend it is what Bolender intends to do with this work. However, how to substantiate the above points more? Use the very reductionist premise. Bolender goes this way - a success! He concludes that origins of the mental are purely physical.

> *All the relevant information (data) supports the view that it is wholly biological, both with regards to the growth and development of each individual and with regard to the evolution of the species* (Bolender 2010 : 3).

Assuming that mind is explainable only by physics, the same can be said about thought, the direct result of mind activity. Indeed, in view of the fact that entirely physical causes generate similarly physical effects, this assumption is very plausible. Therefore, since deep necessities are found in physics, the laws of physics govern mind, and its origins are physical, then the necessities can be expected to be found in the mind as well. *This includes the social mind*, says Bolender, i.e. the social cognition, or as he puts it, the relational cognition. The presence of the deep necessities, as we learn from the book, accounts for the presence of symmetry. To put it another way, physics shapes the mind and social cognition, but they are also shaped, in a part, by deep necessities and most importantly, by symmetries. *Physics is beautiful. Thought should be too* (Bolender 2010 : 5).

**2. Interdisciplinary explanation**

The first thing that draws attention is the way the author treats science and modern interdisciplinary framework that the science provides. Achievements of contemporary sciences like physics, biology, neuroscience and philosophy are very important for Bolender and occupy a core position in his narration. With this, the thinker demonstrates how sciences intermingle with one another creating a coherent and a powerful line of thought.

The prime example of the above is found in the explanation of how brain generates symmetries. Bolender has included the symmetries-generating process for a reason. Symmetries make up the fundamental set of patterns that can be found and observed. Look at the pattern of footfalls (*gait*) when you walk. Why does the *gait* look like this? It is because the Central Pattern Generator (CPG) - a network of neurons oscillating jointly - fires at the same time, breaks and creates symmetry; the phenomenon of the *pattern of neural activation* is what generates the symmetry of the footfalls. A single pattern (*neurons*) generates another one (*gait)*. Bolender argues that an analogous mechanism exists in the generation of social patterns. He claims that symmetry and *(...) spontaneous symmetry breaking structures the activity of the social pattern generator just as it does in central pattern generator.*(Fiske 2010)' In this explanation, the author links the fundamental physical processes with neuroscience and neuropsychology, venturing from the realm of brain physics into the genesis of social cognition.

There is, however, something more basic and more fascinating in the approach to social relations that Bolender proposes. By structuring his book around examples taken from diverse natural sciences like biology, neuroscience and the theory of evolution, the author demonstrates that despite providing a perfect starting point for the explanation of social relations and their links with symmetries, the sciences cannot explain everything and more fundamental, self-organizing, physical processes must exist in the basis of sociability. Effectively, Bolender sets up a bridge spanning the traditional, structural explanation of social relations and the more dynamic, novelty approach to the matter:

> *(...) mental representations of social relations are structured by symmetries, which break in determinable order, yielding descending subsets of previous subsets of previous symmetries.'* (Fiske 2010)

Thus, the current Bolender's book deals not only with issues fundamental to the genesis of social cognition but also with more complex forms that might arise during processes related to the social cognition.

**3. Reductionism**

It is a natural tendency to look for weak points while writing a review of anything. In the case of this Bolender's book, it is not easy. The language is very clear, precise and to the point. Every notion, interposed by Bolender, is explained with great care and attention to details. I have no doubt that even nonprofessional physicists, neuroscientist, biologist, or not experts in social cognition can easily follow the main point of this highly illuminating book. It is a real pleasure to read.

However, there is a risk that the deeply reductionist view presented in the book - that the construction and perception of social relations depend on patterns of symmetries and symmetry breaking in the brain – might be dismissed by researches and scientist in the favour of traditional explanations of social relations' genesis. It would be an unfair treatment of the book and a big mistake. Even if further research proves Bolender wrong, the main thesis of his book is far from being illogical or naive because it is strongly grounded in empirical science, powerful argumentation, logic and reasoning.

Whom is this book for? I think it is for everyone who is interested in the connection existing between mind and society, in the mechanisms of social cognition, or in the way, we perceive other individuals. These who want to have an insight into the way we behave, build relations, or think should also read it. As Alan Fiske does, I strongly believe that this book is setting foundations for a new way of thinking about social relations and a new paradigm of thought demonstrating the power of human creativity and imagination.

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# Language in Action
## a review of *Reinterpreting Gesture as Language. Language "in Action"*

Author: Nicla Rossini
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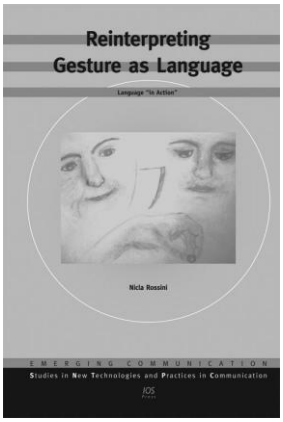


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With the increasing awareness of neural links between gesture and language, the traditional distinction between “linguistic” and “gestural” behaviour has become less clear. Gestures share many traits with certain components of speech (especially prosody) and some of their aspects are studied on a relatively similar methodological basis (Gibbon 2011). Yet, the integration of gesture studies and linguistics remains a challenging task. It is not only the question of unified terminology. Nicla Rossini, the author of the book under review, has a strong academic background in linguistics but most of her research has been devoted to non-verbal communication. Her work clearly draws on the pioneers of gesture studies (McNeill, Kendon) and traditional linguistics, but, simultaneously, it is strongly driven by the cognitivist way of thinking and recent advances in neuroscience. The cognitive-neuroscientific perspective seems to offer a platform where speech and gesture can be studied jointly in the context of interpersonal communication.

Humanities have always been plagued by unclear definitions and hazy notions. Therefore, it is not surprising that the first three sections of the book under review are devoted not only to defining its focus, but also to introducing and explaining fundamental terminology. In a well-thought out introduction, Nicla Rossini reaches back to the seventeenth century to look for the first traces of “modern” gesture studies. This brief but informative overview clearly shows that the idea of the close co-existence of language and gesture was quite obvious to some scholars of the past and that the breakthrough induced by the works of McNeill and Kendon did not come from nowhere. However, among their predecessors, the number of linguists who dared to capture the role of gesture in communication in a more systematic way is almost negligible. Nicla Rossini does not forget to mention Bloomfield and Bolinger. Although in her own approach she leans towards cognitivism, she returns to some linguistic classics many times throughout her book. Presumably, she intends to show the evolution but also a kind of continuity of thought in the study of language, and its changing relationship to the study of gesture.

Another attempt at defining is focused on the notion of Non-verbal Communication (NVC). In order to capture its multifaceted nature, Nicla Rossini visits a number of fields of study. She pays particular attention to the early works by Efron (1941) as well as Birdwhistle and, later, Kendon, both of whom he inspired. She points to the fact that for his kinetics, Efron adopted the structural approach typical of phonology (where kineme is understood as a non-verbal equivalent of phoneme). She tracks the similarities and differences between Birdwhistle's kinesics and Kendon's later works. Moreover, she briefly addresses a number of influential ideas stemming from proxemics, ethology, semiotics as well as philosophy and pragmatics. Some formal and computational works are also included. Language-Action Theory (Flores & Ludlow 1980) appears to be especially relevant in the context of the present book. A significant contribution of ethology (followed by many scholars dedicated to the study of non-verbal behaviour) as well as MacKay's (1972 and later) and a more recent Poyatos' work (2002) is mentioned.

NVC is initially understood as referring only to these non-verbal cues “that are intended as communicative and/or interactive in the sense of Ekman and Friesen work (1969).” However, after a relatively thorough discussion of possible viewpoints, Nicla Rossini returns with a more precise definition of NVC as

> *the intentional transmission of information, either for representational, emotive, poetic, and conative purposes, from a transmitter A to a receiver B, mainly and prototypically through the visual channel, but also through the vocal-auditory channel, by means of specific codes, either innate or culturally-determined, that are not usually specialized for verbal communication.*

While constituting a step forward, the definition refers to the classical categories of language functions and to an early model of communication. The notion of intentionality is adopted from MacKay's (1972) study which is likewise a relatively dated work. As the term “non-verbal” has been criticised as inappropriate for gestures by McNeill (1985) and others, it may be surprising that the Author did not look for a different one.

The questions of communicativeness and intentionality in (gestural) communication remain fundamental and... unanswered. Intentions are hardly accessible for empirical studies and remain hypothetical mental processes, despite a long record of efforts devoted to their identification and understanding. Nicla Rossini proposes more flexibility when it comes to deciding on the communicative value and intentionality – not only of gestures but also of speech. Some aspects of speech are difficult to control intentionally, sometimes speech may be focused on inner communication or just on the internal planning of actions. These facts do not deny its communicative intentionality and should not be an argument for denying it in the case of gestures.

Nicla Rossini makes her own attempt to define gesture itself. As in the previous chapters, the problem is approached from multiple directions, starting with the etymology of the word. Much attention is paid to the available categorisations of gestures and to their specific classes. While the early distinction between communicative and non-communicative gestures (Rosenfeld 1966) may be still acceptable, one would expect more recent viewpoints to be included. The Author moves from Ekman's and Friesen's categorisation (as based on Efron's ideas) through Argyle's proposal and, finally, towards McNeill's and Levy's conception (1982). She discusses and compares these approaches in detail but mentions many others. She also refers to Kendon's distinction between *gesticulation* (occurring as bound up with speech) and *autonomous gestures* – standardised and apt to function independently of speech. A problematic issue that touches some of the available categorisations is that their categories are defined on *different levels* – i.e., using different criteria and referring to various classes of processes.

In this context, the question of *multifunctionality* arises. Apparently, there is no reason to reject the possibility that a single gesture may realise many functions or belong to multiple functional categories. It might be fruitful to follow recent developments on the ground of dialogue analysis (Bunt 2009).

In the final part of the chapter, Nicla Rossini brings up the notion of lexical access drawing on the Levelt's (1987) model of speech production. She points out that “if one interprets gestures as semiotic means, it is easy to see that a form or combination of forms and trajectories is usually aimed at conveying a precise content, or “signified” (in the Saussurean sense).” This thought leads the Author to her own understanding of gestures. She defines them as “intentional movements of hands, arms, shoulders and head, occurring within communicative acts, whose lexical access is shared both by the speaker and the receiver,” while co-verbal gestures as “a subset of gestures strictly correlated to and co-occurring with speech within communicative acts”. Nicla Rossini re-defines also gesture categories (emblems, metaphors, iconics, beats and deicits). While superficially similar to those by McNeil and Levy as well as to Kendon's approach, Rossini's definitions introduce more flexibility.

The fourth chapter of the book, devoted to the cognitive foundations of gesture, starts with the widely discussed issue of interrelations between speech and gesture. Are gestures non-verbal? But what does “being verbal” mean? If Rossini's understanding of gesture implies lexical access, it must be “verbal” in a sense. If gesture and speech are >>the overt product of the same internal processes<< (as suggested by McNeill 1985),

why should they be different in this particular respect? Still, the issue may become less obvious when various categories of gestures are taken into account, as it is shown in Butterworth and Hadar's model (1989).

Gestures are sometimes compared to the prosodic component of speech. In fact, there are many similarities, including those which make both of them difficult to deal with. One of most popular approaches is to divide prosody into linguistic and non-linguistic (e.g. emotional). Gussenhoven (2004) argues that the linguistic component of intonation is characterised by discretely different pitch contours which refer to (discretely) different linguistic categories as well as arbitrary form-function relations and a duality of structure. If one finds this distinction appealing, it might be possible to re-apply it on the grounds of gesture studies.

Tracking the cognitive foundations of gestures, Nicla Rossini very aptly chooses the areas to look for evidence: the emergence of gesture in infants and the gestural behavioural of aphasics and the blind. Due to technical difficulties, ethical issues as well as conceptual problems, infant and child gestures still remain understudied even though such inquiry might provide answers to some fundamental questions of the field. Such studies rarely go beyond observations. There is, however, much evidence on the importance of gesture in early child-parent interaction and on pointings as the first category of gestures to emerge in infants. Nicla Rossini refers to Hewes', Werner's and Kaplan's as well as de Laguna's works showing that early iconic gestures in children are gradually replaced with vocalisations and verbal expressions and therefore can be regarded as “a primitive mode of cognitive representation”.

When discussing gestures in aphasics, Nicla Rossini mostly bases her work on Feyereisen's (1991a, b) studies. She mentions his critique of the verbal vs. non-verbal dichotomy in the context of hemisphere functions. However, since the nineties, a huge body of work has been published on the localisation of language functions in the brain as well as on their disorders. Even though the general tendency towards a complex, distributed model of processing is still dominant, more findings could have been mentioned here. In her brief discussion on gestures in the blind, Nicla Rossini reveals some of their peculiar features. For example, when using pointings (which happens extremely rarely), the blind sometimes add an acoustic cue by tapping the referent. In the conclusion, however, the Author admits that there is no substantial evidence that some categories of gestures (“flat hand”) performed by blind were intended to communicate rather than to support orientating mechanism for the self.

In Chapter 5, the issue of intentional and communicative value of gestures is addressed. Nicla Rossini gives a reasonably detailed review of the two main opposite views and their major claims and does not forget about some other approaches. She mentions also de Ruiter's (2000) conciliatory suggestion that the conflict between the two views is merely apparent. Herself, she seems to be prone to advocate for the more straightforward Kendon's approach stating that gestures provide information about the semantic content of the utterances. She criticises the analysis of Krauss et al. (2000), pointing to a possible misinterpretation of the key gesture in the material under study. Rossini follows the thought of Cassel who stresses that gestures seem to be listener-oriented as they are normally produced synchronously with the rheme of the

accompanying utterance. She also argues that the claim that gestures are not communicative because they do not convey unequivocal meanings themselves is not justified: by definition, they always co-occur with speech. One must note, however, that the understanding of what "to communicate" means may significantly influence such discussions.

The crucial part of the presently discussed chapter is devoted to the reinterpretation of gesture as a prototype category. In order to arrange gesture categories, Nicla Rossini employs five parameters: intentionality, awareness, abstraction, arbitrariness, and extension. As a consequence, she obtains a representation of gesture as a (Roschean) category with the arbitrary emblems in the middle as "the most intentional, aware, arbitrary and abstract class of gestures" and with the most outer circle belonging to batons (beats) which "do not have lexical access but follow and resemble the rhythm of co-occurring speech flow". She proposes a gradient approach to intentionality which seems to be very natural and probably may also be applied to verbal behaviour. Various categories of gestures can be attributed with various degrees of intentionality over the semiotic continuum.

In search for support for her hypothesis, Nicla Rossini carries out an empirical study based on a series of three conversational sessions with a group of five Italian native speakers. The sessions differs in the degree of formality, from a job interview in a foreign language (English) to a guessing game in the native language of the subjects. The use of the foreign language was intended as an additional obstacle to evoke more gesturing. The Author assumes that less intentional gestures would be more difficult to inhibit in formal situations while those highly intentional (like emblems) would be easier to control. Her observations support these claims and indicate that co-verbal gesticulation helps speakers in thinking, also by dissipating emotional excitement. However, the technical description of the experiment is rather sparse and the number of subjects is quite limited when confronted with the set of variables coming into play. As a result, it helps to gather some hints and cues but cannot provide conclusive results.

Going further in her efforts towards bringing gestures back to language, Nicla Rossini makes an attempt to prove that gesture, together with speech, is an instantiation of the human language capacity. She addresses the issue from a neurological perspective and starts with an overview of the studies and views on the neural correlates of language. She confronts the idea of modularity of mind (Fodor 1983) and the connectionist approach involving distributed processing. In this context, she also discusses a selection of hypotheses on the origin of language. While it was obviously not intended to give justice to all of them, at least a few more sentences on the McNeill's claim that language originated as a multi-modal system would have been welcome.

