<u>Tool-makers versus Tool-users: Division of Labour</u>

<u>Inman Harvey</u>

Preprint version: to appear in Adaptive Behavior 17(4), 2009, in response to Barbara Webb's article, "Animals versus animats: or why not model the real iguana?"

I sympathise with the many of Barbara Webb's concerns. Having taught a course on Artificial Life to postgraduate students for a decade, my over-riding and recurring problem each year comes from those students (particularly computer scientists) with an alarming inability to distinguish between their idealised models and any reality these models might refer to. And yet --- I think BW comes to an unjustified overall conclusion, and perhaps makes a convoluted version of this error herself.

There is nothing more annoying than submitting a paper for review on ones favourite topic P, and receiving a detailed response on just how badly this fails to deal with, or perhaps even acknowledge, the reviewer's different concern Q. In inter-disciplinary areas such as Adaptive Behavior, and Artificial Life, there are many overlapping or neighbouring concerns; mathematical, analytical and computational tools may be used, issues in physics, chemistry, cognitive and biological sciences may all be present, and any one topic may be viewed from a multitude of vantage points. In written and verbal communications between people there is so much scope for this P-Q confusion; and further, unless an individual carefully analyses their own mixture of motives, there is much scope for confusion and muddled thinking even within a single mind.

I suggest that much of this confusion can be avoided if we clarify an important distinction between Tool-making (P) and Tool-using (Q). I take it as a given that models are tools. A carpenter may occasionally make her own chisels, and a chisel-maker may sometimes build his own cupboard; but the criteria for good chisels are different from criteria for good cupboards, and division of labour allows people to focus on what they are most interested in, and perhaps on what they are best at. Unfortunately, the notion of model-making can cover both the development of modelling techniques (P), and the application of such techniques to modelling real phenomena (Q), and this distinction is sometimes blurred.

BW cites Taylor (1989) making much the same point about ecological models as 'exploratory tools' for 'mathematical investigations'. BW comments that "… it seems reasonable to say that until or unless these explorations are used to make empirical claims about real life they are not biological science." I can agree with that, in so far as chisel-making is not carpentry. Nevertheless, chisel-making is relevant to carpenters, and the properties of woods are often of interest to chisel-makers (though they may also have other customers who sculpt stone).

BW acknowledges this, and accepts the value of ALife models such as cellular automata, or that "an artificial system built for an engineering purpose, or as a 'pure' mathematical exploration, might subsequently be usefully compared to a biological system that carries out a similar function." However, in moving on to discuss work by Beer, she is uneasy about the legitimacy of models that "are constructed to represent (however loosely and abstractly) some mechanisms taken, by hypothesis, to have causal relevance to biology" *unless* there is "some explicit specification of how the two systems -- artificial and biological -- are supposed to correspond."

Here I believe BW is propagating an error similar to that of computer scientists, confusing models with reality. In my language (though in the following paragraphs I worry about other practices), when we talk of an ant as a biological system, we are already referring to a model-of-an-ant, with which we hope to understand some aspects of how a real, flesh-and-blood (or exoskeleton-and-haemolymph) ant functions. It is part of the job of a biologist to test and validate such models by comparing their predictions with experimental data from observations of the real ants. But when we introduce an abstract ALife model of an ant, we now have 3 entities:-- (P) such ALife artificial systems, (Q) possible biological systems, and (R) the real ants. The validation relationship between the artificial and biological systems is a theoretical one; whereas that between the biological system and the real ants depends on careful, painstaking observations in the field or in the lab. A biologist is ultimately only interested in relating some appropriate (Q) to (R), whereas an ALifer may be focussing on the abstract properties of (P), and the relationships between (P) and (Q).

Here I must highlight a potential misunderstanding, due to ambiguities and confusions with terms such as 'system' and 'model'. Somebody who writes computer simulations can initially be puzzled when a medical researcher refers to a (real) rabbit as a model; but comprehension dawns when it is understood that this means that, for the purpose of testing new drugs, a rabbit can be expected to react in somewhat similar ways to a human, and is thereby a *model* of the *target* human. In section 3, BW refers to the cricket as a 'model animal' for investigating auditory behaviour; as with the rabbit, the cricket can be used to predict how a wider range of species may react. Turning to the word 'system', people who use ideas from Dynamical Systems (DS) in their work, as do many that BW cites, make a very clear distinction that a *system* is in this context a type of formal model of some *target*, consisting of the variables that have been chosen as relevant and the mathematical relationships between those variables. The *system* is the model, and so the *target* is not a system (unless in turn it is a model of something else).

BW does say in section 3 that 'It [a robot] is a model by reason of its intended use'. I would completely agree with this, in accord with the notion of a model as a tool; it follows that when different people have different intentions, confusions may arise if they assume otherwise.

I have followed the DS usage in distinguishing between the biological system as model-of-the-ant and the real-ant-itself as target. But I see that BW often uses these words differently: in section 4.1 referring to the 'mapping between [Beer's] agent and *real cognitive systems*', in section 4.2 in discussing 'minimalist' idealisations as 'a description of the *real system*' and then again a '*target system*' [*italics added*]. At a minimum this leads to potential confusion when debating with users of DS theory, and it could be symptomatic of some basic ambiguity and confusion between models and targets-of-models. When BW compares and contrasts 'artificial and biological systems', I am pretty certain that by artificial systems she means models, yet we are uncertain whether by biological systems she means real organisms or models-of-them; since Q can interact each way in the P-Q-R triad, and since crickets can sometimes be the target of a biological model, and sometimes themselves be the model for a wider range of species, it is easy to get confused. To clarify my own usage, I am definitely using biological systems to mean biological models; a perspective on real animals (real plants, communities, ecosystems) that treats them as an assemblage of interacting parts.

