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Alex Walter

Social Science Information 2007; 46; 691

DOI: 10.1177/0539018407082597

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Alex Walter

The trouble with memes: deconstructing Dawkins's monster

An Essay Review of *The Selfish Meme: A Critical Reappraisal* by Kate Distin and *Not by Genes Alone: How Culture Transformed Human Evolution* by Peter J. Richerson and Robert Boyd.

Abstract. The essay explores the adequacy of the 'meme' concept to explain the basic mechanism of cultural evolution. Distin's defense of 'memetics' is deficient because it is based on a flawed analogy with genetics. Although Richerson and Boyd's 'cultural variant' is not based on a flawed analogy with genetics, their alternative appears primarily to be operant conditioning in disguise and presents no novel innovations in learning theory. Both models of gene-culture co-evolution are examples of one-sided cultural determinism with no genuine biological component. Analysis of the information metaphysics of memes and cultural variants reveals that both concepts are also committed to mind-body dualism.

Key words. Co-evolution – Cultural variants – Culturgens – Dual inheritance – Evolutionary psychology – Gene-culture – Memes – Memetics – Sociobiology

Résumé. Cet article explore dans quelle mesure le concept de 'mème' permet d'expliquer les mécanismes à la base de l'évolution culturelle. La prise de position de Distin en faveur de la 'mémétique' n'est pas convaincante dans la mesure où elle est basée sur une

analogie imparfaite avec la génétique. Bien que la 'variante culturelle' de Richerson et Boyd ne soit pas basée sur une analogie imparfaite avec la génétique, l'alternative qu'ils proposent semble être avant tout un conditionnement déguisé et n'est en rien une innovation pour la théorie de l'apprentissage. Ces modèles de co-évolution gène-culture sont tous les deux des exemples d'un déterminisme culturel univoque sans véritable composante biologique. L'analyse de la métaphysique des 'mèmes' ainsi que celle des variantes culturelles révèle que les deux concepts sont l'un et l'autre étroitement liés au dualisme corps-esprit.

Mots-clés. Co-évolution gène-culture – Culturgènes – Double héritage – 'Mèmes' – 'Mémétique' – Psychologie de l'évolution – Sociobiologie – Variantes culturelles

The question of human culture has been a sticky problem for sociobiologists from the very beginning of the enterprise. Sociobiology's critics asserted that bare-knuckled natural and sexual selection are ill equipped to explain the complexities introduced into human behavior by the phenomenon of culture.¹ Several important founding fathers of the discipline agreed. Dawkins (1976) attempted a pre-emptive counter-strike in the last chapter of *The Selfish Gene* by inventing the *meme*, a cultural counterpart to the gene, while E.O. Wilson solicited the aid of Charles Lumsden after the attacks began by devising a similar construct, the *culturgen* (Lumsden and Wilson, 1981). While Dawkins (2003) regards the two terms as synonymous, Lumsden and Wilson regarded them from the beginning as distinguishable concepts, although they place both in the same polythetic cluster (Lumsden and Wilson, 1981: 27).² For whatever reasons, meme has become by far the most widespread 'allele' of the two (Dawkins, 2003: 120, 125).

The question is this: Is the concept of the meme useful? Does it adequately account for culture and does it forestall claims that sociobiology is inherently guilty of 'genetic determinism, facile reductionisms, and faulty model-building' (Kitcher, 1989: 186)? Cultural anthropologists such as Maurice Bloch (2000) have argued that the gene-meme analogy is fundamentally flawed because 'culture' – whatever it is – cannot be conceptualized as discrete bits of information in the same way that genes can and because the transmission processes are sufficiently different to undermine the model. Philosophers of biology such as Sterelny and Griffiths (1999: 332–4) reach a similar conclusion. Many Darwinians also agree, including the school of dual-inheritance theorists of gene-culture co-evolution led by Peter Richerson and Robert Boyd.

Richerson and Boyd began their long collaboration on gene–culture co-evolution with an article published in 1976, and now they have produced another exceptional and thoughtful book on the topic that summarizes their thinking over the past three decades in especially reader-friendly form.

