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Commentary on "Cognition and motricity" (Zanone & Hauert)

BIOLOGICAL AND PSYCHOMOTOR HYPOTHESES ARE NOT LOGICAL ALTERNATIVES: THEY USE DIFFERENT LANGUAGES TO DESCRIBE THE SAME SYSTEM

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The usual accepted strategy for research in a new scientific field is qualitative inspection followed by concentration on the simple problems first. This permits rapid progress in research but of course includes the risk of forgetting the existence of the more complex parts of the system. The main message of the target article is that cognitive processes (in the broad sense of the authors' definition) provide a very important or even essential contribution for the development of each motor action and that this has been neglected by most researchers. The reasoning, in an article dealing with motor control theory on this level, has necessarily to be speculative in some parts of the paper but I personally like to agree with most of the suggestions made by the authors. However, the question is not one of agreement or disagreement with the existence of cognitive elements. Rather it is a question of scientific strategy whether it is now appropriate to direct attention strongly towards them or not.

It depends on the actual state of the field whether it is a mistake to forget about the more complex parts of the subject. Nobody would disagree that Newton chose correctly when he decided to neglect those things which complicate everyday movements such as friction, although in modern research on dissipative structures this plays an important role. The authors feel that in the field of motor control it is now time to remind the biologists of the existence of cognitive issues. They correctly point out that researchers thinking in biological terms or in terms of information theory often fail to consider the cognitive parts. This is understandable because models containing memory, anticipation and decision making are, apart from the problem of designing these individual subunits, very complicated. They require much parallel com-

putation and thus, it is extremely difficult to judge the properties of such a complex network.

These difficulties of course should not prevent researchers from attempting to solve such questions and this is the attitude which the authors wish to support. However when stressing the importance of cognitive processes, I see a possible risk. The reader might come to the conclusion that cognitive processes and processes dealing with the "biological machinery" such as reflexes for example are of a fundamentally different nature. He or she might conclude that advocating the cognitive approach provides a logical alternative to the biological approach. This would be a gross misunderstanding. The biological terminology and those of information theory and cognition are simply the elements of three different languages. All can be used to describe the same system. Whereas the terminology of neurophysiology might be considered to correspond to a kind of "machine language", the latter two correspond to higher, problem oriented languages at different levels of abstraction. They are not alternatives in the sense that either the one or the other can describe the system. In contrast, all three can describe the same system at different levels of abstraction. Instead of using them as exclusive languages they might eventually be loosely combined to form a qualitative model which consists of both biological and psychological elements. Such a hybrid model could only be considered as an intermediate step. In its final form a model should possess a uniform structure at one level of abstraction. Before attempting to approach the final model, it is necessary to translate the issues described in different languages into a common language. This means before asking how to connect the biological and the cognitive elements one has to establish what cognitive mechanisms mean in terms of biological, information theory or any other language.

The main question in my view is not so much, "Are the cognitive elements essential?" but "How can those processes which we call cognitive processes be translated into a lower level 'machine" language in terms of information theory or even neuronal terms?". This is a prerequisite if both views are to be combined. The paper encourages us to think along these lines. The most important question remains open, "How does the nervous system perform cognitive tasks?", on other words, "How can we understand cognitive processes in neuronal terms?". Such questions must be answered before we can expect to understand how the cognitive processes modify the different levels of the motor control system.

This difficulty in making clear distinctions between the different languages has in my opinion misled the authors on at least one occasion. As I understand the arguments at the end of sect. 3.2, the comparator only works at higher levels. The possibility of low level comparison (i.e. classical feedback in a servosystem) seems to be excluded. I would prefer to see this possibility retained. Another problem with the paper is that sometimes terms are used in a misleading way. This arises from the common (but incorrect) mixed usage of terms defined for different languages. I cannot imagine how an "online" or "feed-forward" control can work which is able to provide corrections

on the ongoing movement but has no "feedback". In another place feed-forward is contrasted with feedback but the former is assumed to involve a control loop. When strictly applying the terms of information theory this is a contradiction.

In summary, I would like to respond to the article that it is not a question of the relative importance of either "biological" or "psychomotor" hypotheses. Both deal with the same reality, both have their own justification. In the last analysis both must be represented by a common language in order to combine both points.

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