What is the difference between a real rabbit (R), and a biological model of rabbits (Q)? The former cannot be misleading (… unless it is being used itself as a model of a human!) but the latter can be misleading (through failing to act like rabbits, as tested against the data). How is an ALife model (P) different again? It can be useless (if it fails to have any power to influence any biological modeller), but that is not the same as misleading. The real rabbit does not need any criterion to justify it; the artificial and biological systems have differing criteria for their differing roles.

John Maynard Smith used to complain about 'Fact-Free Science' in the context of some work at the Santa Fe Institute, and ALife generally (Maynard Smith 1995):
"But first I must explain why I have a general feeling of unease when contemplating complex systems dynamics. Its devotees are practicing fact-free science. A fact for them is, at best, the output of a computer simulation: it is rarely a fact about the world."

In his less guarded moments, he went further (quoted in Brown, 1999, and I can confirm personally!): *"Absolute fucking crap. But crap with good PR."* Much of JMS's own work would count as using ALife-style models of an abstract nature, but his personal motivation was always to pursue their use as biological models right through to the testing in the world. In so far as a model claims to directly apply to the world, but fails to be tested against it, then it is open to such trenchant criticism. Yet if the motivations are different, and are defended as such, it is absolutely justified to focus on one part only of this process, namely the development of abstract model-types as potential tools for the real biologists to develop their biological models. I fully sympathise with both BW and JMS in objecting to any ALifers who might claim to be doing biology without doing the biological work; there remains a large body of ALife work that is more cautious in its claims.

BW discusses the arguments in Harvey *et al.* (2005) that many animat simulations are intended as existence proofs, or proofs of concept, rather than accounting for data. Here again she falls into the same trap as our naïve computer scientists, in confusing a biological model (or system) with the real living entity. She characterises the argument as: "A researcher could thus, for example, refute a claim that a (biological) phenomenon X requires condition Y by showing that an animat can produce X without Y", but then is in error when she claims that such 'existence proofs' require comparisons between model results and empirical data. Such existence proofs deal with the validity of mathematical and/or logical claims about *models,* including here *biological models*, and do not directly deal with empirical data at all. Harvey *et al.* (2005) do indeed, as BW notes, talk of the need to appropriately translate novel hypotheses to domain-specific cases and test them empirically -- in order to complete the scientific function of explaining phenomena in the real world. But existence proofs on their own are not intended to carry through that complete scientific function to its empirical conclusions. They are aimed at the hypothesis-production end of this endeavour (relating Alife models P to possible biological models Q), and should not be criticised for failing to do something very different (such as relating Q to the real organisms R).

To give an example, neural network models are commonly used to model learning behaviour via mechanisms that model synaptic weight changes. In consequence, many people have considered that CTRNNs (that have fixed weights on links between the nodes) are incapable of modelling learning mechanisms; even when it is explained that the values of nodes in a CTRNN can be used to model *any* variable, including a synaptic weight, in the target being modelled, this is found by many to be a difficult concept to grasp. In Izquierdo-Torres and Harvey (2007), it was shown that CTRNNs with fixed weights can indeed be evolved to produce Hebbian-like learning. Now this existence proof neither made nor needed to make any comparison with empirical data, since it was not intended as that sort of biological model; rather, it demonstrated both to abstract theoreticians and to those other people who *do* want to make biological models that they should not dismiss the use of CTRNNs for invalid reasons. One can explain and demonstrate the potential of a specific chisel, indeed speculate on possible new cupboard designs, without having to actually build a cupboard; it is a different job, but can still be valuable.

In section 4.1, where BW discusses similar points that Beer makes, she tries to be fair by acknowledging that Beer is using an existence proof in this fashion, but still worries when he explicitly uses a minimalist invented animat. "But why could this not be attempted using a highly simplified model of a real animal, rather than an invented one?" she asks. But this is to miss the point. Models are tools, and the development of tools is a worthy exercise in its own right; many of us seek more elegant and powerful tools, and to understand more fully the sorts of operations they can and cannot do. Newton and Leibniz developed the differential and integral calculus, and the fact that 'no animal was harmed', no experimental data checked, in its development should not mean that biologists refuse to take advantage of such enormously useful tools to help them construct their own more specific tools (models) aimed at the specific biological phenomena they wish to understand. ALife models may not be as grand and general as the calculus, but I suggest that they should be assessed by comparable criteria.

In summary, if BW is expecting delivery of a cupboard, and she only receives a chisel, then she may well be annoyed. However, if it is clear that the chisel is what is actually on offer, she should judge it on how useful it is to her and her carpentry, and to others who may explore different uses for it; and on its elegance and its capacity for inspiring new techniques. In turn, a chisel-maker should make it clear what they are delivering, but will then be delighted if a new type of chisel makes possible the design of more elegant or more functional cupboards in new materials. Both sides can benefit from such division of labour. Let a Thousand Flowers Bloom!

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