Kate Distin, on the other hand, has produced a spirited defense of *memetics*. Its chief virtue, however, may prove to lie in the precision with which she defines the concept and the philosophical commitments that underlie it. If we choose to reject the meme concept, it may be for philosophical reasons as much as for anything else. But then the question becomes: Does Richerson and Boyd's formulation of cultural evolution fare any better? Is their concept of *cultural variants* and the processes by which they are transmitted any different or any better than that of memes and culturgens? Moreover, if these concepts are inadequate or unacceptable, how then do we conceptualize 'culture'? Let us begin by attempting to deconstruct the meme.

The ghost in the meme

Distin defines the meme, citing Dawkins (1982), as a 'unit of information residing in a brain' (*The Selfish Meme*, hereafter SM, p. 11). Memes include ideas, concepts, skills and procedures such as farming methods that are replicated from one mind to another. The external consequences of that piece of information are its phenotypic effects. Like genes, memes are replicators. Following Dawkins, Distin states that genes and memes are decoupled. By 'decoupled', she means that memetic success is not intrinsically linked to genetic success.³ On this view, both are instances of a larger more general selection process, and both as such are Darwinian (Lumsden and Wilson, 1981: 16). This view has come to be known as 'universal Darwinism'. If memes are not dependent on genes, then what are they dependent on? According to Distin, memes are dependent on representational content.

Representational content refers to *ideas*. Some ideas are simple, others are large and amorphous. All ideas of a potentially transmissible nature qualify as memes. The ability of an idea to obtain and maintain attention provides the basis for meme 'replication' (SM, p. 66). Ideas that are useful will be active in the minds of their bearers and will be transmitted to other minds. If an idea is possible but unused, Distin refers to it as a recessive meme. Here are a few examples that Distin uses:

- fashion meme for miniskirts in Siberia (recessive)
- fashion meme for Aran sweaters in Florida (recessive)
- riding a bicycle to work (success depends on where)
- rejecting contraception (success depends on activation of other memes such as the Pope meme).

One suspects that any possible ‘idea’ might qualify as a possible meme. What then doesn’t count as a meme? Emotions are not memes because they do not possess representational content. Distin suggests instead that emotions are reactions to memes (SM, p. 63).

The mental representations of other species, and possibly earlier hominid species, also do not qualify as memes. For example, frog representations of flies do not count as memes. In order to qualify, representations have to be sufficiently abstract.

Memes emerge at the level of meta-representations (i.e. representations about representations). Distin thinks only anatomically modern humans are capable of this kind of representation. An ape might pull the leaves off a twig and use it to fish for termites, but the ape falls short of seeing it as a tool for that purpose. Distin says the former learned behavior is just instrumental behavior, but the latter involves the forming of an abstract *concept* – in this case the concept would be ‘ant dipper’ (SM, p. 129). The concept is the functional type under which various instruments might be grouped as examples. For another example, the cantilever principle covers both brassieres and shelves – since they perform the function of supporting objects (SM, p. 133). The crucial criterion is that the concept be detached from its immediate stimulus conditions so that it can be placed as an abstract object in relation to other similarly detached context-independent concepts. Thus memes are replicating meta-representations (SM, p. 135). It need not be that culture consists *only* of such high-level representations, but culture does depend to an important extent on the use of such representations (SM, p. 139).

Let us grant that learning comes with various degrees of abstractness. Even though complex learning can occur through mindlessly copying details of another’s behavior, Distin argues that memes require the use of rules. Hence social learning is the key to memetic transmission. Only meta-representations that lend themselves to social learning count as memes. How do memes replicate? According to Susan Blackmore (1999), the only means of cultural transmission that can be called memetic is *imitation*. Although Distin includes imitation, she also includes other forms of social learning, which range from gossip to norm-following (SM, p. 46). She identifies three kinds of imitation (SM, pp. 136–8):

1. imitation of the surface form of the behavior;
2. imitation of the complex structure of behavior (this involves discriminating between different parts of the total pattern of behavior and finding those that are causally relevant);
3. imitation formulated in terms of rules or general principles (e.g. recipes).

