Adaptive Behavior

Never Mind the Iguana, What About the Tortoise? Models in Adaptive Behavior

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1 Tortoises

Grey Walter's pioneering use of his very early mobile robots, or "tortoises," to investigate mechanisms underlying the generation of behavior (Walter, 1950, 1951), is rightly lauded as one of the key starting points for the kind of research championed by this journal. His tortoises had all the hallmarks of what later became known as animats. Walter's work has had a huge influence over the decades, both directly and indirectly. Although they were explicitly intended to shed light on animal behavior, his models did not have tightly specified animal targets: they were made-up artificial creatures. They do not meet the harsh criteria for biologically relevant animat models suggested by Barbara Webb in her well argued and provocative article. Webb suggests that "animat simulations, if they are to be relevant to biology, should be considered as models" (sec. 5) and that for such models to be useful they must be grounded in real target animal systems; to not do so, she claims, undermines any justification for their relevance. While science must ultimately be empirically grounded, and the majority of modeling should probably be of the classical kind supported by Webb, I suggest that other kinds of more speculative, less grounded models also have an important role to play.

In the first of his famous *Scientific American* articles, Walter (1950) described how his simple three-wheeled robots, sporting a protective "shell" and equipped with a scanning light sensor, a touch sensor, a propulsion motor, a steering motor, and an electronic valve based analog "nervous system," were used to "discover what degree of complexity of behaviour and independence could be achieved with the smallest number of elements connected in a system providing the greatest number of interconnections" (p. 44). The tortoises had two artificial neurons connected in such a way as to be capable of sustaining a range of interaction patterns. Contrary to the then prevailing view that large numbers of neurons were necessary to generate behavioral complexity of any degree, Walter was able to demonstrate surprisingly intricate phototaxis-based behaviors in a pair of interacting robots controlled by the minimal nervous system.

The second article (Walter, 1951) outlines experiments with a later generation of robots that were now equipped with extended, although still relatively simple, artificial nervous systems capable of exhibiting learning behavior: a conditioned reflex. Walter demonstrated, for the first time, a neural-like mechanism that generated interesting conditioned reflex behavior in an embodied autonomous sensorimotor system. Although the work was rooted in a close study of Pavlov's original conditioned reflex studies (Pavlov, 1927), Walter's model did not have a specific animal target; it was of a more general, exploratory nature. As he put it, "These models are of course so simple that any more detailed comparison between them and living creatures would be purely conjectural" (Walter, 1951, p. 63).

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2 Models

One of the reasons Walter's models attracted so much attention at the time, and have continued to wield influence, is that they are prime examples of a use of models long recognized as important in psychology and cognitive science, and by philosophers of science (Dennett, 1994; Schlimm, 2008), but apparently given short shrift by Webb: models as existence proofs. The existence of models can help to refute, or at least cast doubt on, certain claims about necessary conditions for phenomenon (e.g., that large numbers of neurons are necessary to generate behavioral complexity). They can also demonstrate new possibilities (e.g., neural-like circuits capable of supporting conditioned reflex learning in an autonomous sensorimotor system). As long as such models are deemed relevant enough to the particular branch of science in question, they can open up the intellectual landscape, catalyzing new research and helping to shape novel questions. The crucial issue of what *relevant enough* might mean is discussed later. While Walter's models may not have led in a simple linear way to new understandings of a particular animal, they did change the intellectual landscape and encourage other scientists to think in new ways. They were particularly influential with young open-minded scientists, many of whom were excited by the creativity of the work. Richard Gregory, who went on to become a very prominent experimental psychologist, was greatly affected by the spirit of Walter's work (R. Gregory, personal communication, 2002); Walter Freeman, the well known neuroscientist, has often acknowledged the formative influence of Walter on his thinking (Freeman, 2007), as have Harry Barrow (personal communication, 2004) and Rod Brooks (Brooks, 2002, p. 27), who both became leading AI and robotics researchers, to name but four.