Nicla Rossini cites a number of works devoted to the issue of lateralisation and argues that some traditional approaches to the function of brain areas as "language control centres" cannot hold any longer as many other factors and areas contribute. Although this view is not new, it turns out to be difficult to find sufficient and direct experimental support for it. As the Author argues, the results of neuroimaging studies are still extremely difficult to interpret and they may support significantly different views

on the nature of the neural substrates of language. Nevertheless, many neuroimaging studies mentioned in the chapter show that there exists a strong link between the syntax of natural language and the syntax of human action (Bongioanni et al. 2002). Another relatively new and seminal idea is modelling the functions of human nervous system in terms of the Darwinian principle of selection applied to neuronal groups, which has been already supported by recent fMRI studies (Edelman 1987). The declaration by Armstrong et al. (1995) on the necessity of redefining the physiological base for language does not sound very radical today. The idea of distributed processing has gained popularity but it is still difficult to judge specific contributions from the neuronal units involved and their interactions as they are extremely difficult to isolate.

In search for support for one of the main hypotheses of her book, Nicla Rossini conducts another experiment focused on gesture-speech synchrony. The participants are asked to perform simultaneously two activities: to read a a piece of prose or a poem, and to imitate a beat (rhythm) previously presented by the experimenter. Although depicted in more detail than the first one, this one also needs more light to be shed on its technical aspects, including transcription and annotation. A more quantitative-oriented approach would probably be beneficiary as well. One may argue whether knocking on the table can actually be classified as gesturing. Obviously it bears much resemblance to beat gestures but, as a study by Karpiński et al. (2009) shows, the synchrony rules hypothesised by Kendon (1980) and McNeill (1985) may be violated due to the influence of external, contextual factors. Moreover, although the methods for analysing synchrony between rhythmically complex phenomena like speech and gesture are still under development, some promising approaches have already emerged (e.g., Port & Cummins (1996), Cummins (2009), Leonard & Cummins (2010); see also Rusiewicz (2011) for a brief overview).

The history of gesture-speech synchrony hypotheses formulated by Kendon and Mc Neill as well as the arguments of Butterworth and Hadar (1989) form a departure point for the seventh chapter. Nicla Rossini assumes that the presence of synchronisation patterns in congenitally deaf orally educated subjects would prove that they are inborn. In order to obtain empirical support for her claim, she analyses a stretch of spontaneous conversation (involving congenitally deaf speakers) for co-occurrences of gestural strokes and accented syllables. She reports that Kendonian rule was always followed (strokes should occur no later than the corresponding accented syllables). Blind subjects synchronise gestures and speech, providing another piece of evidence for the shared cognitive-computational origin of speech and gesture. Again, although based on careful observations, the results might have been more convincing if the author had provided more detailed information on the technical aspect of the study. The detection and precise tagging of prominences in speech and kinematic landmarks in the stream of gestures, as well as deciding on their hierarchy and mutual relations, is not a trivial issue, especially when dealing with sparse data, coming from only few subjects. These flaws are somewhat compensated by a gist of precious finding regarding gestures in the deaf, especially on their locus, the point of articulation and the gesturing rate.

The link between prosody and gesture may be of a complex nature. Asynchronous gesturing is simply more difficult (e.g., musicians must practise to use their hands independently) and it may require divided attention. Speech can be viewed a sequence of gestures, sharing the nature of any other volitional body movements (Gibbon 2011). Besides, there may exist "semantic" or "linguistically driven" synchrony. It can be defined as the mechanism responsible for producing certain units of speech and gesture in adequate timing so that they can form meanings together. From the viewpoint of perception, the phenomenon is equally complex, but a brief look at the studies on rhythm perception may prove useful here (e.g. B. H. Repp's works). Basic synchrony between speech and gesture can be understood in terms of synchronised kinetic activity although it remains difficult to capture and measure. The notion of entrainment, coupled oscillator model and dynamic system modelling belong to promising approaches.

Nicla Rossini concludes that gestures may be embedded in our everyday interactions because of their "unavoidable nature" due to the ontogenetic properties of the human brain: the frequently mentioned proximity of the Broca area and the motor area of the cerebral cortex cannot be accidental. She finally admits that the hypotheses of Butterworth and Hadar (viewing gesture as a mere epiphenomenon of speech) may be true – but only from the phylogenetic perspective. She suggests that the communicative function of gesture may have evolved just because the presence of gestures was inevitable anyway, and views beats as relics of communicative movements.

In the eighth chapter of the book, Nicla Rossini discloses her views on how the methods of parsing should be adjusted in order to accommodate the gestural component of utterances. She introduces the notion of Audio-visual Communication (AVC) in order to stress the fact that the object of linguistic inquiry should be redefined and go beyond the traditionally accepted boundaries of language. Her scheme of AVC system is convincing, but it is limited to speech and gesture. Moreover, even if one decided to remain within the limits of aural and visual phenomena, a more detailed and precise representation might have been welcome in order to reflect the thoughts that are in the text anyway.

Nicla Rossini makes an excursion towards very traditional morphology and lexical semantics. Perhaps more flexible but, simultaneously, more formalised frameworks would also be inspiring here (e.g. Optimality Theory (Prince, Smolensky 1993)). The paradigm of Natural Linguistics (e.g. Dressler 1990) would probably also be able to accommodate the gestural component of language. The concept of "gesture grammar", while certainly tempting (cf. C. Mueller's team recent project), may raise some problems. As we know, approaches that work on the sentence level may not be appropriate for other levels (cf. the critique of "discourse grammar" or "text grammar").

In her approach to simultaneous perception of speech and gesture, Nicla Rossini follows Massaro's model which reflects the fact that articulatory movements produce both vocal and gestural output. As she admits, however, any structural model enforces certain simplifications. Furthermore, in the studies of multimodal perception, possible cross-modal effects should be taken into account.

Nicla Rossini discusses the issues of gesture morphology extensively and in detail. This is probably another way of showing that the “language of gestures” is not so far removed from the spoken one. Starting with a solid theoretical preparation, she formulates her postulates for the morphology of gesture that are based on the reinterpretation of the parameters which are widely applied in the description of sign languages (size, timing, oscilation, point of articulation and locus).

A separate subchapter is spared for the vividly discussed phenomenon of recursion in language (Hauser et al. 2002, Pinker and Jackendoff 2005, Fitch et al. 2005 as well as the most recent collection of papers that can be found in van den Hulst, ed. 2010). Nicla Rossini points to the fact that some problems in the debate on recursion may be due to its restricted understanding. She also notices that the limitations of human brain do not allow for “true” recursion but rather for recurrence, and refers to Byrne (2003) to show that the behaviour of animals can also be viewed as recursive. She demonstrates that recursion is a property of the gesture system, but also points out that while examples of recursion in the weak sense are common, “stronger” recursion can be found where “not only do gestures completely replace the speech signal, but they are also performed within the syntax”. While explaining the origin and application of the concept of recursion in linguistics, she refers to Levelt's model of speech production (1989), to Krauss' model (Krauss and Hadar 1999) of speech and gesture production, as well as to a more recent and complex model by de Ruiters (2000). She finds, however, that even de Ruiter's model treats the production of gesture and speech as separate (although simultaneous) processes.

In this context, Nicla Rossini elaborates further on her idea of computational AVC parsing and proposes her own model which integrates speech and gesture production. While some of its aspects (including its psychological reality) may be disputable, it is an important contribution and one of the key points of the book. The model accounts for recursion and at least partially for the common processing of speech and gesture, from the stage of conceptualisation to the stage of motor commands. Nicla Rossini shows how multimodal utterances can be parsed in the proposed framework. Examples are illustrated with sequences of movie frames and detailed transcriptions. The model adopted for the description of AVC is demonstrated to be capable of representing complex communicational behaviour and the structural complexities of multimodal utterances.

Closing this stage of discussion, Nicla Rossini redirects readers' attention towards planning and self-orientation processes which are central to the subsequent chapter of her book. There, she steps beyond the communicative perspective and moves back towards inner, mental processes again. The works by Piaget, Luria and Vygotsky are declared here as constituting the frame of reference for the discussion and her own studies. Bloomfield is also briefly mentioned as “the only well known linguist” who devoted some attention to the self-directional aspect of language. Although these historical contributions still remain influential, it might be surprising to find that more recent cognitively-oriented works are omitted. Despite these disputable choices, the background for Rossini's empirical studies is precisely and consistently prepared.

The study itself is based on the widely used map task dialogue procedure, with one person giving and the other following route directions. Here, the maps of the instruction givers and followers differ in some detail and each pair of the participants is separated by a screen which blocks mutual visibility. Much place is devoted to detailed discussion of rich audiovisual material and of the sections contains a full transcript of the session, presented along with sequences of movie frames. As previously, Nicla Rossini proves to be an insightful and meticulous observer. Again, some readers would probably expect more information on the transcription and annotation techniques as well as on the video capture set-up. Nevertheless, the findings are valuable and include the discovery of a new gesture, namely "a palm-down-flap". Nicla Rossini suggests that the gestures that occur in this (non-facing) condition are not invoked from "imaginistic short term memory" but rather from "self-orientation in space and planning". In the interpretation of her findings, she refers again to de Ruiter's (2000) hypothesis that the usage of gestures may be due to the adaptation of behavioural patterns typical of the "default" condition of mutual visibility. Blocked visibility does not block gesturing (Rimé 1982) and some studies report that the number of gestures in the condition of limited mutual visibility may be actualy even higher but the gestures are realised in lower areas of the gesture space (Jarmołowicz-Nowikow & Karpiński 2011). Certainly, it may depend on the communicative situation and on the profiles of the participants themselves.

Summarising her findings, Nicla Rossini points to the fact that the amount of posture shifts and gaze shifts towards the interlocutor was significantly reduced. Still, "gaze tended to focus on the area where the partner is supposed to be while waiting for responses or feedback." She also mentions posture shifts and gestures related to planning. In general, she shares and supports Alibali's view (Alibali et al. 2001) that gestures, independently of their communicative and interactive functions, serve self-regulating and planning functions, being a means of self-orientation and self-organization.

The closing chapter of the book is devoted to available and potential technological applications of non-verbal communication research in human-machine interaction. Besides pointing to some implementations, it also offers the reader a technological perspective on some problems discussed earlier in the book. Many important questions regarding the design of Embodied Conversational Agents [ECAs] and robots are put forward here: how can we make them believable and trustable, and how to equip them with a sort of "communicative intuition"? Nicla Rossini stresses that even apparently small details of behaviour, like synchrony of speech and non-verbal cues, may significantly help in reaching these aims. Gaze directing and other categories of visible behaviour may also contribute. The amount of gestures as well as the proportion of speech to gesturing can also be of importance. One should also consider the influence of the social context on the occurrence and quality of non-verbal behaviour.

Nicla Rossini refers to two basic approaches to the architecture of software agents and robots which are typically *function-based* (Nilsson 1984) or *behaviour-based* (Brooks 1991). She also mentions the efforts towards enriching the behaviour of machines with the emotional component. While the ideas and suggestions of the Author are accurate

and well-thought out, the reader may feel that this section is not as well-grounded in literature as other chapters. A reference to the pioneering works of Rosalind Picard in emotional computing (e.g., Pickard 2000) would have been welcome. Even a relatively old text by J. Bates (1994) on the role of emotionality in believable agents could have been cited here as it gathers some ideas that are still valid and important. For more examples of applications, one could also consults a recent book by Scherer et al. (2010).

Most of Nicla Rossini's comments pertain mostly to two robots (iCube and Nexi) and one virtual agent (GRETA). Some portions of their gestural behaviour are analysed and, as the Author suggests, certainly there is much room for improvement. Later in the chapter, Nicla Rossini proposes her own architecture for “a more natural agent,” with some innovative suggestions. Among them, the statement that “a definite improvement should be observed with a different architecture relying less on Fuzzy Logic and a review of the lexicon for the generation of gestures and expressions” strikes as quite surprising and, as such, would probably require further elaboration. The discussion on the interface design and typical programming approaches is somewhat shallow. It is understandable that going deeper into technological details was not the purpose of the chapter but the problem is that in this respect it offers only slightly more than a word of tempting inspiration.

* * *

The meeting of linguists, gesture researchers, psychologists, sociologists, and others, on a common ground of communication studies still remains relatively superficial. How to come closer together while remembering the roots and achievements of the traditional disciplines? The new ideas proposed in the book under review come mostly from re-interpretations of some existing notions, from importing ideas from one field to the other. Nicla Rossini's answer seems to resolve itself in meticulous analysis and profound understanding of the existing knowledge, and its re-interpretation and verification in the new frames and paradigms. The title of the book suggests that the idea of gestures as a part of language has been always around somewhere and the question is only how to bring it back using adequate methodology and recent technological achievements. Taking short-cuts is tempting and sometimes more efficient, but the approach adopted here is not only honest but also gratifying.

Communication Studies are overwhelmed by technological progress. Researchers have extremely powerful tools at hand but sometimes cannot tame them or happen to be not cautious enough in the interpretation of results. Nicla Rossini points out that in certain contexts it may be still safer and more efficient to remain on the level of external behavioural observations in empirical studies than to base one’s findings solely on the incredible amounts of data coming from functional neuro-imaging studies.

Even though readers will quickly find that Nicla Rossini is more a gesture researcher than anything else, her paths are clear and easy to follow independently of the discipline she touches upon in the text. Her readers may feel well-guided and enjoy rich

but precisely adjusted theoretical background. In some places, psycholinguists or computational linguists may be slightly disappointed by the limited or shallow referencing, a problem typical of multidisciplinary studies.

Nicla Rossini remains under a strong influence of cognitive linguistics and neuroscience but she still finds inspiration in the classical works of structuralists. It is admirable that in most cases she manages to trace important ideas occurring in her work back to their origins. On the other hand, maybe some place could have been sparred for more recent models, approaches and theories.

The most significant contribution of the book resolves in re-defining gesture on the cognitive ground as a prototype category, re-arranging the categorisation of gestures, determining the place of gesture in the process of communication and presenting empirical support for the formulated hypotheses in a series of studies. The discussion on intentionality and the communicative value of gesture not only shows the essence of the former achievements, but also adds some fresh critical thoughts. The investigation in gesture morphology provides means to reach the ultimate goal of the book, but it is also an important independent contribution. The section devoted to the origin of language and the emergence of gesture in infants, including the discussion of the origin of pointing gestures, is also very informative and valuable. The closing chapter bridges research and its potential applications, and may be recommended to those who are reluctant to believe that "humanities" significantly contribute to technological progress.

The empirical studies by Nicla Rossini offer a gist of interesting findings rather than a set of statistically interpretable data. Yet, she certainly knows where to look for support for her hypotheses and realises the limitations of her approach. The data from the congenitally deaf subjects are exceptionally valuable, unique and difficult to extend. In the case of map task dialogues, more material is available and it can be analysed in future, probably even as a part of cross-cultural comparative studies. Empirical studies of communication are extremely tedious, time consuming and technically difficult. Nevertheless, while many case studies are extremely valuable, researchers tend to crave for generalisations and for reproducible experiments.

The book is coherent as a sequence of chapters and the reader is well guided from the point of defining some fundamental notions, to the new theoretical constructs and further, to their experimental verification and potential technological applications. The text is definitely inspiring in terms of possible directions of research and lists of unsolved problems. For those who are new to the multi-modal communication studies, it offers a valuable discussion of most of the fundamental problems. It contains a bunch of new ideas, theoretical formulations and empirical attempts for those who have been a part of the field for some time. Nicla Rossini confirms her multidisciplinary background and shows flexible, wide and interdisciplinary thinking. A rich, multifaceted piece of literature to keep somewhere within the reach of hands when one needs inspiration for new research.

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# STUDIES ON MUSICAL PRACTICE

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# Rethinking Musical Affordances

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**Abstract**

The notion of affordance has been introduced by Gibson (1977, 1979) as the feature of an object or the environment that allows the observer to perform an action, a set of "environmental supports for an organism's intentional activities" (Reybrouck 2005). Studied under very different perspectives, this concept has become a crucial issue not only for the ecological psychology, but also for cognitive sciences, artificial intelligence studies, and philosophy of mind. This variety of approaches has widened the already ambiguous definition originally provided by Gibson, contributing to the development of different standpoints in open contrast with each other (see Zipoli Caiani 2011). During the last two decades, moreover, many researchers tried to extend the notion also to musical experience, aiming to draw a coherent theory of musical affordances (e.g. Clarke 2005; Nussbaum 2007; Krueger 2011a; 2011b). In this paper, we will argue for a particular concept of musical affordances, that is, as we see it, one narrower and less ambiguous in scope and more closely related to its original. Taking the discovery of canonical neurons as our starting point, we will (i) introduce the general notion of affordance, (ii) discuss some significant contributions in this area of research, mostly focusing on *musical affordances* and (iii) propose a motor-based interpretation of musical affordances.