Because the last form of imitation involves rules, it qualifies as the most important venue for meme replication. Norms are critical because following a norm means following a rule, and rules can be stated in terms of *ideas*.

Another important requirement is that memes have to be like genes in that they remain both *particulate* and *integrative*. No blending allowed. Towards this end, Distin employs Herbert Simon's idea of subassemblies (Simon, 1962). A meme seems to act like a part that can participate in different wholes (i.e. conceptual systems). Lately Dawkins has arrived at a similar solution. He adds that combination also solves the scale problem. Small memes combine to form larger complex memes (Dawkins, 2003: 126).

If anything, Distin's rigorous formulation of the meme reiterates fundamental issues that critics have raised against memetics regarding the problem of essentialism, the nature of learning processes, and the relevance of genetic analogies. For example, Dan Sperber (1996) raised a fundamental objection to the replication thesis by questioning the essentialism of the meme concept. He argued that the 'information' in my representation and that in yours are not identical. Only if they are identical could memes be replicated similarly to the way identical genes are replicated from one bearer to another. Sperber says, for replication to apply three conditions must be met: (1) A must cause B; (2) A must be identical to B; and (3) B must be inherited from A. If B tries to copy A's mental content, it is certain that that content will not be replicated in its original form because it will be transformed by the background context of B's web of beliefs, which is different from A's.

Sperber has correctly identified the central problem for the meme concept. Distin makes it clear in her discussion of the *type-token* distinction that the meme is an example of a Platonic essence. Whatever private events are occurring in your brain or the brain of someone else, while they might be described as being similar, as in Lumsden and Wilson's (1981) polythetic conception, in order for them to be construed as identical instantiations of the same meme, they would have to be conceptualized as Platonic essences. In the case of the meme, the 'information' that is contained in the meme is the class. The particular instantiations that each of us carries in our individual brains are tokens of the 'information' they

represent. This kind of essentialism persists in the philosophy of mind as what W.V.O. Quine refers to as the 'idea' idea (Quine, 1987: 87–9).

Distin attempts to save the day for memetics by arguing that replication of ideas is possible based on evolved psychological mechanisms that force content into specific forms. Thus information could be inherited in its original form from its source because information exists in the form of similarity of brain events (SM, pp. 102–8). The philosophical issue of essentialism aside, even if we admit that there is no heritability in brain mechanisms, and we concede that these are uniform for all members of our species, it does not follow that representations produced by these mechanisms will also be *the same* across individuals. This is due to the empirical and varied nature of different humans' experiences. For example, if identical twins have different experiences, this will necessarily result in differences in the details of their representations (cf. Quine, 1985). Even on this looser conception of 'ideas', memes are still false isolates. Identity of stimulations does not guarantee identity of content – merely similarities in brain events, which can be grouped statistically. Distin's commitment to Platonic essentialism is especially poignant with respect to the status of statistical variation, a principal component of Darwinian selection. Distin regards statistical variation as noise in the memetic system. She states that you just have to get rid of the distortions and omissions, etc., to get at the true essential core of a particular meme (SM, p. 158). The elimination of 'noise' in the meme is crucial to Distin's project because Platonic essentialism can only be preserved by suppressing any messy empirical variation that exists in 'ideas'. Hence, the recognition of variation in 'representations' undermines the claim that ideas can be treated as essences. It is undeniable that variation is ubiquitous in biological systems. Dennett (1995) pointed out that Darwin's discovery of the messy empirical variation exhibited by members of a species constituted 'Darwin's dangerous idea' and that it dealt the death blow to essentialism in botany and zoology (1995: 36–9).

