A surfeit of models
Edward S. Reed


In his contribution to Modelling The Mind, P.N. Johnson-Laird, arguably the doyen of experimental studies of reasoning, warns that "people... usually hold that a conclusion is beyond reasonable doubt when they have exhausted those possibilities that are obvious (to them). It is easy to show that this judgement is often premature." This kind of 'argument from exclusion' is a common danger in scientific as well as in ordinary thinking. A case in point are the two volumes under review, which pose for readers the much-discussed choice of mental versus neural modelling. For three decades or so, cognitive scientists have been pursuing the implications of the assumption that the mind is analogous to a (digital) computer. Despite some important successes, this point of view is at present under strenuous attack, even being supplanted in certain research areas by 'connectionist' approaches which assume that the brain is a (massively parallel) computer. Modelling the Mind focuses on the mind-computer analogy, although most of the chapters at least acknowledge the potential importance of connectionism. By contrast, throughout The Computing Neuron it is simply assumed that the brain is a computational network.

Reading the two in tandem convinces me that the choice between mind-as-computer and brain-as-computational network in no way covers the possibilities, and that what is needed in cognitive science are theories that use the great wealth of available psychological and neurophysiological data to explore alternatives to computationalism. One can agree with the connectionists that the mind-computer analogy has outlined its usefulness without jumping to the conclusion that brains are computers. After all, these analogies are useful only insofar as they enable us better to understand psychological or neural processes, and it is easy to feel that we have come to the point of limiting returns in both these areas.

Biological honesty

One of the goals of The Computing Neuron is to fashion a connectionism that is biologically honest. Each editor contributes a useful introduction: Durbin on the relation of hypothetical neural units to real neurons, Miall on variation in neuronal properties, and Mitchison on learning networks. Unfortunately, none of them and few of the contributors succeed in connecting the very real powers of their neural nets to principled accounts of biological behavioural function. They seem to think that models can float free of either biological or psychological theories so that, if the hypothetical units of their nets have some of the properties found in some neurons, then this counts as biological realism. The authors show a lack of concern for using models to test behavioural theories and hypotheses. Time and again the reader will wonder what the model being developed is a model of and — more important — why this particular capacity was singled out for modelling.

The more interesting models discussed in the book all derive from strong theories, precisely because these theories provide powerful constraints on the testing and evaluation of models in their domain. Koch and his colleagues, for example, develop a neural model for the detection of optic flow which, thanks to the work of perception theorist J. J. Gibson, we know is a fundamentally important visual process. There are several algorithms available that can accomplish the derivation of flow from image sequences, and Koch's group takes one such algorithm and integrates it nicely with the wealth of neurophysiological data about primate visual cortex. In particular, this group pays careful attention to the kind of 'population coding' by which features of stimulation seem to be extracted in the sensory cortices. This is a model which can be checked against robust data from research on visual perception and neurophysiology. Yet even in this case, there is a remarkable lack of concern for some basic issues in functional biology. For example, despite these authors' care to match the operating principles of the visual central nervous system, they use a model of retinal image optics that is totally at variance with the facts of the ever-exploring primate visual system. The book includes a brief chapter on the salamander retina, but no useful information on ecological optics or on the comparative morphology and physiology of ocular image formation, without which it will be impossible to produce biologically realistic models of visual perception.

Evolved systems do not exhibit rational design properties. One of Darwin's most important contributions was to emphasize how widespread are dysteleology and historical accident in nature. Only one of the chapters in The Computing Neuron, Robertson's fine brief review of the neuronal circuitry underlying locust flight, emphasizes this darwinian point. Yet if central nervous systems have been evolved by natural selection, then, as Robertson aptly puts it, "many details of [their] operation may be suboptimal or baroque", historically derived contrapitions. Hence, the precise modelling of a given behaviour is of dubious value because a great deal of the detail will be more relevant to phylogenetic idiosyncrasy than to neurobehavioural principle. Robertson drives this point home with a diagram of the great variability found in an indentified neuron in five individual locusts. These insects' neural nets are composed of individual cells of such variety that prediction of circuit properties of these 'simple' nervous systems has not been possible. This failure of prediction is worth remembering when evaluating any of the successful but post hoc neural-network models.