**Keywords:** musical affordances; canonical neurons; motor intentionality; musical understanding; embodied music cognition.

**Visual-motor affordances and the power of action**

In the context of motor action research, there is a common agreement[52] regarding a basic understanding of the notion of *affordance*, usually intended in terms of a set of possible motor actions evoked by the intrinsic properties of an object or the environment. According to Gibson (1979), indeed, the visual perception of an object leads to an *automatic selection* of those of its intrinsic properties that support the individual's physical interactions with it. These properties, however, are not only abstract, physical or geometrical features, but „incarnate the practical opportunities that the object offers to the organism which perceives it" (Rizzolatti & Sinigaglia 2006: 34). When, for example, an object like a cup is located in the subject's *peripersonal space* (Costantini et al. 2011), it can represent the goal of the individual's grasping act and this subject/object interaction is codified through an *affordance*. A key role for this current understanding is played by the neuroscientific evidence of *canonical neurons* (Rizzolatti et al. 1988; Rizzolatti, Fadiga, 1998; Rizzolatti et al. 2000; Raos, et al. 2006; Umiltà et al. 2007; Rizzolatti & Sinigaglia 2008), a set of neurons which discharges when an individual simply observes an object without performing any movement, as well as when he/she grasps that object. The selectivity of these neurons, appreciable through the congruence between the codified motor features and objects' visual properties, advocates for their pivotal role in the process of transforming the visual information of objects into the appropriate motor acts (Jeannerod et al. 1995). The discovery of these "visuo-motor" neurons shows how an object can *afford*, according to the subject's motor expertise, a set of possible actions that can be performed thereon, relying on a *sub-cognitive* form of understanding, not linked to mental representation or higher mental faculties. Gallese (2009), in describing the canonical neurons, states that

> *[t]he most interesting aspect [...] is the fact that in a considerable percentage of neurons, a congruence is observed between the response during the execution of a specific type of grip, and the visual response to objects that, although differing in shape, nevertheless all "afford" the same type of grip that excites the neuron when executed [...].The intrinsic relational functional architecture of primates' motor system likely scaffold the development of more abstract and detached forms of intentionality, as those characterizing thought in our species* (Gallese 2009: 489-490).

The basis of the intentional relationship between an organism and the environment, therefore, can be reconsidered in terms of how the *motor possibilities* (Poincaré 1908; 1913) of the subject's body can interact with the surrounding objects, advocating for a *motor approach* to intentionality (Sinigaglia 2008). This form of intentionality doesn't require any high-level, metacognitive ability as "the intentional character, the 'aboutness' of the representational format of our mind could be deeply rooted in the intrinsic relational character of body action". (Gallese 2009: 489). In other terms, cognition can be seen as *embodied*, namely, it arises from the bodily interactions with the environment, depending on "the kinds of experiences that come from having a body with particular perceptual and motor capacities that are inseparably linked and that together form the matrix within which memory, emotion, language, and all other as-

[52] Despite the opposition of Fodor, Pylyshyn (1988).

pects of life are meshed" (Thelen 2001: XX). However, notwithstanding the importance of this new *embodied* paradigm (see also Merleau-Ponty 1945; Varela et al. 1991; Gallese 2011), the current ecological account for *affordances* inspired by Gibson seems to be characterized by epistemological vagueness. Indeed, one of the assumptions at the basis of this perspective is that *every set of behaviours ascribable in terms of a unitary action has the right to be described as an act potentially evoked by its related affordance*. In order to clarify this statement we can think about the possible analogy[53] between these two different conditions: (i) a cup affords the act of grasping and (ii) the sight of a movie-trailer affords the act of going to the cinema. From an ecological standpoint, those actions (grasping and going to the movies) are both homogeneous and describable as unitary. But the *inferential* feature of the second one makes it unsuitable: otherwise, every *a-posteriori* correlation between events and actions should be considered *affordative*. To get an idea of the variety of phenomena included under the concept of affordance we can indeed have a look at this excerpt from E.J. Gibson (1982):

> *Air, the medium we live in, affords breathing. It affords walking or driving through, and seeing through, at least in communities that are free of smog* (Gibson 1982: 55).

This example presents some incongruences with the necessary setting of a scientific research on the notion of affordance. Even if sometimes the air is breathable, this does not imply that breathing is a *goal-directed act*, intentionally linked with the portion of air considered as an *intentional object* (Menin 2011: 12). We cannot indeed integrate every possible interaction between the two poles of action (subject and object) into a genuine reflection on the issue of affordances without applying a rough *objectivation* (see Husserl 1901; 1907; 1936), hence depriving the notion from any phenomenological characterization. The relationship between a subject and the air that the agent is breathing cannot be described as cognitive, or as constituent of any intentional relationship. It can only be described in terms of physical-chemical events[54]. But if some events do have affordances, we should investigate the *object* of these, and also the role of the relevant *subject*, considering the coding of perceptual information with regards to the *motor knowledge* of the perceiver, as the aforementioned literature on canonical neurons clearly states. In the realm of music, the concept of affordance – far from being successfully addressed yet – has been investigated during the last few years in order to clarify the subject/object relationship characterizing musical experience, with a particular emphasis on the surrounding, sound-made environment. What does music

[53] This example has been presented firstly in Menin 2011.

[54] With regard to this point, we claim that the original Gibsonian ambiguity needs to be explicitly overcome, if we are to develop an experimentally expendable notion of "affordance". If it is true that one of the most fascinating features of affordances resides in its being direct and someway "automatic", thus not requiring any cognitive or attentional mediation, we cannot go, on the basis of this not-needed mediation, to the extent of saying that no intentional sensorimotor relationship is required for an affordance to take place. The notion of "basic motor act" introduced in the recent literature (e.g. Rizzolatti, Sinigaglia 2008) clearly highlights goal-directedness as the central feature of sensorimotor interactions.

*afford*? How can a subject *make sense* of a series of sounds intended as musical? Which modalities are involved in this sense-giving process? The current body of knowledge in this area is extremely broad, and besides the obvious interest of music researchers, it does involve scholars from different backgrounds. Hence, the following section presents some of the trends in the current debate, providing incentive for further discussions as well.

**Musical affordances in current literature**

In this section we try to take a closer look at the notion of musical affordances, focusing on the works by Clarke (2005), Nussbaum (2007), Krueger (2011a; 2011b), and Reybrouk (2005), probably the most influential and well-known studies on this specific and problematic issue.

*Ways of Listening* by Eric F. Clarke (2005) aims to face the problem of musical understanding through a Gibsonian interpretation in order to overcome the interrelated dichotomies of subjects/objects, passive/active listening and autonomy/heteronomy of musical experience. However, as we try to show, the epistemological caution necessary in dealing with such a complex body of problems is not always fully addressed. The author firstly suggests that musical structure is not a construction within the mind, but, rather, something inherent in the environment, with which our auditory system would *resonate*. Secondly, Clarke describes the dynamics of these resonances, introducing the key notion of *affordance* (36) without rigorously discussing the problematic aspects implicit in the controversial Gibsonian concept. Two of the most quoted excerpts of Clarke's book help highlighting the consequences of this option, elucidating what kind of *phenomenic topography* is included in his notion of musical affordance.

> *Music affords dancing, worship, co-ordinated working, persuasion, emotional catharsis, marching, foot-tapping, and a myriad other activities of a perfectly tangible kind (Clarke 2005: 38).*

> *In the specific contexts of musical hermeneutics, musical material can be conceived as affording certain kinds of interpretation and not others [...]. Interpretation is also action – the speaking, writing, gesturing and grimacing in which interpretation is manifest [...]. The recapitulation of the first movement of Beethoven's Ninth Symphony affords writing (or speaking) about in terms of murderous sexual rage, or the heavens on fire. Interpretative writing and speaking are forms of action* (204).

Looking at the examples provided in the first list, allegedly belonging to the class *of perfectly tangible acts* that can be afforded by music, we can notice the same incongruities we have previously reported about Gibson's standard account: mainly, the lack of any consistency in the conceptual and phenomenological structure of the provided concept of affordance. It is indeed problematic, for instance, to assume that music is in some way the intentional object of all this variety of behaviours. Moreover, the second excerpt shows Clarke's bias to equate *percepts* and *concepts* (Nonken 2008), objectify-

ing the class of musical acts, intended only from an *a-posteriori* point of view. From this standpoint, there is no substantial difference between the way music affords foot-tapping and the way it affords writing pieces of musical criticism. With regard to this point, someone could argue that Clarke is not actually confusing percepts with concepts, but rather trying to unify our understanding of perceptual and cognitive aspects of musical experience. Our objection, however, remains valid. Indeed, if this holistic stance is one of Clarke's main concerns, it is quite clear that he treats this position as a claim and as a presupposition at the same time, hence giving rise to a circular argument. In conclusion, the meaning of *to afford* which emerges from Clarke's *Ways of Listening* does not exceed the colloquial concept of *to evoke*, or *to elicit*, showing that musical experience is, in a vague sense, evocative. The book's main conceptual contribution, besides its remarkable general introduction to ecological psychology and musical understanding, indeed, could be summed up as a rather direct application of a standard ecological account to the study of musical experience, broadly intended.

More ambitious is the theoretical proposal of Charles Nussbaum (2007) who actually aims to unravel the *riddle of musical experience* (2007: XI) in light of its *representational* nature. To understand the problem he is dealing with, we can first have a look at this citation:

> *It takes only a small amount of perspicacity to realize that music is remarkable, indeed an astounding, phenomenon. The emergence of human musical experience from the audition of organized tones remains deeply puzzling, truly "a riddle wrapped in a mystery inside an enigma," a riddle, moreover, of very long standing* (Nussbaum 2007: XI).

In our opinion, this statement is highly misleading, especially with regards to the set of problems involved in musical understanding. The apparently naïve implication that musical experience builds up from the auditory perception of analytically isolable basic elements is not only unwarranted, but it also stands in open contradiction with the most fruitful studies of action-related aspects of perception (Rizzolatti & Sinigaglia 2008: 50-52).

> *If we are to unravel the riddle of musical experience, we need a thread on which to tug. Construing music as representational, as a symbolic system that carries extramusical content, I hope to persuade you, exposes such a thread (1).*

As this quotation clearly shows, the enigmatic position of the musical problem and the representative option proposed in the book are closely interrelated. This epistemological situation does present analogies with the *post-Cartesian* dilemma concerning the emergence of a thinking substance from an extended body: in both cases, the hypostatisation of the starting dichotomy necessitates an *ad-hoc* solution in order to mediate between the two substantialised realms. It is worth to note, with regard to this topic, that the notion of *affordance* is usually connected with a strong *anti-dichotomist* position, as Gibson (1976: 129) first pointed out. In Nussbaum's work, however, an affordance is conversely meant as a *mediation tool*, functional to the perpetuation of a radically dualistic stance. Indeed, according to this scenario, musical affordances are considered to be conceptual bridges between a *low-level* dimension of musical experi-

ence, conceived in terms of a meaningless isomorphic transcription on a pitch-time diagram of the stimulus, and an idealised *high-level* dimension that includes every aspect of musical experience, broadly meant. Even without discussing the proposed theoretical framework, we can rule out Nussbaum's contribution as irrelevant to our aims, as he considers affordances to be a *cognitive form of understanding*, linked to mental representation or higher mental faculties, thus in contrast with any position developed from an ecological standpoint.

Another approach aimed at developing a sustainable notion of musical affordance is provided by the recent works of Joel W. Krueger (2011a; 2011b). The author claims that "an affordance is a relational property of the animal's environment perceived by that animal as having a functional significance for that animal" (Krueger 2011a: 4). He also states that music is perceived from birth as an *affordance-laden* structure that *affords* a sonic world (Krueger 2011a: 1) that further *affords* possibilities for, among other things, (1) emotional regulation and (2) social coordination. From an epistemological standpoint, however, this proliferation of *affordative levels* seems – at least – suspicious, and gives rise to three questions:

(i) Does music afford a sonic world in the *same sense* as this sonic world affords emotional regulation?

(ii) What would be the relevance of such a claim in the study of the *intentional relationship* between a subject and a musical object?

(iii) How should we describe the *animal-environment* relationship if music affords a sonic world that further affords acts of any type?

In trying to answer these questions, we find out that the *mediation* offered by the notion of "sonic world", besides being unnecessary, if coherently implemented would substantially compromise the *direct character* of the concept of affordance, explicitly acknowledged by Krueger (2011a: 7). Moreover, the mainly homeostatic (emotional and social-related) conception of the activities elicited by musical experience seems to divert from the embodied approach that the author claims to embrace. Besides these issues, the crucial point of Krueger's argument is the characterization of musical space, developed in league with the tradition of *spatiality-for-action*(e.g. Poincarè 1908; for a TMS study, see Cordellicchio et al. 2011) mainly discussed in the *visuo-motor* domain (Rizzolatti et al. 1997; Sakata et al. 1997). In his paper *Doing things with music* (2011a), Krueger faces the problem of *musical space* from a purely ecological standpoint, defining musical environments (or sonic worlds) as *comfortable* or *stressful*, whereas in his other work, *Enacting musical content* (2011b), he does contrast inner (or structural) and outer musical space. *Outer musical space* is here identified with the localisation of the occasional sound source, whereas *inner musical space* is described as "the piece's inner syntactical structure established by the way that constituent components (e.g. tones, rhythmic progressions, etc.) go together, lending the musical piece its sonic coherence as a composed object" (2011b). Since outer space is meant as *non-musical* (related to the localisation of musical stimulus), we would assume that *inner space* and *sonic world* are interchangeable notions, defining from different standpoints the same musical *space for action*. But this assimilation is hardly accomplisha-

ble. The notion of *sonic world* arises indeed from a standard ecological standpoint, while the concept of *inner space* descends from a – at least to our knowledge – misunderstood embodied approach, in which the musical surface is identified with a Cartesian diagram with time and pitch as axes, embracing *de facto* the "pharmaceutical model" (Sloboda 2005: 319) of musical understanding which Krueger explicitly refuses (Krueger 2011a: 3). As a result of this irreducible duality of approaches, the notion of *affordance* connected to the concept of sonic world seems extremely relational, as it is associated with *every kind* of activity that music could possibly elicit, whereas the one connected to the concept of inner space is conceived from a completely *objectivistic* point of view. What both of these concepts are missing, in our opinion, is the *intentional character* needed to make musical affordances a *phenomenologically* relevant notion.

A better awareness of the range of issues implicit in the *enactive* approach to human musicality is shown by Mark Reybrouck (2005),who addresses in his work for an embodied characterization of musical experience. The author, using syncretic integration of different perspectives (from classic pragmatism to cognitive economy), aims to overcome the prevailing *objectivism* in the realm of musical understanding, by applying the key notion of *sensorimotor coupling* (which defines the perception-action loop) to the analysis of this topic. His strategy consists in defining the two domains of (i)*musical experience* and (ii) *motor cognition*, showing how they can be connected in such a coupling. "Musical experience", he claims, "is not basically different from an auditory experience at large. It is continuous with the natural experience or experience proper (see Dewey 1934) with a difference in degree rather than in quality" (Reybrouck 2005: 9). This equivalence between *experience proper* and *aesthetically connoted perception*justifiesthe application of a general concept as sensorimotor coupling to the peculiar realm of musical understanding, considering the importance of action in acoustic perception (Kohler et al. 2002). On the other hand, the processes of motor cognition are introduced through the discussion of the pivotal notion of 'image schemata', defined as "recurring, dynamic pattern[s] of our perceptual interactions and motor programs that give[s] coherence and structure to our experience" (Johnson 1987: XIV). The two classes of image schemata presented as the most relevant to the study of musical experience are the "container schema" and the "source-path-goal schema". While the *container schema* is "a pervasive mode of understanding everyday experiences in terms of 'in' and 'out'" (Johnson 1987: 12), the *source-path-goal schema* represents the feature of being oriented towards a goal in a continuous, temporally extended path (Johnson 1987: 12).The author then illustrates the musical analogies of these image schemata, introducing the concept of "musical affordance":

> *There are, as yet, many possibilities that stress the "action aspect" of dealing with music. I mention five of them: (i) the sound producing actions proper, (ii) the effects of these actions, (iii) the possibility of imagining the sonorous unfolding as a kind of movement through time, (iv) the mental simulation of this movement in terms of bodily based image schemata and (v) the movements which can be possibly induced by the sounds* (Johnson 1987: 24).