Is the 'essential' plight of memes different from that in the case of genes? Susan Oyama (1985) critiqued the notion that there was 'information' in DNA and that it was this 'information' that was passed on from bearer to recipient. Whatever it is that genes do in the process of synthesizing proteins, etc., they do not pass on pre-existing packets of 'information'. Oyama refers to the belief that 'information' exists in genes as the 'ghost in the gene'. For Distin, 'information' plays a similar role: it is the 'ghost in the meme'. Certainly she buys into the information-metaphysics version of the gene. She asserts that DNA is *in* the

genes. (This kind of abstraction is similar to what Paul Churchland has referred to as the Betty Crocker kinetic theory, i.e. the theory that 'heat' is caused by the interaction of kinetic molecules rather than being synonymous with those interactions [Churchland and Churchland, 1998: 113–22]). Similarly, Distin claims that genes are representations, in the medium provided by DNA, of the phenotypic features they control, and she compares them directly to memes, which she claims are representations of the cultural traits they control (SM, p. 142).

Distin frequently acknowledges that genes and memes have significant differences, but, because the processes are admittedly decoupled, this does not necessarily present an obstacle. However, there is one difference that I think is crucial because it bears on the key issue of information-transmission metaphysics that we have been exploring. Genes always contain or specify a mechanism to copy themselves, and memes do not (SM, pp. 64–5). The key element here is the physical nature of the mechanism by which copies of genes get from source to recipient. Genes really are passed on from source to recipient, albeit sometimes with mutations. Nothing remotely similar happens with respect to behavior. The behavior of my neurons is generated in my brain, and the behavior of your neurons is generated in your brain; there is no transfer or sharing of physical activity between two separate brains. Philosophical materialism is committed to the view that the 'mind' is merely the behavior of the brain and not a separate non-physical entity that is related to the physical brain by one bridge or another. Memes are one such imaginary bridge.

What then does learning consist of if nothing is physically transmitted from one brain to another? Could memetics survive if it committed to a form of learning theory that eschews essentialism and metaphysical information transmission? Unfortunately, if its proponents could eschew these commitments, they wouldn't be able to call their discipline memetics. The meme is irredeemably tied to the 'idea' idea. Sterelny and Griffiths (1999: 333) reach the same conclusion.

The Richerson and Boyd variations

Do Richerson and Boyd offer a satisfactory alternative to memetics? *Prima facie*, Richerson and Boyd's definition of culture looks remarkably similar to that proposed by the memeticists: 'Culture is information capable of affecting individuals' behavior that they acquire from other members of their species through teaching, imitation, and other forms of

social transmission' (*Not by Genes Alone*, hereafter NBGA, p. 5). By 'information' they mean any mental state that is acquired by learning and which affects behavior. These include ideas, beliefs, values, skills, attitudes, etc. (p. 5).

Genetic selection involves the transmission of genetic information: '[s]pecies are populations of individuals that carry a pool of genetically acquired information through time'; the difference for culture-bearing humans is also a matter of information transmission: '[h]uman populations carry a pool of culturally acquired information... we need to keep track of the processes that cause some cultural variants to spread and persist while others disappear' (NBGA, p. 59). As stated in their definition of culture, the transmission mechanism resides in learning processes that appear to be fairly straightforward. Richerson and Boyd state that '[c]ulture is taught by motivated human teachers, acquired by motivated learners, and stored and manipulated in human brains' (NBGA, p. 7). So far the picture of culture evolution they draw is difficult to distinguish from the memetic alternative.

Richerson and Boyd certainly do not differ from the memeticists in promoting a selection-based conception of culture. They emphasize the role that selection by learning plays in the Darwinian model. Where they do differ is in the dimensions ascribed to the unit of cultural selection.

Adopting a Darwinian approach to culture does not mean that you have to also believe that culture is made up of miniscule [sic] genelike particles that are faithfully replicated during cultural transmission. The evidence suggests that sometimes cultural variants *are* somewhat genelike, while at other times they are decidedly not. But – and this is a big but – in either case, the Darwinian approach remains useful. (NBGA, p. 80)

The term they invent for a unit of culture is *cultural variant*. One important difference between cultural variants and memes is that cultural variants are not replicators. This is a response to critiques offered by Hallpike (1986) and Sperber (1996). To block Hallpike they have to deny the minuscule, particulate claim, so they admit that some cultural variants can be small, particulate and faithful copies (i.e. genelike), while others can be large and systematically interconnected wholes. We have seen, however, that both Dawkins (2003) and Distin (2005) also enable the linkage of small discrete memes into larger interconnected assemblies, or memeplexes. Yet even if we were to permit the combination of memes into subassemblies and memeplexes in order to form complex wholes, this does not solve the problem of information-transmission metaphysics.

