

## Review

### *Genes, Memes and Human History*

by Stephen Shennan

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#### 1. The Problem

Perhaps the most critical problem facing the human sciences is that of integrating two conceptions of human activity. On the one hand, humans are evolved primates. And humans are encultured beings: intentional agents who are profoundly influenced by the particular cultures in which they are immersed. It has proved remarkably difficult to develop an account of human nature and of the broad patterns of human history that does justice to both these facts. The lingering nature/nurture problem is just one expression of this unresolved tension.

Archaeology, of all the human sciences, can dodge this problem the least, and the great virtue of Shennan's *Genes, Memes and Human History* is that he confronts it directly. For though humans are now both cultural and ecological beings, it was not always so. Once our hominid ancestors had a social organisation and a material culture roughly equivalent to that of today's chimpanzees. Chimps are not encultured in the sense that we are encultured: their social life and their ecology does not depend on the accurate and extensive transmission of information from parents to offspring. It falls to archaeology to document and explain the transition from merely social hominids to encultured hominids. Archaeologists cannot escape our dual nature, for they must explain its coming into being. Thus for an evolutionary archaeologist like Shennan, the evolutionary facet of human nature and human history must be geneologically primary. For our enculturation is the product of a continuing evolutionary process grafted onto the top of a pre-existing set of ecological and social relations.

Accordingly, Shennan begins by describing the toolkit that has been developed by those in the business of giving evolutionary explanations of human behaviour, identifying three basic approaches to human behavioural evolution. He briefly mentions evolutionary psychology: the project of developing evolutionary explanations of human cognition while also using evolutionary theory as a tool for generating hypotheses about our cognitive organisation. But his real interest is

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in two other approaches: human behavioural ecology and dual inheritance theory. Behavioural ecologists model species-typical choice situations—foraging, mate choice, parental investment, social interactions and the like. Human behavioural ecology is just the application to humans of the tools behavioural ecologists have developed, and hence tends to emphasise the continuities between humans and other animals. In contrast, dual inheritance theory is focused on a distinctive and historically deep feature of human life (distinctive but not unique: see Avital and Jablonka, 2000). Children resemble their parents because of the flow of genes from parents to children. But children also resemble their parents because there is an extensive and accurate flow of information from parent to child.

It is not much of an exaggeration to say that the whole of *Genes, Memes and Human History* revolves around the interplay of behavioural ecology and dual inheritance, and on the transformation of behavioural ecological models by dual inheritance theory. Evolutionary psychology plays virtually no role in Shennan's evolutionary archaeology. Evolutionary psychology has been shockingly oversold by its supporters. Even so, though many of the specific claims of evolutionary psychology are not well-supported, for reasons I shall explain in the next section, Shennan cannot afford to leave this leg off his tripod. An account of the transition from social to cultural life needs a systematic consideration of hominid cognitive capacities. The project of evolutionary psychology, if not its current practice, is an essential element of evolutionary archaeology.

## 2. The Cognitive Load of Behavioural Models

In the first seven chapters of *Genes, Memes and Human History*, Shennan demonstrates the value of fitness-maximising models ('optimality models') in probing the archaeological record. He begins chapter two with an elegant model of starling behaviour (pp. 24–28), and then shows how such cost/benefit analyses throw light on crucial features of human activity: the getting of food, children and sex. He discusses models both of the extraction of resources from a local area and of when to leave it. He discusses mate choice, birth spacing, fertility and differential investment in children. Shennan is right to think these models are insightful. Nonetheless, he understates the problems of applying them to the human archaeological record. Let me begin with the starlings.

When starlings feed their young, the default expectation is that they maximise their own fitness by maximising the rate of food delivery to the chicks. For better fed chicks fledge earlier and heavier, with better survival prospects. Since the journey from foraging site to nest takes time and energy, the optimum strategy would seem to be that of bringing back the heaviest load possible, thus cutting down on transit costs and increasing search and capture time. But starlings must keep what they have caught in their beak and so their efficiency declines as their captures increase. They must trade declining efficiency against transit costs. This problem succumbs to economic analysis, and there are plausible models specifying the optimum behaviour for starlings; an analysis which depends both on the

distance between nest and foraging site, and the rate at which their efficiency declines. When travel costs are low because the bird is foraging close to its nest, a lighter load is more efficient. When travel costs are high, it is worth paying the cost of foraging less efficiently and carrying more per load. Actual starling behaviour seems to fit these models quite well (Kacelnik, 1984).