In discussing Reybrouck's contribution to the topic of our enquiry, we have to firstly acknowledge its rare pertinence to the range of problems related to embodied stances and the study of musical experience. Furthermore, Reybrouck's work clearly exceeds the limited topic of musical affordances, so our observations are not meant to refute his general proposal. That being said, we also have to express some concerns about the robustness of the conceptual framework developed, with particular regard to the topic discussed herein. In particular, the choice of defining *separately* the two realms of musical experience and motility seems weak, considering that Reybrouck himself acknowledges *non-objectivism* as one out of the two main claims defining an embodied approach (10).With regard to this topic, we propose a different option in the next paragraph. Moreover, the two definitions proposed appear to be questionable: indeed, as Clarke (2006: 1) pointed out, one of the dichotomies that a notion of musical affordance could help to overcome is that which opposes *autonomy* and *etheronomy* of musical experience, but the *full integration* of musical experience into mere acoustic experience proposed by Reybrouck seems to neglect this aspect. On the other hand, the notion of *image schema* and the *actual schemata* proposed could not be accepted by many proponents of embodied cognition (see Rizzolatti & Sinigaglia 2008), as these concepts seem to individuate a class of mental schemata, rather than a truly embodied kind of sense-making. This problem emerges clearly from the analysis of the notion of "goal" provided by the author: indeed, the kind of *goal-directedness* described by Reybrouck cannot be assimilated into the classic *motor grounded* one (see, for instance, Murata et al. 1997; 2000, for a focus on acts such as *prehension*). However, it does present similarities with the *teleological stance theory* advocated by the *Theory-Theory* supporters such as Csibra and Gergely (2003), where the understanding of the others' intentions and goals can only be possible from an *external*, ascribing, perspective (see also Dennett 1987).

## Musical affordances through the motor perspective

In order to overcome the epistemological inaccuracies just portrayed, we propose, in the realm of music, to consider a musical affordance to constitute a property of the intentional relationship between musical subjects and objects. Indeed, the scenario described at first advocates for a new phenomenological characterization (Pelinski 2005; Schiavio 2012), where the intentional understanding constitutes the (musical) object of perception in regard to the motor repertoire (Rizzolatti et al. 1988; 2000; Buccino et al. 2004; Gangitano et al. 2004; Calvo-Merino et al. 2005; 2006) of the listener (Molnar-Szacaks, Overy 2006; Overy, Molnar-Szacaks 2009; Bangert et al. 2006; Lahav et al. 2007). This pivotal role of the body and its motor knowledge has been addressed in the phenomenological equivalence between perceiving and giving sense to the percept (Merleau-Ponty 1945), where

> *my body is geared to the world when my perception offers me a spectacle as varied and as clearly articulated as possible, and when my motor intentions, as they unfold, receive from the world the responses they anticipate. This maximum distinctness in perception and action defines a perceptual ground, a basis of my life, a general milieu for the coexistence of my body and the world* (Merleau-Ponty [1945] 1962 : 250).

Rather than postulating high-level cognitive abilities to account for understanding, the phenomenological explanation provided requires only my body, conceived as "the meaningful core which behaves like a general function" (46). A skilled guitarist might be unable to say where to put her/his finger to perform a solo, but s/he can use the *motor knowledge* of the fingers to reconstruct the actual set of notes played, by just putting the hand on the strings. We believe that this sensory-motor process not only represents the basis of musical understanding, but it can also shed light on the notion of musical affordance, relying on a sub-cognitive, pre-linguistic, intrinsically motor form of intentionality. In particular, the studies on the *ontogenetic basis of musicality* can provide some evidences of a non-cognitive characterization of the subject-object relationship of musical experience. Indeed, for instance, despite avoiding an explicit reference to the notion of affordances, the work of the French psychologist Francoise Delalande (2009) provides a discussion with a genuine and motor-based approach, in league with the *anti-mentalistic* stance at the basis of the recent thematic rediscovery of affordances. Trying to make sense of the ontogenetic constitution of a musical context from the sensory-motor exploration in infants, the author focuses on the dynamics of these sound-based objects' discoveries (see also Perone et al. 2008; Schiavio & Menin 2011). Indeed, when a child explores the environment and produces sounds, these sounds may surprise her/him, leading the infant to hear and produce them again, maybe introducing some variations (see Imberty 1983). This process of repetition with slight changes can be seen as the development of a sound discovery, whose characterization, rather than cognitive, is represented by the advance of the *sensory-motor* modalities of interaction with the object (Delalande 2009: 300). This ontogenetic, sense-giving and motor-based process reflects the constitution of the intentional relationship of musical subjects and objects, the only plausible scenario where musical affordances can be observed in infancy. This account for musical intentionality, hence, leads the discussion on the analysis of musical-related acts, chains of actions with a musical goal-directedness (*teleomusical acts*[55]) constituting the musicians' motor knowledge. Indeed, a correct characterization of these acts cannot be limited to the executive side of motility, because what allows the possibility of understanding a musical object in terms of its evoked acts (Overy & Molnar-Szakacs 2009: 492) is the *goal* rather than the actual performed movement (Kohler et al., 2002; Ticini et al., 2011).

[55] The term *teleomusical acts* has been coined by Schiavio & Menin (2011).

**Conclusions**

The scientific contributions we briefly reviewed were supposed to clarify the *conceptual topography* around the notion of affordance, dealing with the specific sets of events related to human musicality. Unfortunately, in our humble opinion, none of the above mentioned authors seems to have shed light on the nature of *musical affordances*. We therefore argued against the fitness of the presented theoretical frameworks to consistently make sense of the number of problems implicit in this notion by proposing an *embodied approach* that radically diverges from the standard accounts, considering musical objects as *entities constituted within the intentional motor-based relation that defines a musical context* (see also Schavio 2012). If *musical affordances* are properties of *the intentional relationship* between a musical object and a musical subject, as we have assumed, the significant consequences we can draw from this theoretical paradigm may have repercussions in the *ontogenetic studies* on human musicality, as well as addressing the need for new models of *musical learning*. The *situatedness* of musical affordances, indeed, as it is rooted in action-understanding processes rather than in mental forms of the transmission of knowledge, cannot be decontextualized from the subjective goals (Barab & Roth 2006), advocating for a teaching approach mostly based on the development of the individual's (musical, motor-expressive) *intentions* (Young 2004b) through a strategic study of the relevant (musical-directed) *motivations* (Merleau-Ponty 1945). As we have seen in the preview paragraph, in fact, what motivates children in exploring the environment and producing sounds primarily emerges as a *spontaneous* behaviour completely immersed in a bodily-based intentionality. Spontaneous exploratory activities are firstly focused on directing attention outward toward events, objects and their properties, and the layout of the environment (Gibson 1988). Through the manual exploration of objects, infants develop their motor acts and familiarize themselves with *musical structures* such as repetition and variation. The knowledge acquired from those discoveries will make the children able to construct a *musical context* as well as a basic vocabulary of *musical-directed acts* (simple sound-oriented actions such as *plunging*, *hitting*, *scratching* (Delalande 2009) can be seen as examples). From this standpoint, the notion of *musical affordance*, correlatively with the key concept of music-directed (or *teleomusical*) acts, becomes then crucial for understanding the ontogenetically originary elements of music experience, and the processes that lead to their development to a fully constituted musical – embodied – intentionality.

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# Ecological introduction to improvised counterpoint

If someone wanted to be malicious and point out a word that reappears in AVANT editorial comments particularly (or even irritatingly) often, the word they would likely choose is "counterpoint." Yet, even were the word to be taken at its most literal, as in the Latin *punctus contra punctum*,[56] it is difficult to find a better description for the basic rule guiding the journal structure. Two forms equally (but differently) adequate and in their ways, equally authentic. What is created between them, however, is entirely up to the Reader. So it is in this case, where it is not us but the Readers who can apply the music researchers' skills to those of musicians themselves.

Having invited two eminent representatives of improvised avant-garde, the brothers Marcin and Bartłomiej Oleś, our interviewer let himself conduct a (perhaps pretentious) experiment: he asked the two brothers (separately) mostly identical questions. The result did not resemble that of a simple poll. The musicians' personalities defined the form and direction of the answers. One cannot force the way into their musical work and thought, but can only ever be allowed to enter, in a process both peaceful and thoughtful.

Let us finally follow with a short biographical note of our guests. The Oleś Brothers: Marcin (Double Bass) and Bartłomiej Brat (Drums). Born in 1973, composers and jazzmen as well as authors of film and theatrical scores. They have collaborated not only with each other, but also with a lineup of true stars of jazz, such as Theo Jörgensmann, David Murray, Chris Speed and Eric Friedlander. The first album they recorded together is *Mr. Nobody*, which was published in 1999 by their group Custom Trio, which included also the saxophone player Krzysztof Kapel. Since then, they have performed at a number of international festivals and collaborated on more than ten albums, among others under the name of Marcin & Bartłomiej Brat Oleś Duo and as Oleś | Jörgensmann | Oleś. They are responsible for soundtracks for four film and many theatrical plays, staged by Teatr Polski in Wrocław, Teatr Narodowy in Warsaw, Teatr im. J. Słowackiego in Cracow as well as Teatr Telewizji (Polish Television Theatre); their compositions could be heard during the performances of plays by such Polish playwrights as Grażyna Trela and Marcin Wrona (*Pasożyt* – Eng. *Parasite*), Daniel Odija (*Tartak* – Eng. *Sawmill*) as well as in contemporary stagings of Shakespeare's *Othello* or Ibsen's *Nora*. Their newest albums include 2010 *Other voices, other scenes*, containing aforementioned film and theatre compositions, and 2011 *De Profundis*, co-recorded with Andrzej Przybielski. Both of them were published by Fenommedia Records, a recording studio in whose establishing Oleś brothers participated in 2005, together with the Fenommedia studio. Additional information can be found on the brothers' website: *http://www.oles-oles.com*

prepared by Nelly Strehlau
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[56] Note against note.


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# Music is You
## Interview with Marcin Oleś[57]

Witold Wachowski

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translated by Katarzyna Cheromińska

**Many words (of varying quality) have been said about music. Have you ever been annoyed by such questions as: "What is jazz?", "What do you feel when you play?", "What is the spiritual dimension of improvisation? – or maybe this hasn't yet affected you that much?**

I have been asking myself these questions for as long as I can remember and I keep doing it constantly, even though they annoy me, because I'm unable to answer them. I think that if the average musician's nature was more reflective, we would be dealing with different music, that is, we would be surrounded by different music (for in the times we live in people are surrounded by music, which is historically quite unusual). The thing about music, as one of the arts, is that it can change our disposition or broadly understood mood. It is a great responsibility, but also a possibility for manipulation (personally, I regard music as the art most exploited for the purpose of manipulating people, followed by photography, movie, theatre…) Among the questions you have asked, there is only one of a different kind, because what you feel when you play is irrelevant for the perceived spiritual dimension of music, and all the more for the answer to the question what jazz, music or improvisation is.

**I'll be more specific: I took "what do you feel when you play?" from among the questions which musicians tend to answer: "Dude, it is mysticism," "It is a connection with the Absolute," "I leave my body," etc. The boundary between the conscious and the intuitive in music – do you think it's worth researching at all?**

This question supports the idea of the spiritual aspect of music, which in fact can, and should, be challenged. For, assuming that we are dealing with a musician, thus, a person who practises playing an instrument almost daily (this is an aspect we don't have in mind asking such questions), we would have to ask if this facet appears also in this daily routine. And if not, then maybe the *connection with the Absolute*, or the *mysticism* you've just mentioned are merely illusions accompanying public performances, thus, the illusory force/aspect of music? I'm not trying to say that such feelings cannot appear – I have frequently experienced them myself, so I know that a *spiritual elation*

[57] ***http://oles-oles.com/mArco***

doesn't have to be spiritual on the recording, and the other way round: something which seemed to be inferior turns out to be spiritually elating to the listener. Try going to the philharmonic and looking at the musicians: do they all seem to be equally *spiritually elated*? Besides, the boundary between the conscious and the intuitive is a process rather than a state, which is why it can't be easily defined. What I mean is that without the conscious we cannot reach for the intuitive – let's imagine a musician who hears a phrase, but is unable to transfer it onto the instrument, or the other way round - a musician who is able to play any phrase, but none comes to his mind, so he plays those clichés. Well, technical skills can get in the way instead of helping just as their absence, and intuition, although it's a seal of every artist's authenticity, evades being methodically captured.

**Are you one of these artists who get nervous because of reviews? Is it a question of personality – or of getting used to it, keeping yourself between the enthusiasm of the "fans" (who appear not only in rock music) and the precision of music critics?**

Increasingly, reviews are the opinions of people who have decided to review, share their attitude to what they have heard, or to put it bluntly - to make public their sense of good taste or lack thereof. I don't take musical criticism like that, I don't want to be reviewed like that and this shouldn't shape public opinion. But it is like that and there is no way to fight it, although it's possible to learn to resist it. The paradox is that it is increasingly difficult to review on merits, and at the same time more and more people take part in this discourse, which is so elusive both in terms of intellectual concepts, and of intuitive judgment. Besides, fewer and fewer people are able to impart a wider view and increasingly frequently we have to deal with the specialists in the field of a given genre, which is absurd inasmuch that the history of the given "genre" is sometimes 20 years.

**If not the reviewers, then maybe scientists would make better verifiers? I will ask a question most artists find annoying: would you let your brain be scanned as a part of neurological studies on meditation?**

A good reviewer is probably someone who has a very good theoretical knowledge to which he/she ought to resort, as scientists do. Without it, it is impossible to show the continuity of music, point out the instances of imitation and epigonism, but also bring out the substantive, strictly musical issues. Brain scanning will not help here, as it is unable to describe a piece of music, it can only indicate what parts of the brain take part in its creation. This kind of research will not cause any revival of criticism, but it can explain the mechanisms by which our bodies function, and maybe in the future it will prove useful for the development of the individual and in everyday life.

**You are an author of a blog where you present interesting and structured reflections on music and its background. Once, looking through it, I thought: "Well, I'm jealous, some of these remarks could be expanded upon and appear in AVANT." At the beginning of March you posted an entry concerning the ethics of an improviser... Without going into self-evaluation - could you tell me if you regard extended self-reflection and knowledge as necessary elements of the musician's maturity and professionalism? On the other hand, there is an opinion that a good musician is some kind of a joyful performing beast, operating with hands and heart, to whom thinking can be simply harmful.**

This question is similar in its nature to that from the beginning of our interview; nonetheless, it emphasises a slightly different aspect. For while the musical, aesthetic and technical consciousness are, in my view, fundamental (I mean all of them, not one of them treated selectively), self-reflection is an additional, and a not-at-all essential, quality. You can unreflectively become an esteemed and popular artist, but I am not sure if you would also be an exceptional one. This is because self-reflection is the search for your own self, your place and role, which often gets in the way of staying naive. While musicians can be joyful beasts, I would rather opt for alpha individuals who can present values to others and set the directions.

**To what extent was your and your brother's way to professionalism professional, strewn with diplomas? Here comes again the question of "professionalism" and "education" in the context of jazz and music of jazz-like provenance.**

My musical adventure consists rather in transposing family traditions, absorbing, in childhood, sounds played live and being an apprentice of the masters. In this sense I can say that my way to professionalism was professional, but rather in the historical, pre-academic sense of this word. My official musical education ended after four years, excluding the musical studies at the pedagogical faculty. The music secondary school which I started to attend disappointed me greatly, so I quit it and decided to learn on my own. I am by no means convinced that it is the most appropriate form of education; similarly, I think that it is difficult to develop harmonically within the treadmill of the music education system. Schools put an emphasis on preparing the musicians for working at philharmonics, or – in the case of the most talented - for solo careers. Self-teaching, on the other hand, imposes a narrow specialisation, which is also disadvantageous. I think that being a musician should be considered as constant, incessant development and this is what schools should prepare their students for.