Nevertheless, Richerson and Boyd appear to recognize and accept the objection that Sperber raised against the meme concept: 'ideas are not

transmitted intact from one brain to another. Instead, the cultural variant in one brain generates some behavior, somebody else observes this behavior, and then (somehow) creates a cultural variant that generates more or less similar behavior' (NBGA, p. 82). If there is similarity in the private events between individual brains, this is due to similarity of stimulations and the way our genes construct the nervous systems of the members of our species. Thus Richerson and Boyd construe *cultural variants* in terms of a population of similar but not identical learned representations. This conception thus appears to be similar to the polythetic *culturgen* concept devised by Lumsden and Wilson (1981). However, despite this apparent correspondence, it remains unclear whether cultural variants are physical events in brains or non-physical mental events (i.e. information complexes) that are embodied in physical brain events. Given the information-based definition of cultural variant provided on page 5 of NBGA, I strongly suspect that it is an instance of the 'idea' idea. If that is the case, then each individual brain houses a set of ghostly mental representations that are distinct from those in other individuals, but the total n sets that comprise a culture can still be grouped together in a polythetic cluster.

Cultural selection and social learning

Richerson and Boyd argue that cultural variants are transmitted by means of various selection processes. They provide a handy chart (NBGA, p. 69) that inventories the type of selection processes available, and they go on to explore these processes in some detail throughout the remainder of the book. In every instance the selection process turns out to be some type of social learning. In chapter 4, Richerson and Boyd discuss the different types of learning mechanisms and how they differ between humans and other species. They show that social learning is not highly developed in species other than humans. As with the memeticist rival Blackmore, *imitation* seems to be the most critical factor in human learning compared to other species. Chimps, for example, learn by imitation but they do not pay attention to the precise movements so they are comparatively ineffective in comparison to human children (NBGA, pp. 110–11). The authors draw a crucial distinction between imitation and learning. By 'learning' the authors mean figuring out on your own which behavior is best. Imitation is thus a substitute for learning. They hypothesize that when learning is 'cheap' it will be preferred but when learning is 'costly' imitation will be preferred (NBGA, p. 113).

It is odd that the principle ‘natural selection’ itself appears on their chart of modes of cultural learning and is defined as: ‘[c]hanges in the cultural composition of a population caused by the effects of holding one cultural variant rather than others. The natural selection of cultural variants can occur at individual or group levels’ (NBGA, Table 3.1, p. 69; cf. p. 117).

Presumably the analogy with Darwin’s concept is close enough in their view to justify hijacking the concept for cultural ‘evolution’. The authors argue that ‘guided variation’ (i.e. innovations during learning) is not like natural selection but that ‘biased transmission’ is. At the top of page 116, guided variation is distinguished from Darwinian selection because the mutations are not random but instead are themselves fitness enhancing. Biased transmission consists of cultural variants that are selectively imitated and hence passed on differentially. The transmission of cultural variants through differential imitation may seem like selection of alleles but it is clearly just an analogy with genetic selection and nothing more.

The common element that underlies the various modes of cultural learning is its *cumulative* nature. ‘Culture’ is largely the accumulated learning of others. We are the only species that passes innovations on for multiple generations, and our imitative ability appears to be a crucial adaptation. We can learn from others’ mistakes and successes without having to do the basic trial-and-error research ourselves. Although Richerson and Boyd argue that imitation itself sometimes allows cumulative improvement (i.e. imitation means you don’t have to reinvent the wheel each generation), they conclude that cumulative improvements usually depend on a combination of imitation and learning (NBGA, p. 114).