Local variability

The state of the art in neuronal modelling is thus highly paradoxical: network models of processes as complex as reading exist side by side with serious failures to elucidate principles even in simple invertebrate systems where many neurons and circuits have been identified and characterized. Until biological realism means more than having hypothetical units copy features of neurons, such paradoxes will be commonplace. Neural modellers need to address such questions as why many features of neural units and connections can vary between individuals without compromising the species' neuronal functioning. The local variability of nervous systems is not noise to be added to one's model of nervous systems but a biological principle that requires explanation.

Modelling the Mind, by contrast, is concerned with psychological function at a fairly high level. Considering the scope and ambitions of some modellers, it is surprising how infrequently the question "what is it we should be modelling when we model the mind?" is asked. Only Wilkes raises it in this collection. She uses the example of 'memory', pointing out that this apparently simple mental process is now widely seen as being a conglomeration of different capacities (short or long term, semantic or episodic, item or auto-biographical and so on). "We cannot say that we have another instance of 'the same' capacity in a computer until we know what it is that we have got." In general, the psychological processes which we find most intriguing — imagination, thinking, perceiving and so on — are described by words for which the references are not clear. As Davidson explains in his marvellous critique of Turing's
test', we know these capacities when we see them, but we cannot make explicit what it is we know. This interperson­ral recognition is yet another of these marvellous psychological skills. If David­son is right, our intuitions, which emerged during the evolution of ourselves as social primates, may work for creatures like ourselves but are suspect when applied to different entities like computer models of the mind. The moral of this is not despair, but the need to develop hypotheses that are precise enough to be tested about these intriguing psychological processes. This sort of nonglamorous hypothesis-testing has been the mainstay of not a few cognitive psychologists (such as the work on reasoning and thinking reviewed here by Johnson-Laird and Evans) but it seems that neuroscientists and artificial intelligence researchers are reluctant to take advantage of the gains of their psychol­ ogist colleagues. And some philosophers seem willfully ignorant, as witness the nonsensical chapter by Boden in which she uses Spinoza as the model of scientific psy­ chology preceding artificial intelligence, ignoring three centuries of research.

Noble's intriguing essay comparing Hodgkin–Huxley type models of the nerve impulse to computer models of the mind suggests that without rigorous theories of psychological functioning we may not even be able to interpret the models of lower level processes we already have. The Hodgkin–Huxley model would certainly be of far less interest or use if we had not already a considerable body of theory and data about the higher-level, integrative functioning of the nervous system, within which context the model could be interpreted.

There is no easy short cut to a science of the mind, computer technology and its advances notwithstanding. What is needed in cognitive science is good thinking tempered by careful research unhindered by restrictive models and analogies. After this, modelling might help to clarify some issues. For Johnson-Laird, everyday mental models of the world are acts of construc­ tive imagination, but he emphasizes that it is imagination that is central in these models, not inference. Similarly in science, much of the effort of thought lies in the theoretical imagination, with the kind of inferencing characteristic of formal models playing a secondary role. Cognitive science has had far more than its share of modellers, but precious few thinkers and theorists. If ever there were a task that required good imagination intermingled with hard thought, it is the job of understanding the human brain and mind.

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**Star exhibits**

A.C. Fabian


We are taken on a tour of a museum by an amiable guide. In this cabinet we have a red giant, over there we have the Hale telescope and just come and look at the solar flare round the corner. Here we have the eminent Soviet astrophysicist about whom it was said that “fifty per cent of

what he does is brilliant but no one can tell which fifty per cent it is”, and an extract of a letter from Thomas Edison's associate and his co-workers supported a steady-state theory of the Universe, as calculated by Fred Hoyle at that time, predicted a background X-ray flux 100 times higher than Friedman's data allowed. This calculation was presumably forgotten about in 1962 when Hoyle published a paper claiming that the X-ray background just discovered by Giacconi and his co-workers supported a steady-state cosmology.

Perhaps the most currently relevant aspect of these tales is the trial-and-error and try-and-try-again nature of the research. Generally it was successful and when it wasn't, it was usually the fault of the apparatus or the rocket. Serendipity played, and still plays, an enormous role. Most cosmic phenomena have been found by chance. Observations lead the subject and theory provides an essential later synthesis. How this era of discovery would have proceeded if it had all gone through the present peer-review system, which is usually overloaded by more than three good proposals to each one accepted, does not bear thinking about. Discoveries often come from an advanced form of playing and should not be made accountable. What does bear thinking about is how our present systems stifle enthusiastic trial-and-error research.

I found the tales of the early rocket days made interesting reading and wished that...