**On the Internet, there is a pleasant video of you, Bartłomiej and Mikołaj Trzaska performing "portably" on the street. On the other hand - you knock out your audience with such a project as "Contemporary Quartet". Would you like to maintain this wide artistic breath, from a contemporary troubadour to the Warsaw Autumn atmosphere? Or maybe the right course of the development is from the former towards the latter, and someone who has been permanently introduced to the musical high society loses the authenticity of a street musician?**

What for me is the most beautiful in music is its unlimited richness, not just a selected one of its aspects - although, of course, as a musician I explore this richness through selected aspects. From my perspective, a wide artistic breath guarantees development and retaining freshness, provided, of course, that there is the "inhale-exhale" awareness, namely, the awareness of that from which you draw and of that which can stem from it. I think everyone has their own story, but it is worth remembering that until the 20th century being a musician and a busker were two sides of the same coin. It was only Arnold Schönberg who called for *abandoning the futile attempts of drawing attention to oneself and stopping dreaming of mutual happiness of the audience and the musician*. American jazzmen, in turn, brought this buskery back into favour, and, as a performing musician, I am primarily interested in a consolidation of these, as it could seem, contrary approaches, which, actually, should never be separated.

**Philharmonical background of the "Contemporary Quartet", musical precision on "Mikro Muzik"... would you consider playing Webern? Or if not something by him - it is just my subjective reference - then maybe something by one of the other punctual, terribly meticulous composers -watchmakers, operating on a microscale and on the verge of silence. Would you take such or similar projects into consideration?**

It has been ten years since these projects, and I haven't played anything by Webern in this time, although I greatly appreciate his music. On the other hand, many fragments of music I have played took place on the verge of silence, and although they were not punctual by definition, they were in some sense inspired by punctualism. But what I observe goes beyond the music, and even though I would like to perform various types of music, both Bach and Webern, my employment prevents me from doing in the near future.

**But at the same time there is the temptation of a jazz rebellion, Broetzmann-like powers, liberated improvisation and romantic, rebellious faith in world-changing art. To what extent do you identify yourself with these ideas? I'm not expecting a black-or-white answer; I'm rather trying to bring out more than one layer of your musical personality.**

In my view, the time of rebellion is over and in our times these rebellions are, in a way, melancholic journeys inspired by the past and by the values which may have shaped the new world and after which we long, but which, at the same time, have already served their historical purpose. This longing prevents us from reflecting on the present and its possible contribution: the musicians, lingering in the past, dress up as the old-time masters as if they were theatre actors, enact their roles, and we - the spectators - are being subjected to an illusion. The difference is that in the theatre the illusion is conventional, while in music it tends to be forgotten. I am not comfortable with this movement, I challenge the point of its existence in our times and I cannot appreciate its value, just as I find remaining in the jazz mainstream pointless - moreover, I equate these two phenomena.

**The three forbidden questions would be: what is the essence of music, jazz, improvisation. Let me tease you a bit: could you answer any of these questions by negation? That is, for example: what improvisation definitely isn't, although it is mistakenly described as such.**

In my opinion, the questions you named as forbidden should be considered basic for anyone who wants to present their music to others. For it is much easier to answer a question of what music is not, than what it is, what I want it to be and what it should be in the context of who I am in the times it has come for me to live in. Certainly, improvisation is not something ready-made, but it also isn't something random (it's as if you were ready for something that is going to happen to you, but you didn't know exactly what it is). Improvisation is not something you can be indifferent to, but you also cannot become attached to it. Improvisation is not something that comes reluctantly, but it’s also not something easy or unreflective. Improvisation is not superficial, but also not pretentious. Improvisation is not unwitting, but it is also not fully subordinate, or even conscious. Neither is improvisation a display of skills, nor is it possible without having them. Like music, it isn't conventional, but it is impossible without any conventions. It is not a science, but it cannot be achieved without science. It is not only intuition, but  it is unattainable without intuition. And so on...

**photos: © Marta Eloy Cichocka**

**And what exactly is new music or a "novelty in music"? Are the Oleś brothers' albums new music?**

**Are Trane's recording already "old" music?**

In my view, new music is everlasting. It is able to capture the ideas of the period and its most vibrant elements, so I think that the so-called new music never ages - but only provided that it contributes something important to the musical discourse. That's why the music of artists such as Coltrane always retains an aspect of freshness, which cannot be said about the music of the imitators and followers of the aforementioned Brötzmann. The attempts at transferring phenomena and emotions over time are like commercial photography, which draws from the great artists' works or decorative painting, which can draw on the style of Jackson Pollock, but nothing more. There is no added value or an air of authenticity, at best, there is only fascination. Of course we could cling to the statement that the best music is the sixties' jazz or European free improv, and so on, but it leads to the wilderness of epigonism and mindless mimicry. Most frequently, the music which stems from fascination can at most impress us with its technicality or stimulate our ego, but the more fascination there is in it, the less authentic search for value and personal input can be found.

**What do you think about the following, supposedly most popular procedure of practising one's skills of improvisation: [1] analyzing harmony, [2] selecting "scales" and chord sounds, [3] reorganizing "licks", that is, the practised phrases or their fragments, with reference to the rhythm used, and finally: [4] constructing a form that creates tension.**

I'm not sure if it is the most popular method, but I can surely say that it concerns only a fragment of the issue that is improvisation. We can speak about such a, as you called it, procedure in the case of working on compositions in which improvisation is supposed to be stretched over their harmonic structure, and we must remember that harmony is just one of the elements of music. It's impossible then not to know it [this procedure], or not to refer to it during improvisation. At a certain stage of the improviser's musical development, the analysis of harmonies, scales, and chords is (from my perspective) essential. But this is a basis on which professional musicianship – apart from being an underperformer – cannot be built. For me, it is the most important and the most difficult to develop your own language, which is the resultant of all acquired skills, aesthetic taste and musical reliability, by which I mean the knowledge of the genre's development and musical awareness. Improvisation according to harmony is, at a certain stage, much easier than a substantial improvisation based only on the melody, tone colour, or texture of the composition (by substantial improvisation I understand such an improvisation listening to which one cannot distinguish if the musician improvises, plays the theme, or performs a part which was written down earlier).

**You have worked with various, sometimes outstanding musicians. What has made the cooperation easier: their technical skills perfectly corresponding to your needs, or a compatibility of characters, personalities, views = - the musical cooperation being, in a way, an effect thereof?**

There are different reasons to create music and, as the listeners, we never know what these could have been. Although I think that a well-tuned band needs a certain emotional bond, it is often a result of playing together and achieving common goals. It is possible only when the musicians are at a similar stage of development, or when a group of similarly thinking friends gathers and tackles music. The best option is, of course, when we deal with brilliant musicians as well as with similarly vibrating personalities, but this is not essential, nor does it guarantee anything.

**What is the role of the audience in your development as a musician? Would you say that it cooperates with the musician in some way during the concert, for instance, during the entirely improvised parts of performed music? I ask about it time and again, because the answer doesn't seem so obvious to me.**

For a musician, the audience is a mirror and I perceive it exactly as such. Admittedly, I look at this phenomenon from two different perspectives: as a broad, impersonal, general concept of an audience and as the particular group of people present at the concert. I don't think the audience cooperates, I'd rather say that it either helps or not, sometimes even disturbs, so I regard it as another member of the band in a sense. It is, however, important for an experienced musician not to let the quality of his performance be determined by the present audience. On the other hand, in the wide, potential perspective you cannot help thinking of the audience for which you make your music – though it is important to balance what you want to give against what others want, can, and expect to get.

**Once, in an interview with Andrzej Grabowski, your answer to the question about your double bass was: "(...) I'm very happy with it, because it's been designed to suit my needs and I "fraternize" with it every day. I would like to have the opportunity to play a fine, masterfully crafted instrument one day, but it's not exactly on my wish list." What do you think about it now? And could you tell our readers something more about the double bass as its master: what are the differences in the quality of each particular copy of this instrument, what is the lifespan of its parts, are there any dangers of using them, especially in an unconventional way (if we can talk about such)...**

Unlike any other string instrument, the double bass has undergone a significant metamorphosis in the 20th century, becoming a virtuoso instrument. It poses a serious challenge to the luthiers as well as to the musicians, because people who wrote music having the older types of the instrument in mind didn't even anticipate this kind of development. Undoubtedly a hundred years ago nobody expected that in the 20th century double bass jazz players would pluck the strings rather than use a bow. The need

to use both of those techniques has led to contradictory demands concerning the instrument's features. On the one hand you need a heavy, dark sound with a strong, definite attack for the pizzicato technique, but then, open, bright and rather light sound is preferable when playing with a bow. There are no such basses among the historical instruments, because there was no such need. Even now most musicians specialise only in one of these techniques. Nowadays basses like that are made by a few masters around the world and their prices can reach up to thirty thousand euros. Of course, the strings as well as the adjustment of the instrument, the bow, and the technique influence the final sound. And another equally complex matter which looms large in the mind of every "jazz" double bass player is their instrument's amplification.

**How strong is your attachment to this instrument? Do you think or dream about music through its sound? Or maybe such musical fantasising is an interactive combination of various sounds? This question is probably very personal, but it is very interesting (just as the fact that some people compose music almost exclusively on an instrument, while others freely formulate quite abstract structures, sometimes difficult to render using the instrument.)**

Double bass is a tool which I use to filter the music, give it a final shape and by the means of which I speak my own language. It is not the only instrument I use to compose or through which I think about music, although certainly it is one that I have mastered best and through which my thoughts flow in the most undistorted way.

**In what musical constellations do you see yourself in the near future? And another question: which musicians (leaving out those you have already worked with) would you especially like to play with, or maybe already have some plans in that regard?**

What I am doing is rather looking for the answers to the question of what I should do to make my music and my language autonomic. What I should do to become a double bass player distinguishable by a single sound/tone and a composer/musician valued for his own language and not for the ability to copy or keep up with the trends. My need goes beyond the pleasure of just playing music and trying to fit into its requirements, and all the more beyond playing improvised music, just like that. This way of thinking is more characteristic for composers than performing musicians, but this is exactly how I perceive the stage I am currently at.

**Patrizia Bovi from Ensemble Micrologus says that projecting one's own personality on purpose is an inappropriate approach; it is better to set yourself in the position of the servant of music (although her recordings don't show any gentle humility or colourless conservatoire spirit; they are full-blooded and full of character). And what is your opinion on the boundaries of the expansion of the performer's personality?**

I share this opinion, but it requires to be significantly expanded upon, because otherwise it could be misunderstood. It seems to me that it emphasises the fact that it is the musician who should serve music, and not the other way round. Coming back to the earlier thought, I could say that music created by someone with a servant's ethos has a much greater chance of not aging. If a musician treats music only as a tool or a medium, it is frequently an attitude of making a claim, in which a musician puts him/herself above music. Besides, in a properly understood servant's attitude, based on the appreciation of and respect towards one's subordinate position, submission and esteem are visible. This enforces a certain attitude and a musician has to have a reason to say/play something, which is not the case when he/she merely utilises music. This ethos is similar to the so-called civil service which implies serving the society - and we all know what it looks like in reality (similar statistics applies to music and musicians).

**How much did the profession of a musician and composer influence your personality and the organisation of your private life up till now? In the sense of restrictions, giving things up, your needs, your lifestyle.**

There was a moment when I realised that my life is completely subordinated to music. I am trying to work on slowly changing this state of things, because its consequences are literally painful. I have been experiencing problems with my backbone which have almost stopped me from playing and walking. Music is my passion and my remedy, music harmonises me, but it also brings troubles upon me. A dedication in one of the books I have recently received includes a sentence: MUSIC IS YOU. It made me realise that only by caring about myself, my needs, development, and health can I become a better musician and it is the only way I can make my music better.

**photo: © Marta Eloy Cichocka**

**In your opinion, do our country's realities regarding the functioning of musical life still leave much to be desired, or has this already changed over the last years?**

The market is constantly changing and evolving, and according to my recent reflections, it represents trends rather than values. The public can choose between black and white (and I don't have in mind the colours of the musicians' skin), and the thus polarised market imposes constant limitations on its offer. We import more and more foreign musicians, valuing their projects higher than they deserve, and thus we enter a vicious circle, losing our sense of direction. And now, referring to your earlier question, it could be said that the majority of musicians and producers use music rather that serve it. But we must come to our senses and realise that by frequently using the words "outstanding", "the best" or "unique" inappropriately, we lose the possibility of convincing the public to come to concerts just because, not because it is a great event (which is unfortunately a Polish speciality).

**What have you been listening to recently?**

I've been going through a musical fast lately and I don't really listen to anything.

**All photos: Marta Eloy Cichocka (*www.zoomwzoom.blogspot.com*, *www.artforart.pl*)**


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# The Road
## Interview with Bartłomiej Brat Oleś[58]

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**Many words (of varying quality) have been said about music. Have you ever been annoyed by such questions as: "What is jazz?", "What do you feel when you play?", "What is the spiritual dimension of improvisation? – or maybe this hasn't yet affected you that much?**

Such questions do not bother me at all. People who need that kind of classification and musical labels have always been intrigued by how music comes into being, what it means for musicians who perform, what they feel while creating it. That's why such, sometimes provocative, questions are asked and one shouldn't be irritated about that. They concern, not necessarily intentionally, the issue of expressing oneself through music, which is not as easy and popular as it may seem among musicians, especially the younger ones and those thoroughly educated. In order for the artist to be able to answer these questions, a process is needed, a process that is inseparably connected with doubts concerning one's chosen way, the universality of what one does etc. In my opinion, the essence of improvisation lies precisely in expressing oneself through music, but to achieve that, one needs to achieve a high level of musical awareness, not only concerning style and the instrument, but also mental awareness. Sure, it's easier to gain applause while using borrowings of the language that had already been created by someone famous and accepted by the community, musicians and listeners.

**Are you one of these artists who get nervous because of reviews? Is it a question of personality – or of getting used to it, keeping yourself between the enthusiasm of the "fans" (who appear not only in rock music) and the precision of music critics?**

If the review is a stupid, amateur scribble, the only thing to do is wring one's hands. Unfortunately, there is less and less decent jazz criticism in our country. It's easy to write about recognized and stereotypical things, whether it's mainstream or the so-called avant-garde. We'll find hundreds of identical "lukewarm" recordings in both stylistics. There's no risk here. Risk is connected with writing about things that seek their own style, things on the border of different stylistics, running away from com-

[58] ***http://oles-oles.com/BBO_english***

mon standards; sometimes one review may bare the whole truth about the writer. Admittedly, a new Internet tendency may be observed – blogs, where the authors express their opinions about albums and concerts or simply share the information about what they have been listening to recently or what they consider interesting, but that does not have much to do with professional criticism. However, it is a very valuable phenomenon, because it leads to an unstoppable exchange of information between people who are interested in that particular subject, who buy CDs and listen to music.

**Your musical biography includes among others rock music. What do you feel about the rock today? On the other hand: have you been treated as a musical deserter? However it doesn't have to be a rule (we are familiar with jazz-rock and other fusions).**

Rock is where my musical path started; I played with my "backyard" mates, with whom I haven't had any contact for a long time, I don't even run into them on the street. However, my first official recording was with a blues band, not a rock band. I never considered myself a full-time rocker. In those years I was more interested in just playing – it didn't matter what. The phase of being a musician who will play some rock in Jarocin, blues and everything that is needed where it is needed is far behind me. By the way, I do listen to rock sometimes: to Bruce Springsteen, Robert Plant, Pink Floyd or Tangerine Dream, but mostly older recordings. My Vinnie Colaiuta fascination drained off while I was getting older, but a part of his is still somewhere inside me for sure. Rock does not correspond with my musical sensitivity, wild guitars wind me up. There's no betrayal of ideals involved, because my approach was rather youthful.

**To what extent was your way to professionalism professional, strewn with diplomas? Here comes again the question of "professionalism" and "education" in the context of jazz and music of jazz-like provenance.**

My road to professionalism was somewhat rocky. I graduated from musical schools with classical profiles: marimba, vibraphone, timpani; they were my musical daily bread, and none of these instruments was the one to make me feel comfortable as a musician. The profession of a musician was not really respected in my house. My father, admittedly, was a conductor, but jazz??? I put my first drums together all by myself, I made the handles using a tap and pucks with the help of my friend's father who was a turner and made some elements for me, which helped me assemble everything. When I was 17 I recorded my first album with a group called Blustro Blues Band and that's how it started. I remember listening to Charlie Parker for the first time – I was stunned, jazz came in. I practiced like crazy 11 hours a day, because I knew that if I wanted to play jazz, I had to know the instrument from inside out. There were many hours of rehearsals and many various projects when finally the first recordings with Custom Trio, Andrzej Przybielski and Adam Pierończyk appeared, as well as the reviews and my presence on the musical market.