At this point one might wonder what distinguishes Richerson and Boyd’s gene–culture co-evolution model from other evolutionary rivals such as evolutionary psychology and behavioral ecology. The authors themselves make a point of establishing the differences and lobby for the superiority of their view in explaining culture. They hypothesize that the evolution of the hominid brain was produced by the cultural benefits derived from imitation (NBGA, p. 115). Learning may occur in environments that are so variable in the short term that none of it will be passed on. If the environment is stable for thousands of generations, then learning may be replaced by phylogenetic adaptations. Social learning, i.e. cultural adaptation, will occur when environments are variable but change slowly (NBGA, p. 131). Richerson and Boyd hypothesize that the Pleistocene presented the type of environment that facilitated the evolution of social-learning mechanisms. The culprit seems to be rapid climate

change, which was chaotic across periods of dozens of years during the late Pleistocene. Richerson and Boyd point out that 'evoked' culture is the sort preferred by Cosmides and Tooby (1989) and the evolutionary psychology school. Cultural sorts are predetermined by the adapted mind, and their elements evoked under specific circumstances. According to evolutionary psychologists, we have domain-specific cognitive modules. Specific content may vary from place A to place B, but the general form of the response is generated by a species-specific module. Against the evolutionary psychologists, Richerson and Boyd propose:

1. Whatever domain-specific mechanisms we evolved didn't help us adapt to the chaos of the Pleistocene.
2. The cost of evolving domain-general intelligence for that purpose was prohibitive.
3. Social-learning heuristics were the cheapest and most effective solution.

They also oppose the Tooby–Cosmides 'big-mistake hypothesis' that the adapted mind becomes maladaptive only because modern societies are different from the Environment of Evolutionary Adaptation (EEA). Richerson and Boyd argue instead that maladaptive behavior (i.e. behavior inimical to the reproductive interests of individuals) became common due to social learning itself. Once non-parental models became available for imitation, individuals came to be susceptible to manipulation and exploitation that would not be expected when only parental models were available.

Richerson and Boyd also raise an argument against the behavioral ecologists because the latter make the mistake of thinking the organism responds only to the (non-social) environment. They claim that behavioral ecologists ignore the role of the cultural environment. The authors cite the example of how farming methods in culturally diverse communities vary even though the physical environments are virtually the same (see NBGA, pp. 20–9). Cultural tradition rather than ecological contingency explain which methods are used and learned by succeeding generations.

Operant conditioning by any other name...

The question I would like to pose is whether Richerson and Boyd's, or any other, variant of Darwinian social learning offers something distinct compared to already existing well-defined theories of learning such as Skinner's operant conditioning. Lest the Darwinists who read these words make 'a dash for the door', let me quickly draw the reader's attention to

Dawkins's (1984) evaluation of Skinner's (1966) paper 'Selection by Consequences', which was put forward in 1984 for multiple open-peer review in a special edition of *The Behavioral and Brain Sciences* (Skinner, 1984a). Dawkins was one of the few commentators who approached the evaluation of the paper with an open mind, possibly because of his immediate recognition that natural selection and operant conditioning were both examples of 'selection by consequences'. Even though it might be tempting to thus classify Skinner as a 'universal Darwinist', I think the processes described by the two forms of selection are sufficiently distinct to merit their separation, and I think important consequences follow for how we relate cultural evolution to biological evolution.

Dawkins finds a three-level system of replicator selection in Skinner's formulation. 'At Skinner level i, the ordinary Darwinian level, the replicators are genes, and the consequences by which they are selected are phenotypic effects, that is, mostly their effects on the embryonic development of the bodies in which they sit' (Dawkins, 1984: 486). Dawkins goes on to say that Skinner recognizes individuals are not replicators per se but bundles of consequences, the position that Dawkins has himself championed since *The Extended Phenotype* (1982). Consequences can be genetic or behavioral. According to Dawkins, in the case of evolved behavioral systems, the consequence of getting the slower-footed 'gene' means you are more likely to end up in the stomach of the predator than those who got the faster-footed 'gene'. Skinner thus gets credit for recognizing that there are genetically evolved dimensions that underlie behavior (cf. Skinner, 1977: 1). Dawkins continues: 'At Skinner's level ii the replicators are habits in the animal's repertoire, originally spontaneously produced (the equivalent of mutation). The consequences are reinforcement, positive or negative' (Dawkins, 1984: 486). Dawkins goes on to compare the 'habits' that are shaped by operant conditioning to the genetic replicators selected by the mechanism of natural selection.