In a similar way, Ashby's Homeostat (Ashby, 1948, 1952), an electromechanical device that demonstrated a self-organizing theory of adaptive behavior, and which was almost as famous as the tortoises at the time, had an important influence on several people who went on to play leading roles in neuroscience and machine learning (J. Cowan, personal communication, 2004; Cowan, 2008; Freeman, 2007) and continues to inspire to this day. Again, while explicitly intended to help explore theories of animal behavior, Ashby's model was not empirically grounded in studies of a specific animal. These kinds of models often play a particularly important role in the early development of a field—Walter's and Ashby's models are good examples of that—but they can have powerful catalytic effects at any stage.

A closely related kind of model that does not require any direct representational function, but is used in all branches of science, is the *toy*, or *probing*, model (Frigg & Hartmann, 2008). Such models are not intended to represent anything real, but to be used as uncluttered vehicles for testing new tools and methods, preparatory to more detailed empirically based modeling (Hartmann, 1995). This is exactly the justification Randy Beer gives for his minimally cognitive agent work (Beer, 2003), which Webb has used as the main exemplar of the kind of model to which she objects. He explicitly states that "The intention here is not to propose a serious model of categorical perception, but rather to use this model agent to explore the implications of dynamical explanation for cognitive agents." (Beer, 2003, p. 210), and exhorts us to "Think of this exercise, then, as a form of mental calisthenics, an intellectual warm-up for the dynamical analyses of a wider range of agents and behaviors." (Beer, 2003, p. 210). For an area as difficult and underdeveloped as dynamical analyses and explanations of embodied situated agent behavior, Beer's justification seems appropriate and pragmatic. The reason toy models are used in physics is the same reason Beer uses one: their relative tractability. Webb wonders why he does not use a highly simplified or minimalist model of a real animal. That would certainly be a valid alternative approach, but the difficulties inherent in distilling such a model may distract too much from the intended warm-up nature of the work. Such a model might be more appropriate at the second stage of the research program once the toy modeling has helped to develop tools and clarify ideas. Indeed, contrary to Webb's belief that "the animat conception of an invented animal often seems like a convenient way to put off this [empirical] testing indefinitely" (sec. 4.1), Beer's work has inspired others to start to move to the next stage where models like his are developed in relation to specific empirical studies, for example, Rohde and Di Paolo's work on minimal models of human perceptual crossing (Di Paolo, Rohde, & Iizuka, 2008; Rohde & Di Paolo, 2008).

An interesting associate of the toy model is the false model—a model of something known to be wrong—which can have a useful heuristic role in refining and developing "true" models by elaborating their underlying assumptions (Wimsatt, 2002). Models can also act as substitutes for theories where none exist, a situation common in cognitive science and biology, and, in the case of computational models, as a kind of animated thought experiment aimed at clarifying conceptual issues.

There is insufficient room in this article to discuss the full range of models used in science, and in particular in biology, but the fact that there is such a multiplicity of model types and model uses has prompted a growing number of philosophers of science to point out that there is no simple standard received notion of what a model is or should do (Beatty, 1997; Frigg & Hartmann, 2008; Odenbaugh, 2005).

3 Biological Relevance

Central to Webb's thesis is the claim that for an animat model to be relevant to biology it must be grounded in a specific target animal system(s). By extension I assume she therefore suggests that for *any* kind of model to be relevant to biology it must have a specific animal target. Remembering that science is a social activity in which communities collectively decide on what is relevant or interesting or worthwhile, it is clear that biology as a field does not agree with Webb. Although they are (rightly) in a minority, there are many examples of abstract or toy models appearing in the most prestigious journals. They are deemed important. The question of relevance is not a simple cut-and-dried matter and does not necessarily require a specific animal target. The evidence suggests, not surprisingly, that more abstract models are often used in areas that are theoretically underdeveloped and in which data is sparse and hard to come by.

The development of individual-based models in evolution and ecology is an interesting example. Some of the best-known early modeling of this kind was highly abstract and often not empirically founded (e.g., Nowak & May's, 1992, work on spatial prisoners' dilemma models as a metaphor for issues surrounding the evolution of cooperation), but it was deemed interesting and relevant by the community. As individual-based modeling has taken root in ecology, there has been a trend towards greater empirical grounding, but some, that part concerned with big theoretical questions, remains abstract while still being embraced by the field (Grimm & Railsback, 2005; Huston, DeAngelis, & Post, 1988).