Professionalism, in a universal sense is, in my opinion, a sincere and in-depth approach to what you do. Everything depends on your attitude and what you want to achieve. You simply have to know the basics, it's indisputable. Naturally, schools help, but they won't teach you creativity if you don't have it already. It's enough to list four names: Louis Armstrong, Charlie Parker, Miles Davis, Bill Evans. Each and every one of them would be an amateur without a diploma in today's academic categories, but they were the ones who changed music. As I observe the development of music, not only jazz, I notice an even bigger drop in creativity and originality. It's a great paradox – although we've got more and more well-educated musicians, there's less and less creativity in music.


**On the Internet, there is a pleasant video of you, Marcin and Mikołaj Trzaska performing "portably" on the street. On the other hand - you knock out your audience with such a project as "Contemporary Quartet". Would you like to maintain this wide artistic breath, from a contemporary troubadour to the Warsaw Autumn atmosphere? Or maybe the right course of the development is from the former towards the latter, and someone who has been permanently introduced to the musical high society loses the authenticity of a street musician?**

This, as you called it, "charming movie", was a single incident on the artistic level of Mikołaj Trzaska. Because of this, an album was made, together with a short movie interlude, and they didn't mean a thing to me and they didn't mean a thing to all the serious projects I'm involved with on a daily basis, like Contemporary Quartet, Chamber Quintet, Oleś Duo and many more. I'd like to be identified with them. The street performance with Trzaska may be called an 'artistic breath of air' or an exception to the rule, as I've never performed on the streets. However, one thing remains the same – no matter where I play, I always try to preserve my musical authenticity.

**Philharmonical background of the "Contemporary Quartet", musical precision on "Mikro Muzik"... would you consider playing Webern? Or if not something by him - it is just my subjective reference - then maybe something by one of the other punctual, terribly meticulous composers -watchmakers, operating on a microscale and on the verge of silence. Would you take such or similar projects into consideration?**

Such projects are in the past. There's no use in entering the same river twice, you have to move forward.

**But at the same time there is the temptation of a jazz rebellion, Broetzmann-like powers, liberated improvisation and romantic, rebellious faith in world-changing art. To what extent do you identify yourself with these ideas? I'm not expecting a black-or-white answer; I'm rather trying to bring out more than one layer of your musical personality.**

Brötzmann's ideals are completely foreign to me, no matter of the perspective they are considered in: rock, jazz or philharmonic. Such music is of no interest to me, I seek my own artistic path. I've never tried to start musical revolutions, but rather to find a way to develop my composing and instrumental individuality. I consider Peter Brötzmann to be the musical equivalent of Pollock; no one in the world of the so-called arts will paint the way he did, because it would be a great mishap, however, as we can see, in the world of free jazz Brötzmann's epigones are doing quite well. There are even Polish departments opening, the question is: what for? Speaking of believing in art that changes the world – if it was possible, the world after Bach should have become an idyll.

**And what exactly is new music or a "novelty in music"? Are the Oleś brothers' albums new music?**

**Are Trane's recording already "old" music?**

This distinction is a bit artificial and, in my opinion, wrong. Music can be divided into two categories: good and bad. The first one is also original by its nature. Looking for novelties in music is a waste of time nowadays, unless we assume that new music is music that is being made now. Trane's music, despite its age and the number of copycats of his great talent, hasn't aged a day, because you simply can't copy his spirituality. It's not the sounds everyone can play that constitute the essence of music; the essence of music is the meaning the sounds convey. If our music stirs up emotions and the listeners find original things in it, then it's very nice, no matter if someone calls my music old or new. The path is the most important thing for me. The process of my continuous development as a composer, but mainly as an instrumentalist, is in the foreground. In this way it will never end, because I'll never stop developing, my music evolves constantly and softly, one can say – it becomes more seasoned.

**What do you think about the following, supposedly most popular procedure of practising one's skills of improvisation: [1] analyzing harmony, [2] selecting "scales" and chord sounds, [3] reorganizing "licks", that is, the practised phrases or their fragments, with reference to the rhythm used, and finally: [4] constructing a form that creates tension.**

This is a very proper academic analysis, very useful in terms of education, because it's worth knowing what's played in a piece. There's only one question: will you introduce it as a rule or will you really improvise? Being a true improvisational musician requires three stages of development: imitation, simulation and innovation. The last stage is the most difficult and not every musician cares enough to achieve it. The majority is satisfied with the first stage. It's easier to be applauded and accepted then. To really improvise, you need to listen to the musicians you play with and to what's going on around you, and have enough executive freedom to react to it creatively, creating musical dramaturgy together.

**You have worked with various, sometimes outstanding musicians. What has made the cooperation easier: their technical skills perfectly corresponding to your needs, or a compatibility of characters, personalities, views - the musical cooperation being, in a way, an effect thereof?**

Every person has a different wealth of experience and comprehends music in a different way; therefore, it's hard to demand that we understand each other at the same level. There's no rule, although technical skill always helps; however, to be open-minded is of great importance. While working together with all possible kinds of individualities, the musical effect is always a result of the contact of personalities. Sometimes the artistic process is easy, because the artist, whom I sometimes know only from his/her recordings, finds him-/ herself perfectly in my music and can develop it by bringing his/her personality into it. Sometimes you need some way to do it, a longer moment, a discussion – sometimes about non-musical subjects in order to make things work, although until now I haven't had many problems with it. When you invite an artist consciously, there are no disappointments. The most vital thing is – you need to know how to listen to each other and this rule applies to everyone.

**What is the role of the audience in your development as a musician? Would you say that it cooperates with the musician in some way during the concert, for instance, during the entirely improvised parts of performed music? I ask about it time and again, because the answer doesn't seem so obvious to me.**

The audience is very significant; if it is sensitive and listens closely – it receives more, if not – one has to fight for the audience, but sometimes it's not even worth trying and you just have to do everything as well as you can. The audience is always a part of a concert, it's one of the musicians taking part in the performance. People prove their interest in what we do by buying CDs and therefore they give sense to our actions. It's even more valuable because, as it's commonly known, jazz is not a very popular kind of music.

**You have created music for theatre and film. What kind of experience is it? Can it be defined simply as "illustrating" or "accompaniment" – or is it a quite different work?**

Composing does not differ much from writing music as such. The only difference lies in convincing a director or a producer that this particular composition really fits a certain scene [laughter]. Everything hinges on the sensitivity, openness and flexibility of the person who orders the music. Sometimes the music is illustrative, sometimes it accompanies a scene. Writing theatrical and film music is usually done on commission and it does not differ much from performing services. A hairdresser does a customer's hair the way the customer wants it, and may stumble upon a wish to curl hair on a bald head.

**You have played different instruments; finally we know you as a drummer. How much do you feel connected with the instrument(s)? Or more radical, does the sound of percussion is the one that you dream of or that you think?**

The percussion is one of the most important things in my life, I jokingly say that percussion is my lover. The amount of time I have spent with it is impossible to measure; even these days I try to practice 3 or 4 hours a day. I'm constantly changing something, no matter whether it concerns the technique or the instrument itself. Seriously, to play music professionally, one has to feel one's instrument; if they don't feel it, they can't play it well. The percussion gives me the freedom to express myself and is a medium through which I tell my stories. However, I compose on the piano, even though I would never appear playing it in public.

**You have intrigued me. Why piano? Is it a matter of personal preferences or specific characteristics of the instrument, of the sound ...?**

The piano simply is the most comfortable instrument to do it. If I had absolute pitch, I probably could do without it, but as I don't, I need an instrument that allows me to check the melodies, gamuts or harmonic solutions. It's nothing unusual; many composers use the piano to create music.

**Could you briefly describe us your instrument? I mean a set of elements, brand, durability etc. Is there any set of drums typical for improvised music, jazz ...?**

For the last fifteen years I've been using Yamaha Maple Custom with the following dimensions: bass drum 18"x14", toms 10"x8", 12"x8", 14"x10", snare drum 14"x4". Cymbals are by Zildjan and I've been assembling them for about twenty years – right now I'm using the following configuration of trays: 22" K Constantinople Thin High Ride, 22" K Constanitinople Overhemmered Thin Ride, 20" K Light Flat Ride, 18" K Constantinople Crash, 19" K Custom Dark China, 13" K Constantinople Hi Hat and 9" Oriental Trash Splash , 8" ZBT/ZHT Splash Hi-Hat. The drumheads are by Remo Ambasador Coated, drumsticks are by Osca4Drums with my signature, and they have quite recently made a new model for me, but this time it's made from hickory. The change of technique brought with itself the need to change the shape of the drumstick and the kind of the wood it's made from. Hickory is heavier and less springy than maple I'd used before; it's easier to control it in one's hand and it gives my music more calmness and a much deeper sound. I use Yamaha hardwere.

The Maple Custom model is an extremely comfortable instrument in terms of size and arrangement. After many years I have completed the instrument of my dreams that works well both in the studio and on different kinds of stages; it allows my music to develop and evolve constantly.

**Patrizia Bovi from Ensemble Micrologus says that projecting one's own personality on purpose is an inappropriate approach; it is better to set yourself in the position of the servant of music (although her recordings don't show any gentle humility or colourless conservatoire spirit; they are full-blooded and full of character). And what is your opinion on the boundaries of the expansion of the performer's personality?**

Unfortunately, I know neither Patrizia nor her band. Everything depends on whether you play your own music or music composed by someone else. Sometimes the composer or leader of a band invites a certain artist because of their sound, expression. This opinion is more useful in case of the so-called classical music, where most of the written parts have an exactly defined way of being performed, unless the composer wishes to change it. The performer is just a tool. In jazz, the individuality of the performer is crucial. Personally, I believe that expression must always be subordinated to the performed composition, but with preserving artist's sensitivity. It's always good to tell a story. It's the idea for an interpretation that causes music to live and allows us to play it endlessly in so many ways.

**How much did the profession of a musician and composer influence your personality and the organisation of your private life up till now? In the sense of restrictions, giving things up, your needs, your lifestyle.**

It's a tough issue. If you want to be a musician and to live from making music, you have no other choice but to subordinate your life to music and all the limits bound with it, and it changes a lot. I try to live quite normally, to the extent the financial and organizational reality of this profession allows me. Generally, you should apply survival skills. The amount of offers that you get may be counted on the fingers of one hand. I'd like to leave the question of limitations and what you have to give up unsaid. Self-discipline and working rhythm are important.. Practising, composing and analysing music comprise my everyday life.

**In your opinion, do our country's realities regarding the functioning of musical life still leave much to be desired, or has this already changed over the last years?**

Sometimes it’s hard to keep one’s dignity while speaking with people who manage the money spent on culture in our country. All they see is the tip of the iceberg, and our profession comprises years of hard, continuous work. The only thing that has changed is that we have ever more musicians who play better and better for less money or do not play at all. The market has divided itself into those who play for pitiable rates and celebrities. Poland becomes a great musical backwoods. It’s not easy to think hopefully about new projects, albums and further development.

**What have you been listening to recently?**

I don't listen to music only for pleasure, it's just one of the aspects. I try to be abreast of what's happening on the market and, accordingly, I sometimes listen to different things once. While listening, I analyze the albums from musical and instrumental angles. There's no use in listing all the albums I've recently listened to. But if you ask me which album has made the biggest impression on me, then I'll tell you. So... which one?

**?**

Joe Henderson – *Power to the people*; Kenny Werner – *Baloons*; Thomas Savy – *French suite*; Jacaszek – *Glimmer*; Branford Marsalis – *Four MFs Playin' Tunes.*

**Photos: BlankaTomaszewska & Grzegorz Domagała**


ARTIST OF THE ISSUE

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# Painting from the Inside Out
## Interview with Teresa Young[59]

Monika Włudzik (questions & realizations) and Przemysław Staroń (questions)

Realized November 2012; published online 24 December 2012


Teresa Young: *Dreams of Lothlorien*

[59] ***http://www.teresa-young.net***

**Looking at your paintings, one may think that your compositions are based on glances and how their lines meet. What are the characters from your paintings looking at? Do they look outwards or inwards? At the world of the painting or the viewer?**

Actually, I think that's an interesting concept! Very often, the eyes within my painting are looking outwards, but usually only when they are by themselves. The characters are often looking inwards contemplatively, unless it's obvious that they are meeting the eyes of the viewer. When I did portraits in my youth, my patrons often mentioned that the portraits I created had eyes that appeared to follow the viewer as they changed position; and I think that's still true of some of the characters in my paintings.

But really, sometimes when I paint, creativity itself just causes the designs to flow in different directions, I tend to paint until I feel that the artwork is 'balanced' or 'right'. Usually, I can see several valid interpretations after the work is complete. So it's an interesting doorway into my own psyche, and it's enlightening for me as a way of knowing my own mind. An artist acquaintance of mine offered his opinion that in a way, I'm still doing portraits in a way, but these are now focused on my internal makeup rather than the external.

**The divine symbol of the disembodied eye that sees and knows everything inspires both fascination and horror. Do the eyes from your pictures exist as separate organs that link the material dimension to the immaterial one? Are they in a way similar to the Cartesian pineal gland that was supposed to connect the mind with the body?**

Yes, the floating eyes in my paintings are definitely symbolic. And you are bang on when you think they link us to an immaterial world. These eyes form a window into 'something' else. By joining the fantasy art realm and the viewer, the eyes serve as an emotional bridge for us to connect to what's going on in this other realm.

**Some of your more recent works resemble the pictures you can see in a kaleidoscope or under a microscope. What inspires those colourful hallucinatory forms and organic compounds? Would that be microbiology or rather stained-glass art?**

Actually, it might be a bit of both. I love stained glass, and early on, it was pointed out to me that my acrylic paintings often have that feeling to them. So, of course, I had to try stained glass in real life (one of my day jobs was in the military as an electronics technician, I was already great with a soldering iron). Later on, since I found stained glass too restrictive artistically (The lines weren't fine enough for me, and they all had to connect!), I moved on to faux stained glass painting. This was interesting, as the colours were vivid and dynamic.

Regarding the organic tendencies in my paintings? That was probably due to the fact that my early focus on a career was in biology. (Every artist needs a day job, so I thought biology was a good choice!) So I started out as a scientist of sorts, which is

probably why I ended up getting a degree in electrical engineering as an older adult. My orientation to details and love of mathematics influences my ideals of beauty and expresses itself in my art style.

**Could you tell us what are your major inspirations? What topics tend to reappear in your art?**

Patterns, manual fractals[60] and dancing colour harmonies are integral components to most of my paintings. But what are my major inspirations for painting? I guess nature is one, as I've always been fascinated by the play of colours and shadows in the world around me.

I like to think that sometimes I paint with light, so that's a source of inspiration as well. A topic that does reappear consistently in my work is personal growth and pushing your own boundaries. I really believe that art is self-exploration for the artist.

**Would you say that your works have more surrealistic or expressionistic overtones?**

Expressionistic, oddly enough, because it is all really an expression of my inner environment. I've always thought that artists truly only have their own voice to speak with, and if we're lucky, it resonates with others.

**Could you name three artists that have had a decisive influence on your painting style?**

Actually, it's pretty easy - Salvador Dali, Emily Carr, native aboriginal art of the Haida tribe on the west coast of Canada. The last isn't a particular artist, but you can see a native Indian influence on my artwork, and I really can't discount it. It's so noticeable that I researched my family tree and found out that I have roots in that culture. My grandfather was Cree and British (Métis).

**One of your paintings printed by Avant was inspired by Tolkien's realm of elves. Is literature important to you as an artist?**

Yes, I'm quite an avid reader, and sorry to say, I've read most of Tolkien's works many times. I'm a veteran fantasy fan, as this is my favourite type of fiction.