This example is typical of the approach. These models share a set of fundamental elements. They specify the set of strategies available to an agent in a given situation; they include a measure of the costs and benefits that attach to each strategy; and they specify the features of the environment that determine those costs and benefits. The optimal strategy best trades benefit against cost in the agent's circumstances, and the general expectation is that in species-typical choice situations, agents will find that optimum strategy. When they do not, it will be the fault of the model-maker not the creature-maker. We will have somehow mischaracterised the strategy set, or the costs and benefits of different options. Moreover, behavioural ecologists aim to predict patterns of behaviour. Their models are not hypotheses about the proximate psychological causes of behaviour. There is no suggestion that starlings mentally represent the graphs of the optimisation problem, and find the line that represents their best trade-off between the costs of carrying and decreasing search efficacy. The assumption is that starlings have some fairly simple heuristics that lead to near-optimal behaviour.

In the case of fairly simple, relatively invariant, and species-typical decision problems, this assumption seems reasonable. The starling decision model has a *low cognitive load*: the information required for correct strategy choice is minimal and it is potentially available to starlings as a byproduct of their routine activity.<sup>1</sup> These starlings gathered only one kind of food; they were sensitive only to one cost (time); and their fitness was maximised by maximising a fairly simple and immediate physical quantity: the rate of food delivery. If all this is right, there are unlikely to be serious cognitive constraints on starling strategy choice. Imagine a more complex starling world, in which starlings had to take into account both risks to themselves in foraging and in transit, and risks to the nest occasioned by their visits. Suppose too that starling chicks needed a variety of foods that varied by age and sex. The rate of food delivery would not then covary so simply with parental fitness, and the cognitive load on optimisation might be very considerable indeed. A model of starling foraging could not then tacitly assume that they could make optimal choices relying on cognitive capacities that were the common inheritance of their clade. We would either have to explicitly build cognitive constraints into the starling model, or (and/or) show how starling evolution had built cognitive capacities to support this cognitive load.

This point leads to our first human/starling contrast. The problems facing humans are far more complex even than those of our imagined starlings. As

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<sup>1</sup> For example, one thing they need to know is the distance from foraging site to nest, and that information is available as an ordinary byproduct of feeding.

Shennan's own discussion shows, parents in provisioning their children need to be sensitive to the sex of their child—for in some circumstances they should invest more heavily in one or the other sex. The application of foraging models suggests that foragers are not just maximising the rate of calorie delivery. There are tradeoffs between meat and plant foods (pp. 146–147). Likewise, there are trade-offs between maximising the expected rate of food delivery and risk reduction; perhaps one reason why women hunt less is that it is for them too risky (p. 147). The social context of foraging is relevant, for agents cannot always assume that they and theirs can eat all and only what they catch. So too are negotiations between the parents. If parents do not have all their children in common, they may well not have identical views on how much to invest in a given child. In short, the optimal decision on what to feed your children will be sensitive to the specific environment in which agents find themselves. Moreover, even if we fix the environment, there is no species-typical optimal choice. For the right choice depends not just on the environment but on the individual. How much to invest in a given child; what risks to accept; whether to invest more heavily in a male or female child all depend on the age and location in social space of the agent (pp. 188–197).

Thus agents' choices are constrained by the informational demands on action. The costs and benefits of information, and limits on the capacity to use it, cannot be ignored in explaining patterns in human behaviour. That is especially true once we recall that much human action is *epistemic*: its point is to change the informational character of the environment. Trail and boundary marking are simple examples of such actions, but there are many more (see eg. Kirsh, 1995; 1996). These issues also arise in connection with cultural inheritance, as, in their different ways, Sperber and Tomasello show. Sperber has analysed the way cognitive capacities affect the *kind* of information that is transmitted, and Tomasello the *fidelity* with which information is transmitted.<sup>2</sup> Shennan does not completely neglect these issues. Dual inheritance theory, with its cumulative solutions to informational problems, in itself partially addresses the problem of cognitive load. Moreover, he discusses models of social behaviour which do not presuppose that agents are hyper-rational (pp. 217–218), and he points out that technological innovations are not automatically generated by a need for them. But there is no systematic discussion of cognitive constraints on action or on cultural transmission.

