Iguana modelling is not the only game in town

Commentary on Barbara Webb's paper
"Animals versus animats: or why not model the real iguana?"

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We agree with Webb's point that discussions of the importance and novelty of "synthesis" in ALife and AB have sometimes obscured the fact that these are fundamentally model-building enterprises. It's true that standard practice in ALife and AB is to construct a program or a robot, and on the surface this can look different to the theories-and-experiments approach of normal science. The picture is further complicated by the fact that many researchers have engineering goals as well as biological ones. However, to the extent that ALife and AB are going to teach us anything about biology, it's important to recognize that they will do so not by becoming independent of laboratory and field work but by giving us new ways to understand empirical data. Webb is right to remind us all of how contributions to biology are made.

It follows that we applaud Webb's attack on the ontological mumbo-jumbo of the strong ALife position. "It's alive!" is the Frankensteinian claim of the strong ALifer. Like Webb, we cannot agree. It's not alive, it's a formal model instantiated as a computer program, and the map should not be mistaken for the territory. ALife's ability to ask counter-factual questions (e.g., "what would it be like if there were more than two sexes?") no more licenses talk of a new, alternate reality than does the ability of a mundane traffic simulation to ask "what would London be like with more motorways?". Webb is right to point out that some ALife and AB authors have used the "life as it could be" position to dodge their responsibility to make reference to real biology.

We further agree that Webb offers the right rebuttal for those who want to use ALife creations as "source models", in the same sense as using a pump as a model for the function of the heart. (Webb suggests this interpretation of Langton's 1989 "alternative biology" position.) An ALife simulation is not an independent real-world object but an artifact that has been constructed with strong influences from biological theory. It is therefore already a theoretical model, and we should not be surprised when it turns out to resemble the real-world target system in some way.

Thus we find much to agree with in Webb's paper. However, we are uncomfortable with the narrowness of a view on modelling that dismisses Beer's (2003a) work on minimally cognitive agents. Frankly, we are suspicious of an analysis that places the over-complex, over-optimistic multi-scale modelling effort of the *Psikharpax* project (Meyer et al., 2005) in the "good" column

while placing Beer's simplicity, clear reasoning, and careful analysis in the "bad" column. We see Beer as part of a long tradition of useful, simple and abstract models in the cognitive and biological sciences. So where do we part company with Webb in her view of legitimate modelling practices?

Webb argues that model builders must have a target system in mind, they must develop a hypothesis about how that target system works, and they are then justified in building a model if the hypothesis is complex enough to be non-obvious in its implications. In other words, the model's purpose is to spell out the consequences of the various assumptions in a complicated theory. We think that's not a bad description of what modelling is (although see Di Paolo et al., 2000, for a more complete account) but we disagree with Webb on the range of epistemological payoffs one can expect from this activity. For Webb, the result of a successful modelling venture is increased support for a particular view of the rather specific target system, e.g., one becomes more confident that cricket phonotaxis actually works thus rather than so. We believe that there are other things that can usefully be done with models. For example, building speculative or highly abstract models that are designed to develop or to "try on for size" a new paradigm seems to us legitimate. Of course, this is what Beer is doing: his over-arching goal (Beer, 1997) is to use models of simple agents to explore the utility of the dynamical systems perspective as a new way of understanding cognition.

It would be a shame if Webb's restricted view of what models are good for was to become universally accepted in the ALife and AB community. Of course, models of specific behaviours in specific animals, with model outputs being compared directly to empirical data, are a valuable contribution, and Webb is probably right that it would be healthy if there was more work like this in the literature. However, ALife and AB are well placed to do more than this. Traits, capacities, and behaviours that are shared across many organisms (e.g., the capacity to hear, anti-predator observation, possessing an immune system, etc.) can be investigated in general and their likely evolutionary histories outlined in advance of more detailed investigations with reference to a particular species. These kinds of simple, general models can be especially useful in clarifying conceptual issues on the border between theoretical biology and the philosophy of biology, e.g., the origin of intelligence (Godfrey-Smith, 1998), the relationship between agent-level and

mechanistic descriptions of behaviour (de Pinedo & Noble, 2008), and the possibility of selection occurring at levels other than the gene (Powers, et al., 2008).

We think that the problem with Webb's analysis is rooted in her dichotomy between "specific" and "invented" animal models, and we reject this dichotomy. There is a presumably unintended pun at work when Webb talks about "specific" animal models: all of her examples are pitched at the level of species. We note that classical (non-computational) models from game theory, such as the Hawk-Dove game (Maynard Smith & Price 1973), have been usefully applied to real biology but cannot be said to be models of a "specific animal". Does Webb intend to privilege the species as the only level of biological organization at which she thinks useful models can be made, or is this a coincidence of the examples she has chosen? We are unsure, but we cannot see why much more general categories (e.g., animals or plants) are not also valid objects for model-building. Consider von Neumann's model of a self-replicating machine, originally presented in lectures in 1948 and 1949, and surely a progenitor of modern ALife models. von Neumann showed that a self-replicating system needs to be able to treat key parts of its structure as both code and data, and in doing so he anticipated Watson and Crick's results on the double helix by several years. von Neumann's machine is not a model of any particular animal; it is an abstract model of an invented system, exploring the general logical requirements for self-replication. Is it worthless as an exercise in modelling? We think not.

Webb tolerates a certain amount of invention or creativity in model-building, in that she accepts that models may be imperfect or imprecise or pitched at differing levels of explanation, and that a hypothesis may sometimes be just a wild guess about how some real-world process might work. On this we agree completely: there is more value in being usefully wrong than being silent. We suspect that Webb is annoyed with "invented animal" work because of a tendency in the ALife and AB literature to noodle around with model-like structures that are not true models but undirected fictions. Certainly the language used in Dennett's 1978 paper now seems inflammatory, and if Dennett is taken as saying "Why bother with the biological details, let's just make something up" then we can see why Webb has a problem with this. It's true that whereas tightly targeted work like Webb's own has the obvious success criterion of being a good or a bad match to real data, some of the more speculative ALife and AB work has much more vague

success criteria: how long should Beer's modelling project continue before the weight of evidence pushes us one way or another on the utility of the dynamical systems perspective on cognition, for example? This is a tough question, but we feel Webb is wrong to reject everything more general than empirically linked species-specific modelling simply because the success criteria for the more speculative work are not as crisp as we might like them to be. The problem is not that proofs of concept and conceptual exploration are worthless, far from it. The problem is perhaps that some researchers in the ALife community seem to be happy to keep re-discovering the same principles again and again: for example, we do not need any more papers where the chief contribution to knowledge is a demonstration that complex global behaviour can arise from simple local rules.

In conclusion: we agree that ALife and AB is, or should be, all about model-building. And we want to stress that if Webb's paper is read as a cry for more rigour and less chaff in the ALife and AB literature, we very much agree. We feel, however, that Webb is unfortunately prejudiced in favour of the level of precision and detail that she uses in her own modelling projects. Levins (1966) and other analysts of the modelling enterprise have outlined dimensions that must be traded off when building a model, e.g., in Levins's case, precision, generality, and realism. Points other than Webb's in this space of trade-offs are also legitimate.

References

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