The drawing that you refer to actually became one of my favourite pieces I've created this year, so last weekend I finally finished an acrylic painting based on the drawing. The painting is slightly different than the original drawing, as I don't ever copy using

[60] According to the definition provided by Kerry Mitchell in *The Fractal Art Manifesto*, "Fractal Art is a genre concerned with fractals—shapes or sets characterized by self affinity (small portions of the image resemble the overall shape) and an infinite amount of detail, at all scales. Fractals are typically created on a digital computer, using an iterative numerical process." More at *www.fractalus.com* DOA 23 Nov 2012.

grids or any other artificial means. I just looked at it and redrew it onto the canvas freehand, and it's a different medium, so it has a different feel to it. I like it though, it has added a more spread-out balance to the piece, and the contrast using deep darks and bright highlights with complementary colour harmonies offers a different experience for the viewer.

**In one of the entries on your blog, you said that you are fascinated by computer graphics. How do see painting in the era of digitization?**

I see digital graphics as a brave new world, and the latest art expression frontier. It gives rise to quite a spectrum of creativity and like the evolution of technology and the Internet itself, it's constantly changing. Ironically, it's not one that I've delved into very deeply on my own, yet.

**More often than not you use organic or pseudo-organic forms in your paintings. Would it be justified to say that your art is ecological, in the sense that it expresses a longing for a life close to nature?**

I'd sort of agree with you there, but I don't think it's expressing a longing for a life close to nature. More accurately, it's an expression of my connection to the world and my feelings about life in general. I find a lot of beauty in organic forms, since my early bent in art was towards realism. (Heavily towards realism! I never did an abstract piece until my late teens, it's almost like I was rigid and couldn't get my brain around it.)

In realistic art, you're interpreting nature to create. Personally, I see no reason why abstraction needs to be formless and undefined. It's a world of its own in every artwork, and like a good fantasy writer, I'm *world building*, so I give it depth and definition on its own terms.

**Let's go back to nature, this time in a more literal sense. Are you in any way inspired by the landscapes of Nova Scotia?**

Of course, but in a more emotional rather than representational way; over time, I've really moved away from copying what I see in the outer world.

One thing I've always thought about as an artist is that we see differently than most people. Sometimes I can turn my mind towards a different kind of sight, viewing the world around me as a painting. In Nova Scotia, when I look at the landscape I can sometimes see brushstrokes there. Especially in the sky and distant hills, it's that vivid.

**More about Teresa Young: *http://www.teresa-young.net/information/***

# AUTHORS OF THE ISSUE

**Adrian Alsmith** (*né* Smith) is a post-doc at the Centre for Subjectivity Research, University of Copenhagen. In 2011, he defended his PhD at the University of Mainz, Germany, supervised by Thomas Metzinger. He is a member of the network "The (un)bound body: Exploring the limits of body representation &the constraints of embodiment" funded by the Volkswagen Foundation. He has co-edited a special issue of the *Review of Philosophy & Psychology* (March 2012), titled "The Body Represented / Embodied Representation". His interests span topics in the philosophy of mind and cognitive science.

**Adrian Alsmith** (z domu: Smith) – post-doc w Centrum Centre for Subjectivity Research na University of Copenhagen. W 2011 r. obronił doktorat na University of Mainz; promotorem był Thomas Metzinger. Członek sieci „The (un)bound body: Exploring the limits of body representation &the constraints of embodiment" wspieranej przez Volkswagen Foundation. Współredagował numer spec. *Review of Philosophy & Psychology* (Marzec 2012) pt. „The Body Represented / Embodied Representation". Zainteresowania: filozofia umysłu i kognitywistyki.

**Robert St. Amant** received a B.S. in Electrical Engineering and Computer Science from the Johns Hopkins University in 1985 and a Ph.D. in Computer Science from the University of Massachusetts, Amherst, in 1996. He is an associate professor in the computer science department at North Carolina State University. His current research is in human-computer interaction and cognitive modeling. His book, *Computing For Ordinary Mortals*, will be published by Oxford University Press in the fall of 2012.
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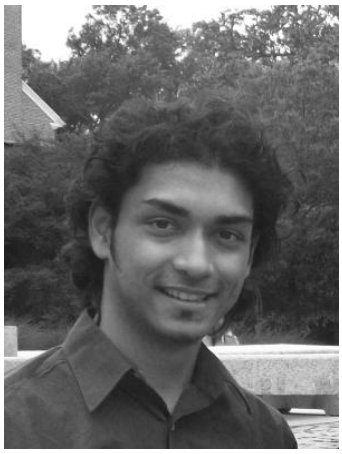
**Robert St. Amant** otrzymał dyplom inżyniera elektryka i informatyka na Johns Hopkins University w 1985, a doktorat w zakresie informatyki zrealizował na University of Massachusetts w Amherst w 1996. Jest asystentem na wydziale informatyki na North Carolina State University. Jego zainteresowania badawcze obejmują interakcję człowiek-kompuer i modelowanie poznawcze. Swoją książkę *Computing For Ordinary Mortals* opublikował w Oxford University Press w 2012.

**Annemiek Barsingerhorn** is a post-graduate student at the Center for Human Movement Sciences of the University Medical Center Groningen, The Netherlands. Annemiek is interested in how people interact with their environment, how decisions arise, and why we act upon some possibilities for action and not on others. Her main research has focused on decision making in volleyball from an Ecological Psychology perspective.

**Annemiek Barsingerhorn** uczęszcza na studia podyplomowe w Center for Human Movement Sciences na University Medical Center Groningen w Holandii. Głównymi zainteresowaniami Annemiek są: interakcja ludzi ze środowiskiem, podejmowanie decyzji, a także zagadnienia wyboru takich a nie innych możliwości aktywności ruchowych. Jej główne badania skupiają się na podejmowaniu decyzji podczas gry w siatkówkę z perspektywy psychologii ekologicznej.



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**Arpan Chakraborty** przystępuje do studiów doktoranckich na wydziale informatycznym North Carolina State University. Jego badania podejmują widzenie komputerowe, robotykę kognitywną oraz interakcję człowiek-komputer. Brał udział w badaniach z zakresu informatyki teoretycznej nad logiką rozmytą i niemonotoniczną w Indian Statistical Institute (Kolkata). Jego praca w dziedzinie robotyki ewoluowała od planowania trajektorii z użyciem percepcji allocentrycznej do wnioskowania w zakresie ucieleśnienia z perspektywy egocentrycznej.

**Anthony P. Chemero** is a professor at the Departments of Philosophy and Psychology, University of Cincinnati, USA. His research is both philosophical and empirical. Empirical interests are: Dynamical Modeling, Phenomenology, and Artificial Life. Philosophical Areas of Specialization: Philosophy of Cognitive Science and Philosophy of Science. Philosophical Areas of Competence: Philosophy of Mind and Philosophy of Biology. Author of books: *Radical Embodied Cognitive Science* (2009, MIT Press), *Phenomenology and Cognitive Science* (co-author: S. Kaufer, forthcoming, Polity Press) as well as many articles. He is working with M. Silberstein on a new theory of dynamical explanation. Website:
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**Anthony P. Chemero** jest profesorem w Departments of Philosophy and Psychology na University of Cincinnati (USA). Prowadzi badania zarówno filozoficzne, jak i empiryczne. W tych ostatnich podejmuje zagadnienia modelowania dynamicznego, fenomenologii i sztucznego życia. Jako filozof specjalizuje się w filozofii kognitywistyki i filozofii nauki, a obszar jego kompetencji filozoficznych obejmuje filozofię umysłu i filozofię biologii. Jest autorem książek: *Radical Embodied Cognitive Science* (2009, MIT Press), *Phenomenology and Cognitive Science* (współautor: S. Käufer, w druku, Polity Press), a także wielu artykułów. Pracuje z M. Silbersteinem nad nową teorią wyjaśniania dynamicznego.

**Alan Costall** is a Professor of Theoretical Psychology at University of Portsmouth. His research interests are wide, and include: children's drawings, psychology of art, music perception, perfect pitch, autism, event perception, the meanings of things ('affordances'), so-called 'theory of mind', and anthrozoology. The topics of his current courses are ecological psychology, and the nature of science, but have also presented courses on the psychology of art, and on Darwin's impacts on psychology. Co-author of books: *Against Theory of Mind* and *Michotte's experimental phenomenology of perception*. Editor of some books and other works. Author of many articles. Website: *http://www.port.ac.uk/departments/academic/psychology/staff/title,50471,en.html*

**Alan Costall** jest profesorem psychologii teoretycznej na University of Portsmouth. Jego zainteresowania naukowe są szerokie i obejmują m.in. dziecięce rysownictwo, psychologię sztuki, percepcję muzyki, słuch absolutny, autyzm, postrzeganie wydarzeń, znaczenie rzeczy (afordancje), tzw. teorię umysłu oraz antrozoologię. Tematami jego obecnych zajęć akademickich są psychologia ekologiczna i natura nauki; prowadził też zajęcia poświęcone psychologii sztuki oraz wpływowi Darwina na psychologię. Współautor książek *Against Theory of Mind* i *Michotte's experimental phenomenology of perception*. Redaktor książek oraz innych prac. Autor wielu artykułów.

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**Maciej Dombrowski** jest adiunktem w Zakładzie Ontologii i Teorii Poznania na Uniwersytecie Wrocławskim. Ukończył polonistykę i filozofię na Uniwersytecie Mikołaja Kopernika w Toruniu. Zajmuje się ontologią, filozofią przyrody i filozofią polską; przygotowuje edycję prac filoz. S. I. Witkiewicza w ramach kolejnego tomu *Dzieł Zebranych*. Autor książki *Filozofia i nauka. Trudne związki. Metallmann – Witkiewicz – Gawecki* (Toruń 2011). Publikował w *Kwartalniku Filozoficznym*, *Ruchu Filozoficznym*, *Przeglądzie Filozoficzno-Literackim* i pracach zbiorowych.

**Dobromir Dotov** is a PhD candidate in experimental psychology at the Center for the Ecological Study of Perception and Action. His work covers problems in movement, visual perception and tool-use as well as theoretical issues related to the application of tools and concepts from nonlinear dynamics and complexity into the sciences of mind and behavior.

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**Tom Froese** is a Postdoctoral Research Fellow at Departamento de Ciencias de la Computación, Instituto de Investigaciones en Matemáticas Aplicadas y en Sistemas, Universidad Nacional Autónoma de México (Mexico). Previous academic positions: Postdoctoral Research Fellow at Ikegami Laboratory, University of Tokyo (Japan) as well as Postdoctoral Research Fellow at Neurodynamics and Consciousness Laboratory, University of Sussex (UK). His research tends to be situated in interdisciplinary debates and geared toward using the tension of these intellectual interfaces to clarify our understanding of the ideas, assumptions and phenomena involved.
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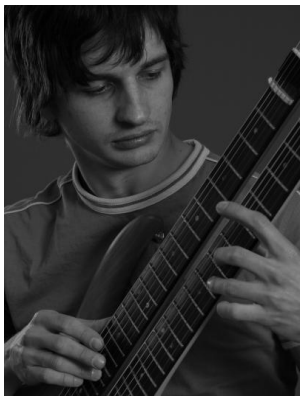
**Tom Froese** jest adiunktem-badaczem w Departamento de Ciencias de la Computacion Instituto de Investigaciones en Matemáticas Aplicadas y en Sistemas na Universidad Nacional Autónoma de México (Meksyk). Poprzednie stanowiska to adiunkt-badacz w Ikegami Laboratory na University of Tokyo (Japonia) oraz adiunkt-badacz w neurodynamiki i świadomość Laboratory na University of Sussex. Jego badania coraz bardziej sytuują się w debatach interdyscyplinarnych i nakierowują się – dzięki napięciom w tych intelektualnych interfejsach – na wyjaśnianie rozumienia idei, założeń i zjawisk powiązanych.



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**Thomas E. Horton** recently completed his Ph.D. in computer science at North Carolina State University. His research focuses on the application of concepts related to the use of physical tools to issues in computing, including work on tool-based software interfaces and the development of computer vision techniques for robotic tool-using agents.

**Thomas E. Horton** uzyskał ostatnio stopień doktora w dziedzinie informatyki na North Carolina State University. Jego badania skoncentrowane są na wykorzystaniu pojęć związanych z użyciem fizycznych narzędzi w badaniu zagadnień w informatyce, wliczając w to prace nad narzędziowymi interfejsami oprogramowania oraz rozwój technik widzenia komputerowego dla używających narzędzi agentów robotycznych.

**Hanne De Jaegher** is Deputy Training Manager and Research Fellow of the TESIS Project. She works at the San Sebastián node, where she is part of the IAS-Research Centre. She works in cognitive science. Her main interests are in the dynamics of social interaction and their relation to social cognition and cognition in general. She investigates these from an enactivist perspective. Her interests include: cognition, social cognition, embodiment, enaction, dynamical systems, rhythm, movement, gesture, language (in the broad sense of communicative behaviour), autism, education, learning, psychotherapy.
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**Hanne De Jaegher** jest zastępcą kierownika szkolenia oraz członkiem grupy badawczej w ramach TESIS Project. Pracuje w San Sebastián Node, gdzie jest członkiem IAS-Research Centre. Zajmuje się kognitywistyką. Jej główne zainteresowania badawcze obejmują dynamikę interakcji społecznych oraz ich związki z poznaniem społecznym i poznaniem w ogóle. Badania te prowadzone są z perspektywy enaktywistycznej. Zajmuje się poznaniem, poznaniem społecznym, ucieleśnieniem, enakcją, systemami dynamicznymi, rytmem, ruchem, gestem, językiem (w szerokim znaczeniu zachowań komunikacyjnych), autyzmem, edukacją, nauczaniem, psychoterapią.

**Maciej Karpiński** is a member of the Department of Psycholinguistics at the Institute of Linguistics AMU, Deputy Dean for Research at the Faculty of Modern Languages and Literature, Deputy Head of the Center for Speech and Language Processing AMU where he participates in the DiaGest Research Group, and a co-founder of Polish Centre for Language Resources. His scientific interests include dialogue structure and dynamics, prosody, pragmaphonetics, multimodal communication, speech corpora, emotional speech, and more.

**Maciej Karpiński** jest pracownikiem Instytutu Psycholingwistyki Uniwersytetu im. Adama Mickiewicza w Poznaniu, prodziekanem do spraw badań Wydziału Języków Współczesnych i Literatury oraz zastępcą kierownika Center for Speech and Language Processing UAM, gdzie bierze udział w DiaGest Research Group. Jest współzałożycielem Polskiego Centrum Zasobów Językowych. Jego zainteresowania naukowe obejmują strukturę i dynamikę dialogu, prozodię, pragmafonetykę, komunikację multimodalną, ucieleśnione aspekty mowy, mowę emocji i in.

**Andrzej Klawiter** is a cognitive scientist and philosopher. Professor of Adam Mickiewicz University (AMU) in Poznan, Poland. 1999-2007 – Head of the Section of Epistemology and Cognitive Science, Institute of Philosophy, AMU. 2007-2012 – Head of the Section of Logic and Cognitive Science, IP, AMU. Author of papers on audition, consciousness, principles of cognitive science, phenomenology. Selected publications: "Why did Husserl not become the Galileo of the Science of Consciousness?" (2004, *Poznań Studies in the Philosophy of the Sciences and the Humanities*), "The audition of natural sounds — its levels and relevant experiments" (co-author: Anna Preis, 2005, *Forum Acusticum*) and others. Website:
*http://kognitywistyka.amu.edu.pl/en/?page_id=57*

**Andrzej Klawiter** jest kognitywistą i filozofem. Pracuje nad kognitywistyczną koncepcją słyszenia, podstawami teoretycznymi kognitywistyki, problemem świadomości i percepcją multimodalną. Dr habilitowany, profesor nadzwyczajny Uniwestytetu im. Adama Mickiewicza, wcześniej pracował w Instytucie Filozofii UAM, gdzie w latach 1999-2007 kierował Zakładem Epistemologii i Kognitywistyki. W latach 2007-2012 kierownik Zakładu Logiki i Kognitywistyki w Instytucie Psychologii UAM. Autor artykułów o słyszeniu, świadomości, podstawach kognitywistyki, fenomenologii.

**Tomasz Komendziński** (PhD) is a researcher at the Department of Cognitive Science and Epistemology and at the Centre for Modern Interdisciplinary Technologies, Nicolaus Copernicus University, associated lecturer at the Warsaw School of Social Sciences and Humanities and fellow of International Communicology Institute. His main area of interest covers integrated theory of communication, embodied cognition and extended mind, neurophenomenology, enactivism, theory of interdisciplinary studies. Editor of international journal *Theoria et Historia Scientiarum* and author of two books.