It follows that if agents do routinely make optimal decisions of these kinds, this is a fact that in itself would require explanation. But do they in fact do so? This brings me to the second contrast between the behavioural ecology of hominids and that of starlings. Powerful empirical tests of optimality models depend on independent measures of their ecological assumptions. Such measures are available for starlings, and this makes it possible to make a quantitative prediction about their optimal load per distance. Without these independent measures of the model's parameters, the best we can expect is qualitative consistency between the predictions of the

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<sup>2</sup> See (Sperber, 1996; Tomasello, 1999); for a response see (Henrich and Boyd, 2002)

model and patterns in human behaviour as shown in the historical record. We do seem to find this qualitative consistency. As Shennan points out, there is good evidence that as foragers deplete high value resources, their diet breadth increases, and resources previously ignored come to be exploited (pp. 149–153). This is certainly an adaptive response to resource depletion. But we have no idea whether in general such responses have been optimal: the data Shennan discusses is not nearly fine-grained enough to tell us whether (say) New Zealand Maori hung onto their moa-hunting ways long after it ceased to represent the optimal return for effort or whether they panicked early.

These limitations on the tests of optimality models deprive us of the most important benefit such models can provide. For a great strength of these models is that *predictive failure* is very informative, revealing unexpected constraints on the strategy set, unnoticed costs, or unappreciated features of the environment. An optimality model is not a bet that nature is perfect. It is a device for finding the limitations on adaptive design. But we need strong, quantitative tests to use optimality models to find such unexpected limitations. If the archaeological record is too poor to support strong tests, that would seriously limit the value of behavioural ecology for archaeology.

### 3. Dual Inheritance

Optimality modelling has a significant role to play in the analysis and explanation of the human archaeological record. But the tools of behavioural ecology need to be used with an awareness of history, of cultural inheritance. At this point, I must note a complication. There are two ways of conceptualising dual inheritance theory. Here is the first. Information flows through psycho-cultural channels (language, imitation, teaching) from parents to offspring, and results in parent/offspring phenotypic similarity. To the extent that these phenotypic features enhance fitness, and to the extent that the psycho-cultural channels are reliable and of high fidelity, these information packages and the traits associated with them will increase in frequency as generations pass (Mameli, 2004). On this view, information is replicated, and the vehicles are biological individuals (organisms or perhaps groups). On an alternative conception, the vehicles are not biological agents but *artefacts*. Successful memes ('coding' for artefacts) replicate, as artefacts are made or used more frequently. Shennan seems to endorse both pictures, but I am decidedly underwhelmed by the second, for it seems to have no explanatory power. For example, Shennan uses the second conception to describe the replacement of snowshoes by snowmobiles. But the evolutionary analysis tells us nothing that we do not learn by a standard rational choice explanation: namely, that the Cree maximised expected utility by shifting from snowshoes to snowmobiles (pp. 56–58). Cultural inheritance is important, but only if we think of it as an information flow between biological individuals. So understood, cultural inheritance has three key effects on human evolution.

1. The most pervasive influence of dual inheritance is through its impact on niche construction. Many organisms partially construct their own habitat. They do so by physical engineering; through their social organisation (eg. for collective defence); and by epistemic action that changes the information demands on behaviour. Some niche-constructing organisms reconstruct the habitat of their offspring, not just their own. Humans are such downstream niche constructors. Moreover, our niche construction is cumulative. The high fidelity transmission of information across the generations enables humans to *successively* improve their technology and their knowledge basis.<sup>3</sup> The use of fire, for example, is not a one-step invention. Almost certainly, naturally occurring fires were domesticated before ignition technology was invented and improved, and all this preceded the invention of hearths and other fire-amplification techniques (Ofek, 2001). As a result of cultural transmission, human environments gradually came to include fire as a resource and a tool. Cultural transmission intersects with behavioural ecology by transforming the environment and hence the costs and benefits of particular strategies. Shennan, for example, points out that bow and arrow technology massively reduced the capture times of small game, and thus made hunting them worthwhile (p. 113). Cultural transmission transforms the environment in which agents operate.
2. Cultural transmission makes group selection important to human evolution. A tool of behavioural ecology that is of minor importance in explaining animal behaviour becomes central to explaining human behaviour. For cultural transmission magnifies between-group differences and reduces differences with groups. Once agents have ‘common knowledge’ of what other agents are likely to do—once certain practices begin to be established—it will rarely pay individuals to act against these social defaults. For their existence allows both social groups and the individuals that compose them to reap the benefits of co-ordination. But since the particular conventions that are established in a culture depend on historical accident, the processes which tend to make cultures internally uniform, also tend to make them vary one from another (pp. 218–22). Sanctions and/or conformist traditions accentuate this tendency. If there is variation across groups and little variation within them, group selection is likely to be powerful: see (Boyd and Richerson, 1996; Sober and Wilson, 1998; Richerson and Boyd, 1999; 2001)). One obvious signature of group competition (and hence group selection) is raiding and other forms of intercommunal violence,