Neuroscience, much of which is ultimately concerned with understanding the generation of adaptive behavior, is another area in which abstract models are rife, again mainly because of the lack of overarching theories and the difficulty of obtaining detailed data on many aspects of the operation of neuronal networks. An example with which I am very familiar, and which has clearly led to a useful advance in scientific knowledge, is the modeling of NO volume signaling by Philippides et al. This modeling was abstract, in that it was not grounded in a particular animal system—made up neurons and neural structures were used—but its parameters were partly based on what little data was available. Those few parameters gave it relevance, although of a kind that seems to me to be significantly weaker than that demanded by Webb, but which was, however, deemed strong enough by the wider neuroscience community. Because it illustrates how speculative, abstract modeling can lead to an advance in empirically based understanding, it is worth briefly describing the arc of the NO modeling work.

A few years ago Philippides, Husbands, and O'Shea (2000) published a paper describing a model of the diffusion of the gaseous messenger NO from neural sources. By using more advanced methods than in previous work, it was possible to highlight the important influence of the geometry of NO sources, providing insights into the four-dimensional spread of a diffusing messenger. The appearance of this theoretical paper prompted communication from a number of experimentalists who were interesting in a deeper understanding of NO generating meshworks of fibers they had observed in both vertebrate and invertebrate brains. Because size severely limits the signaling ability of an NO-producing fiber, the predominance of fine fibers seemed paradoxical. This led to further modeling work (Philippides, Ott, Husbands, Lovick, & O'Shea, 2005) that showed how cooperation between many fibers of low individual efficacy can generate an extensive and strong volume signal, and that signals generated by plexuses of fine fibers were better centered on the active region and less dependent on the particular branching morphology of the mesh. This second stage of work was more empirically grounded than the first but was still abstract and did not claim to be a model of a particular system, rather it was intended to show how cooperative signaling could arise. In turn this work led to further theoretical and empirical investigations that have recently provided experimental evidence for the kind of cooperative signaling predicted by the 2005 model (Steinert et al., 2008). While once a largely atheoretical subject[1], neuroscience now has an increasingly enlightened attitude to many kinds of models.

A current example is the very interesting work by Fernando, Karishma, and Szathmáry (2008) that proposes a mechanism for copying neuronal networks that could act as a basis for causal inference, function copying, and natural selection within the brain. The model, although speculative, and at this stage ungrounded empirically, is nonetheless firmly based in theoretical work in neuroscience. Again, the biology community deemed it relevant and interesting even though it is not tied to any real target system. Implicit in this acceptance is the belief that the appearance of such models will lead to the search for empirical support, as illustrated in the previous example.

Grey Walter partly justified his tortoises by claiming that "These machines are perhaps the simplest that can be said to resemble animals. Crude though they are, they give an eerie impression of purposefulness, independence and spontaneity" (Walter, 1950, p. 45). Although the hard-line empiricists of the day disagreed, a large enough section of the scientific community found this claim convincing; hence their lasting influence and impact.

4 Conclusion

While I am sympathetic to much of Webb's viewpoint, I think her very useful article goes too far. Poorly motivated, meandering modeling is to be discouraged, and in most cases the aim should be to use, or at least work toward, empirical grounding. But by being over-prescriptive about what kinds of models can and cannot play a role in adaptive behavior research, there is a great danger of stifling creativity and imagination, of cutting off avenues for the kind of alternative thought that is surely necessary to keep science vital. There should always be room for abstract, toy or metaphorical models that introduce new and intriguing ideas or methods. Whether or not those ideas or methods lead anywhere is another question, and one that will often take decades to answer. But to dismiss such work as a convenient way to put off empirical testing indefinitely, seems to me to be unnecessarily impatient as well as to deny the reality of how science actually works.

Note

1 According to Oliver Selfridge (2008), who collaborated on the work, an article as historically important as *What the frog's eye tells the frog's brain* (Lettvin, Maturana, McCulloch, & Pitts, 1959) was rejected from the *Journal of Neurophysiology* for being too speculative and "not having real data." Eventually published in an engineering journal (albeit a very prestigious one), the article changed the way we think about seeing and went on to become one of the most cited publications in the field.

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