**Tomasz Komendziński** (dr) jest pracownikiem Zakładu Kognitywistyki i Epistemologii oraz badaczem Interdyscyplinarnego Centrum Nowoczesnych Technologii Uniwersytetu Mikołaja Kopernika w Toruniu, wykładowcą w Szkole Wyższej Psychologii Społecznej w Warszawie, członkiem International Communicology Institute. Jego główne zainteresowania badawcze skupione są na teorii komunikacji, ucieleśnionym poznaniu i umyśle rozszerzonym, neurofenomenologii, enaktywiźmie, teorii badań interdyscyplinarnych. Redaktor międzynarodowego pisma *Theoria et Historia Scientiarum*. Autor dwóch książek.

**Dawid Lubiszewski** is a PhD student in Department of Philosophy of Science at Institute of Philosophy, Nicolaus Copernicus University. His main scientific interests are theory of complexity, cellular automata and cognitive science.

**Dawid Lubiszewski** jest doktorantem w Zakładzie Filozofii Nauki Instytutu Filozofii Uniwersytetu Mikołaja Kopernika w Toruniu. Do jego zainteresowań należą teoria złożoności, automaty komórkowe i kognitywistyka.

**Damiano Menin** is a PhD student at Department of Philosophy, University of Milan (Italy). Research interests: Philosophy of Mind, Philosophy Of Science, Science Epistemology, Embodied Cognition, Music Education, Music Psychology, Neurosciences, Neurology of Music, Music Cognition, Improvisation, Affordances.

**Damiano Menin** jest doktorantem w Department of Philosophy na University of Milan (we Włoszech). Jego zainteresowania badawcze obejmują: filozofię umysłu, filozofię nauki, epistemologię nauki, ucieleśnione poznanie, edukację muzyczną, psychologię muzyki, neuronaukę, neurologię muzyki, poznanie muzyczne, improwizację, afordancje.

**Lin Nie** studied psychology and mathematics at Franklin and Marshall College. Currently, she is in the PhD program of the Center for Ecological Study of Perception and Action and is excited by research and problems in perception-action, dynamical systems, complexity and emergence. She also dances, choreographs, and loves reading phenomenology.

**Lin Nie** studiowała psychologię i mathematykę we Franklin and Marshall College. Obecnie realizuje swój pogram doktorancki w Center for Ecological Study of Perception and Action. Bardzo zaangażowała się w badania i problematykę percepcji-działania, systemów dynamicznych, złożoności i emergencji. Pasjonuje ją taniec, choreografia, a także literatura fenomenologiczna.

**Przemysław Nowakowski** is a graduate in Philosophy at Faculty of Humanities of Nicolaus Copernicus University in Torun. The title of his PhD thesis is "The role of the body space in the embodiment of cognition. In search of cognitive conception of the body". Research interests: embodied cognition and bodily self-consciousness.
Website: *https://sites.google.com/site/prnowakowski/*

**Przemysław Nowakowski** jest absolwentem filozofii, którą ukończył na Uniwersytecie Mikołaja Kopernika w Toruniu. Tam też przygotował swoją rozprawę doktorską „O roli przestrzeni ciała w ucieleśnieniu poznania. W poszukiwaniu kognitywnej koncepcji ciała" (Zakład Kognitywistyki i Epistemologii, promotor: prof. U. Żegleń). Zainteresowania: ucieleśnienie poznania i cielesna samoświadomość.

**Ezequiel Di Paolo** is a Research Professor working at Ikerbasque, the Basque Science Foundation, in San Sebastián, Spain. He remains a member of the Centre for Computational Neuroscience and Robotics and the Centre for Research in Cognitive Science at Sussex. His interdisciplinary work on the enactive approach to life, mind and society integrates insights from cognitive science, phenomenology, philosophy of mind and computational modelling. His recent research focus is on embodied intersubjectivity and participatory sense-making. His other research interests include embodied cognition, dynamical systems, adaptive behaviour in natural and artificial systems, biological modelling, complex systems, evolutionary robotics, and philosophy of science.
Website: *http://ezequieldipaolo.wordpress.com/*

**Ezequiel Di Paolo** jest profesorem w zakresie badań w Ikerbasque: Basque Science Foundation w San Sebastian w Hiszpanii oraz członkiem Centre for Computational Neuroscience and Robotics i Centre for Research in Cognitive Science at Sussex. Jego interdyscyplinarne prace ujmują życie, umysł i społeczeństwo z perspektywy enaktywizmu; wykorzystują wiedzę z zakresu kognitywistyki, fenomenologii, filozofii umysłu i modelowania komputacyjnego. Jego ostatnie badania koncentrują się na ucieleśnionej intersubiektywności i tworzeniu sensu we współuczestnictwie. Interesuje się ucieleśnionym poznaniem, systemami dynamicznymi, adaptacyjnym zachowaniem się systemów naturalnych i sztucznych, biologicznym modelowaniem, systemami złożonymi, robotyką ewolucyjną, filozofią nauki.

**Gert-Jan Pepping** is Assistant Professor at the Center of Human Movement Sciences at the University Medical Centre Groningen in The Netherlands. Gert-Jan studies the social, cognitive-affective, and perceptuo-motor processes involved in decision-making in sport. He studies these topics in team sports such as football and volleyball. In his research Gert-Jan adopts an Ecological Psychology perspective. Website: *http://www.pmarc.ed.ac.uk/people/gert-janpepping.html*

**Gert-Jan Pepping** jest adiunktem w Center of Human Movement Sciences na University Medical Centre Groningen w Holandii. Badacz ten zajmuje się społecznymi, poznawczo-afektywnymi i percepcyjno-ruchowymi procesami zaangażowanymi w podejmowanie decyzji w sporcie. Bada te zagadnienia w zakresie sportów zespołowych, takich jak piłka nożna i siatkówka. W swoich badaniach przyjmuje perspektywę psychologii ekologicznej.

**Andrea Schiavio** is currently a PhD student in Music Psychology at the University of Sheffield. After graduating in Philosophy of Science and in Musicology at the University of Milan, he started his research on the phenomenological and neuroscientific implications of an embodied approach to human musicality, in light of the mirror mechanism's theory of action understanding.

**Andrea Schiavio** jest obecnie doktorantem Music Psychology na University of Sheffield. Po ukończeniu studiów w zakresie filozofii nauki i muzykologii na University of Milan rozpoczął badania nad fenomenologicznymi i neuronaukowymi implikacjami ucieleśnionego podejścia do ludzkiej muzykalności w świetle teorii lustrzanego mechanizmu rozumienia działania.

**Joanne Smith** is Assistant Professor at the Center for Human Movement Sciences of the University Medical Center Groningen, The Netherlands. Joanne is interested in understanding the development and control of perceptual-motor skills. Her main research focuses on decision-making and the control of action, and how these two issues are integrated in child and adult populations. In her research Joanne adopts an Ecological Psychology perspective and aims to create an integrative approach to the study of the development of cognition and action.

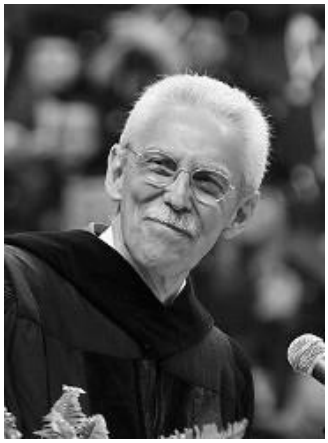
**Joanne Smith** jest adiunktem w Center for Human Movement Sciences na University Medical Center Groningen w Holandii. Interesuje się rozumieniem rozwoju i kontroli umiejętności percepcyjno-motorycznych. Jej badania koncentrują się na podejmowaniu decyzji i kontroli działań, a także na tym, w jaki sposób te dwie kwestie są zintegrowane w populacjach dziecięcych i dorosłych. W swoich badaniach przyjmuje perspektywę psychologii ekologicznej, mając na celu stworzenie zintegrowanego podejścia do badania rozwoju poznania i działania.

**Przemysław Staroń** A graduate of psychology and cultural studies at Catholic University in Lublin. Teacher of ethics and knowledge of the culture, psychologist, trainer at the Centre for the Development "Jump", and expert in SWPS Press Center. He works creatively with young people, adults and seniors.

Absolwent psychologii i kulturoznawstwa Katolickiego Uniwersytetu Lubelskiego. Nauczyciel etyki i wiedzy o kulturze, psycholog, trener w Centrum Rozwoju „Jump", ekspert w Centrum Pra-sowym SWPS. Na co dzień pracuje twórczo z młodzieżą, dorosłymi i seniorami.

**Nelly Strehlau** is a lecturer at Department of English, Nicolaus Copernicus University in Torun (Poland), language editor, as well as English to Polish and Polish to English translator in the field English to Polish translator of works in the field of Cognitive Sciences, Philosophy of Mind and others.

**Nelly Strehlau** jest wykładowcą na Katedrze Filologii Angielskiej Uniwersytetu Mikołaja Kopernika w Toruniu, a także redaktorem językowym oraz tłumaczem tekstów naukowych z języka angielskiego na polski i polskiego na angielski, w zakresię kognitywistyki, filozofii umysłu i in.



**Michael T. Turvey** is a Board of Trustees' Distinguished Professor (Emeritus, University of Connecticut). He received his Ph.D. from Ohio State University in 1967. He joined the University of Connecticut in 1967 and the Haskins Laboratories in 1970. His awards include a Guggenheim Fellow, the American Psychological Association (APA) Early Career Award, Fellow at the Center for Advanced Study in the Behavioral Sciences, Cattell Fellow, Honorary Doctorates from Free University of Amsterdam and Florida Atlantic University, and others. He has published over 380 scientific articles, produced more than 40 Ph.D.s, and taught more than 27, 000 undergraduates. His research on perception and action and their inter-relation follows James Gibson and Nicholai Bernstein in emphasizing the search for general laws and principles. His research on visual word recognition pursues the key role of phonology in reading identified by Alvin and Isabelle Liberman. Website:
http://ione.psy.uconn.edu/mturvey/

**Michael T. Turvey** jest profesorem emerytowanym (specjalny tytuł: *Board of Trustees' Distinguished Professor,* University of Connecticut). Uzyskał doktorat na Ohio State University w 1967. Rozpoczął pracę na University of Connecticut w 1967 i w Haskins Laboratories w 1970. Wśród nagród, które otrzymał, można wymienić: Guggenheim Fellow, American Psychological Association (APA) Early Career Award, Fellow w Center for Advanced Study in the Behavioral Sciences. Opublikował ponad 380 artykułów naukowych, był promotorem ponad 40 doktora-

tów i uczył ponad 27 000 studentów. Jego badania na temat postrzegania i działania oraz ich wzajemnych relacji podążają za badaniami Jamesa Gibsona i Nicholaja Bernsteina w zwracaniu szczególnej uwagi na poszukiwanie ogólnych praw i zasad. Jego studia nad wzrokowym rozpoznawaniem słów dążą do wskazania kluczowej roli fonologii w czytaniu, tak jak to zauważyli Alvin i Isabelle Liberman.

**Adam Tuszyński** is a graduate in Philosophy at University of Edinburgh (UK) and a PhD student at Department of Epistemology, Warsaw University (Poland). Main research interests: philosophy of mind and cognitive science. Other interests: rock music and belles-lettres.

**Adam Tuszyński** jest absolwentem filozofii Uniwersytetu w Edynburgu oraz doktorantem w Zakładzie Epistemologii Uniwersytetu Warszawskiego. Jego główne zainteresowania badawcze obejmują filozofię umysłu i kognitywistykę. Ponadto pasjonuje się muzyką rockową oraz literaturą piękną.

**Frederique de Vignemont** is a CNRS researcher at the Jean Nicod Institute (Paris). Her research on bodily awareness include the phenomenology of bodily awareness, body representations (body schema and body image), the body and the space and more. She also wrote several experimental papers related to one of the most famous experiment in the field - the rubber hand illusion. Besides her scientific research interests are in the field of self-consciousness, social cognition and psychopathology. She has published about 50 papers. She has worked with many philosophers (Alvin Goldman, Pierre Jacob, Adrian Alsmith) and cognitive scientists (Marc Jennerod, Patrick Haggard, and many others). She is currently working on a monograph entitled *Mind the Gap*. Website: *https://sites.google.com/site/fvignemont/*

**Frederique de Vignemont** jest badaczką w Jean Nicod Institute w Paryżu. jej dwa główne tematy badawcze to świadomość ciała oraz poznanie innych umysłów. Pracowała zarówno z wieloma filozofowami (np. Alvinem Goldmanem, Pierrem Jacobem, Adrianem Alsmithem), jak i kognitywistami (jak Marc Jennerod czy Patrick Haggard). Prace de Vignemont można podzielić na teoretyczne i eksperymentalne. Jest autorką jednej z ciekawszych konceptualizacji roli ciała w poznaniu, a z drugiej strony – współautorką interesujących prac eksperymentalnych, będących często dopełnieniem i wsparciem dla jej dociekań. Opublikowała około 50 prac naukowych. Aktualnie pracuje nad monografią *Mind the Gap*.

**Andrew D Wilson** is a Senior Lecturer in Psychology at Leeds Metropolitan University, UK. His broad interests are in ecological, embodied approaches to perception, action and cognition, and his empirical work focuses on the perceptual control of skilled actions, with a particular focus on how we learn to perceive and act in the world.
Blog: *http://psychsciencenotes.blogspot.co.uk/*
Twitter: @PsychScientists

**Andrew D Wilson** jest starszym wykładowcą psychologii na Leeds Metropolitan University w Wielkiej Brytanii. Jego szerokie zainteresowania badawcze obejmują ekologiczne i ucieleśnione podejście do percepcji, działania i poznania, a praca empiryczna koncentruje się na percepcyjnej kontroli wprawnych działań, ze szczególnym naciskiem na to, jak ludzie uczą się percypować i działać w swoim świecie.

**Matthieu M. de Wit** holds an MSc degree in psychonomics from the University of Amsterdam and is currently a PhD candidate in experimental psychology at the Institute of Human Performance, University of Hong Kong. His empirical work focuses on the interrelationships between visual perception and action-examining the visual information used by humans to perceive distance, reach, grasp and use tools. He also studies and writes about theoretical underpinnings of psychological science such as the concept of illusion and philosophy of mind.

**Matthieu M. de Wit** uzyskał tytuł magistra psychonomii na University of Amsterdam, a obecnie jest doktorantem psychologii eksperymentalnej w Institute of Human Performance na University of Hong Kong. Jego praca badawcza skupia się na relacjach między percepcją wzrokową a czynnym rozpoznawaniem informacji wizualnych wykorzystywanych przez ludzi do postrzegania odległości, zasięgu, chwytania i używania narzędzi. Poświęca się również studiowaniu teoretycznych podstaw nauk psychologicznych, takich jak pojęcie iluzji i filozofia umysłu.

**Monika Włudzik** is a doctoral student in literary studies at Nicolaus Copernicus University in Toruń. Her research interests include anthropology of the body and medical humanities. She translates texts on cognitive science, philosophy and practice-based art research.

**Monika Włudzik** jest doktorantką literaturoznawstwa na Uniwersytecie Mikołaja Kopernika w Toruniu. Jej zainteresowania badawcze obejmują antropologię ciała i medyczne nauki humanistyczne. Tłumaczka tekstów z zakresu kognitywistyki, filozofii i badań nad praktyką artystyczną.

**Frank Zaal** is Associate Professor at the Center for Human Movement Sciences of the University Medical Center Groningen, The Netherlands. Frank is interested in understanding natural movements, such as the ones people perform every day. His main research has focused on the tasks of prehension (reaching and grasping) and interception (e.g., catching). He has studied these tasks from the perspectives of Ecological Psychology and Dynamic Systems Theory.

**Frank Zaal** jest adiunktem w Center for Human Movement Sciences na University Medical Center Groningen w Holandii. Zainteresowany jest zrozumieniem naturalnych ruchów, takich jak te, które ludzie wykonują na co dzień. Jego główne badania skoncentrowały się na zadaniach związanych z chwytaniem (jak np. sięganie) oraz przechwytywaniem (np. łowienie). Do zagadnień tych podszedł z perspektywy psychologii ekologicznej oraz teorii systemów dynamicznych.

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