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<sup>3</sup> On niche construction in general, see Odling-Smee, J., K. Laland, *et al.* 2003: *Niche Construction: The Neglected Process in Evolution*. Princeton, Princeton University Press. On its crucial role in human evolution, see Sterelny, K. (2003). *Thought in a Hostile World*. New York, Blackwell., and on the importance of high fidelity transmission, see Tomasello, M. 1999: *The Cultural Origins of Human Cognition*. Cambridge, Harvard University Press.

and there is ample evidence of such activity in both the ethnographic and the archaeological record (pp. 244–252).

3. Suboptimal action can be a consequence of cultural transmission. For the flow of information is sometimes oblique; from unrelated individuals of the parental generation. And there are horizontal flows, from other children and from adolescents (pp. 49–51). Once the flow of culturally transmitted information is decoupled from gene flow, there is no theoretical reason to assume that the behaviour supported by culturally transmitted information will maximise biological fitness, for its continued transmission does not depend on its doing so. Moreover (to put flesh on this abstract possibility), it is not hard to envisage how there might be selection for social learning rules that depend on oblique transmission. For example, in many circumstances it would make sense to copy the most successful member of the parental generation (or of one's own generation). For their success often depends on their special skills (Boyd and Richerson, 2001; Laland, 2001). Yet this rule could produce profoundly maladaptive behaviour. For some policies which are good for the successful are not good for everyone. The preference for male children found in many cultures may be a maladaptive consequence of such a 'copy the best' learning rule. A preference for sons is adaptive for the successful. Male reproductive variance is greater than that of females, and the successful have the resources to invest heavily in sons. So their sons are likely to be amongst the successes of their generation. But for just this reason the less successful should, if anything, prefer daughters.

So the existence and importance of cultural flows that do not go via parents to their children can indeed explain departures from optimum strategy choice. That said, identifying maladaptive behaviour is less straightforward than Shennan sometimes suggests. For example, he takes an African study to show that individuals fail to choose the optimal action because they have learnt the wrong ones from their parents. Thus he says 'it was the learning history of cultural inheritance from parents and other members of the older generation that largely determined what people did in many spheres of life, not what would be ideal from the adaptation point of view in present circumstances' (p. 44). The study showed no such thing (Guglielmino, Viganotti *et al.*, 1995). This study took account of the ecological settings of the different cultures, and local ecology did not predict cultural characteristics. But for all this study showed, *individual behaviours* were locally optimal. For other agents and their expectations and reactions are part of the local environment—and a very important part at that. Individual defection from local expectations about kinship; housing; and economic activity may well have had very serious consequences. Guglielmino's study shows the importance of niche construction, not the role of culture in explaining failure to select the optimal strategy.

Thus cultural inheritance plays a central role in niche construction, empowering group selection, and in explaining variations from optimal action. This may not

exhaust its importance for our evolutionary history. Let me return to the issue of cognitive load. I argued in section 2 that finding the optimal strategy is often so informationally demanding that we would need to explain how those cognitive demands were met. For example, in foraging, to make the optimal decision about what to chase and what to leave, and when to continue searching through a patch, and when to go elsewhere, an agent needs rich, detailed natural history information. They will need information about encounter rates of particular prey, about capture probabilities, about risk, and about the comparative profile of different patches. Intelligent, sophisticated, informed response to difficult decision problems are the products of cultural adaptation—cumulative niche construction—not just genetic endowment (or so I have argued: (Sterelny, 2003)). Technology is often built cumulatively. But so is information and the capacity to use it. The rich natural history data bases that were necessary for optimal foraging choices by creatures like us are the work of generations. History does not just explain suboptimal choice; it also explains optimal choice.

#### 4. Final Word

*Genes, Memes and Human History* is not a narrative. It is not an evolutionary archaeologist's version of Tattersall's *Becoming Human*, or a picture of the more recent transition from prehistory to history. Instead it develops and demonstrates a set of tools for the evolutionary analysis of such transitions. I have argued that the toolkit is not complete. To understand the major patterns of our past, we need to identify the informational capacities and limitations of our ancestors. Be that as it may, this is a very fine book. It is full of both ideas and examples; ideas and examples which jointly vindicate Shennan's belief that nothing in archaeology makes sense, except in the light of evolution.

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