So far, so good. The question is what to make of Skinner level iii: cultural evolution. One of Skinner's other commentators, Donahoe (1984), asked whether cultural evolution involves a different kind of selection. Skinner replied that he thought cultural selection was a different kind of selection but that it involved 'no new behavioral process' (Skinner, 1984b: 504).

There is clearly a question about what exactly is being selected and what are the selecting consequences. Within a given group, the answer seems to be practices – better ways of hunting, gathering, growing, making tools ... The practices are transmitted from generation to generation when those who acquire them under the contingencies arranged by one generation become the transmitters for the next. (p. 504)

Cultural 'evolution' thus seems a relatively straightforward example of operant conditioning. It is difficult to see any significant difference between Skinner's position and that of the gene-culture co-evolutionists – except for the application of Darwinian concepts in the place of operant concepts. Decades ago Skinner wrote: 'the culture into which an individual is born is composed of all the variables affecting him which are arranged by other people' (1953: 418). In the same work, Skinner said that 'the individual acquires from the group an extensive repertoire of *manners and customs*' (p. 415). Even then Skinner was interested in describing the elementary processes by which behavioral repertoires develop: 'Behavior comes to conform to the standards of a given community when certain responses are reinforced and others are allowed to go unreinforced or are punished' (p. 415). Responses may be shaped by the factors in the social environment but will also be shaped by factors in the physical environment. 'The contingencies to be observed in the social environment easily explain the behavior of the conforming individual' (p. 416). Skinner offers some valuable insight into the mechanism by which conformity is induced.

As each individual comes to conform to a standard pattern of conduct, he also comes to support that pattern by applying a similar classification to the behavior of others. Moreover, his own conforming behavior contributes to the standard with which the behavior of others is compared. Once a custom, manner, or style has arisen, therefore, the social system which observes it appears to be reasonably self-sustaining. (1953: 418)

I think Richerson and Boyd say something very much like this, but I do not think they say it any better. Skinner also appears to have anticipated the 'cultural variant' notion itself. He goes on to say that the variables that compose a culture are usually not unitary: 'In any large group there are no universally observed contingencies of control. Divergent customs and manners often come into conflict...' (1953: 418). Richerson and Boyd refer to these divergent customs and manners as 'cultural variants', but what we are talking about are different sets of contingencies of reinforcement that an individual is subject to. The selective mechanism by which some contingencies 'win' is a matter of operant conditioning.

The chimera of representational content

Perhaps some will object that what is different in the modern Darwinian versions of the meme or the cultural variant is the internal and interconnected nature of the mental representations. This distinction might be telling, but it might also not be a virtue. We have already voiced several

objections to the ‘idea’ idea (Quine, 1987), the proposition that mental events exist in a non-physical sense (cf. Feyerabend, 1963). Richerson and Boyd refer to cultural selection as involving a decision process where different beliefs and attitudes are subject to an evaluation of their merits (NBGA, p. 119). A decision is manifest as a rule, such as ‘imitate the most common type’ (i.e. frequency-dependent bias) or ‘imitate the most successful type’ (i.e. model-based bias) (see NBGA, Table 3.1: 69). The real issue is why we evolved dispositions towards conformity or prestige. Skinner would point to the successes in the ontogenetic frame, Darwin to the phylogenetic frame. Skinner (1977) argues that references to cognitive decision-making processes are surrogates for the effects of contingencies of reinforcement and that the former should not be construed as a causal alternative to the latter. Although it is a common belief that Skinner excludes private events from the experimental analysis of behavior, this belief is incorrect. Skinner is a materialist who believes that ‘mental’ events are the behavior of the brain. He states: ‘[n]o one doubts that behavior involves internal processes’ (1977: 9). His criticism is that ‘if cognitive processes are simply modeled upon the environmental contingencies, the fact that they are assigned to space inside the skin does not bring them any closer to a physiological account’ (p. 10).

