More Synthetic Work Is Needed

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More Synthetic Work Is Needed

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In her article Barbara Webb explores the differences and similarities between the two research programs artificial life and adaptive behavior. Both fields are mainly conducted at computer science or cognitive science departments. She draws attention to the difference between them: research into artificial life involves the invention of artificial animals that do not have to bear any resemblance to either real animals or real behavioral systems, while in adaptive behavior the study of real animal or human behavior is common. It should perhaps be pointed out that for somebody outside these research fields, as we are, the differences between these research programs within computer science is not always so obvious.

There are two central focus points in her article. The first point is a synthetic point, that the studies of invented artificial animals, that is, *animat* research, should be treated as models and thus as theories or hypotheses of animal behavior and not only be seen in its artificial life context. She emphasizes that animat research does represent hypotheses about animals. Therefore, by treating animat research as biological models of behavior more useful conclusions about the biology of behavior can be drawn from research on animats. We agree with the author on this point. It is difficult to see any important value of, say, animat research unless it tries to say something about reality (we recognize that engineering is the motive for some of this research). If it does not connect to reality it resembles some modern branches of mathematics that have lost all such connections.

But, and here comes Webb's second point, for animat research to be fully useful for understanding the biology of behavior she argues that the models should be based explicitly on some real biological system. In contrast to Webb, our opinion is that theoretical work can be done in several ways and serve different purposes which complement each other, and still be grounded in knowledge about real animals.

Let us loosely identify two approaches to theoretical (and also empirical) work on behavior. The *detailed approach* which tries to build a very detailed low level, often neuronal, based theory for behavior in a particular species exemplified by the author's own work on phonotaxis in crickets, and the *general approach* which tries to generate more simple but also general understanding such as standard learning theory.

Webb seems to prefer the detailed approach, which is also the approach of neuroethology (Pfluger & Menzel, 1999). One example of this approach is the model of swimming in lampreys (Grillner et al., 1995). The neural control of swimming was elucidated through a long series of laboratory experiments in combination with neural network modeling. The detailed approach is important because it guarantees, if successful, that nothing is left out and all parts are accounted for. It is also important if knowledge at a physiological or cellular level is sought as such or is needed, as is the case for medical applications. The drawbacks with this approach are related to complexity. First, it is very time-consuming research and we can, at least in a near future, only expect a limited number of studies in comparison with the number of species that exist and the diversity of behavior systems they display. Second, detailed models at the level of neurons are difficult to grasp, penetrate, and use at the behavioral level. Third, it is unclear to what such a detailed account for a particular behavior system of a particular species can be generalized.

Thus, there is also a strong need for a more general and practical understanding of behavior that

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applies both to animal species in general and to different behavior systems (e.g., drinking, feeding, sex, and orientation). Basic learning theory (*Macintosh, 1983; Pearce, 2008*) illustrates this best. It is based on a few basic principles and used daily by millions of people for a number different purposes. For instance, it allows us to train any animal of any species or it is successfully used to treat people with various anxiety syndromes (e.g., behavior therapy and CBT-therapy). It is interesting to note that learning theory developed only through observing behavior. This does not mean that work on a more detailed level cannot generate general and useful behavior-level theory. For instance, we have shown that standard three-layer networks accurately reproduce all major findings about how animals generalize responses to stimulation (Enquist & Ghirlanda, 2005). It is also the case that the model of swimming in lampreys has generated understanding about locomotion and gate control in vertebrates in general.

It seems to us that a pluralistic approach serves science best, but we also want to stress that the theories in any field have little value unless they agree with and provide an understanding of reality.

Webb also touches on a bigger and more important issue. At least we think so. The research into behavior and behavior mechanisms suffers from a fragmentation into a significant number of different subjects and research programs. Today research about behavior and its mechanisms include, for example, ethology and animal behavior, behavioral ecology and evolutionary biology, several research fields within animal and human psychology, neuroscience, and computer science.

We agree completely with Webb that research in adaptive behavior and artificial life are of interest and importance for understanding animal and human behavior, and also evolution. However, reference to such research is often more or less absent in the fields that we represent (ethology and evolutionary biology) and when we read work on adaptive behavior and artificial life we see the opposite pattern. It is clear that cultural differences emerge quickly between subjects and research programs. Take just one example. The word adaptive does not mean the same thing in evolutionary biology and within adaptive systems research.

How can we change this development? How can we promote more recognition, cooperation, and synthetic work among disciplines? We therefore agree with Webb and her passion for wanting to synthesize different, but yet so similar fields. We think this should be encouraged, but here we call for a synthesis of a bigger scope. It is obvious to us that while cognitive science had the ambition to bring all this research together, it unfortunately failed.

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