Richerson and Boyd cite Churchland (1986) and admit the possibility that the concepts of folk psychology (i.e. beliefs, desires and other propositional attitudes) might one day be eliminated from behavioral explanations in favor of neurological predicates, and they justify using them at present primarily on the basis of convenience (NBGA, p. 63). That admission does not forecast an overwhelmingly strong prognosis for the cognitive stance in gene–culture co-evolutionary theory. We have to ask if there is a way to get around the ‘idea’ idea and eliminate information metaphysics from cultural-selection theory. We could stand with behaviorists such as Skinner and stay outside the behaving organism, concentrating on the contingencies of reinforcement since these cover much of what needs to be explained in cultural selection. On the other hand, a neurobiological account of the nature of internal processes might help lay the problem of information metaphysics to rest.

The problem posed by the metaphysics of representation is fundamental. Some cognitive neurobiologists are so suspicious of the representation ‘idea’ that they propose we do away with it. For instance Gordon Globus (1992) has proposed that we need to devise a ‘non-computational’ approach to neuroscience in order to eliminate the concept of representation altogether. Similarly, William Uttal (2004, 2005) has argued that the very idea of representation itself implies a dualism between

mental events and physical brain events. The implication of dualism is that cognitive theories stipulate that non-physical mental events exist and are represented in separate physical brain events. Certainly, in the hands of many philosophers of mind, the type-token distinction has been used to justify a distinction between 'mental' events and their 'physical' substrates (e.g. Horgan and Tye, 1986; Kim, 1986; McLaughlin, 1986). The task as the materialist sees it is how to describe brain events in such a way as not to imply a non-physical dimension in the brain's 'representational' activities. Philosophers such as Paul Feyerabend (1963), John Searle (1984) and Paul and Patricia Churchland (1998) have provided critiques that combat dualism.

However, the dualist problem itself may be a chimera since the apparent metaphysics of representation may be an artifact due to ordinary language conventions. That is, the representation concept may not entail the metaphysics that it appears to require. Quine (1985) advises us not to expect to eliminate mentalist talk even if there are no mental events in a non-physical sense. This is because the way we use psychological terms cuts across the way we use physical terms. He suggests that we adhere to Donald Davidson's (1980) doctrine of 'anomalous monism', which recognizes that only physical events exist even if we have to talk about them in apparently non-physical terms (cf. Uttal, 2004: 246). The behaviorist stance is one way of avoiding mentalist talk. Nevertheless, Quine (1990: 72) sees promise in the efforts of the Churchlands to eliminate folk-psychological entities. However, despite Quine's cautious optimism, Globus is critical of the Churchlands' attempt to conceptualize cognition in neuro-computational terms because he thinks a representational ghost is let into the neuronal machine. I would argue that, despite the Churchlands' employment of the concept of representation, they are exempt from this accusation because they are without doubt committed to a materialist account of the activities of the mind-brain (Churchland, 1986; Churchland and Churchland, 1998).

The sort of theory that more clearly falls under Globus's and Uttal's axe would include cognitive accounts as diverse and opposed as those offered by Pinker (1997) and Fodor (2000), but the axe would also fall on the implementational connectionism of someone such as Smolensky (1988), who actually does believe that 'mental' representations are implemented in a separate substrate at the physical neural level. Such a view obviously implies an acceptance of Cartesian dualism. Pessimistically, Uttal concludes that connectionism has simply become a new machine metaphor for cognitive processes (Uttal, 2005: 231). Having concluded that cognitive theory is inherently committed to dualism and that physical accounts of the

'mind' are doomed to failure because the behavior of the brain is computationally intractable, Uttal recommends that we take a behaviorist stance and abandon the objective of explaining the brain's representational activities (2005: 259–63). The implication for the study of cultural evolution is that researchers cannot hide inside the protective bubble provided by the representational fantasies of cognitive scientists. Instead we would have to build theories based on operational definitions that link behavior to its consequences. Non-cognitive learning theories attempt to do just this. Richerson and Boyd only confound the issue by inserting propositional attitudes in the guise of cultural variants between behavior and its consequences.

If the metaphor doesn't fit, don't wear it

I would like to conclude this review by returning to the issue of the utility of employing explicitly biological metaphors in models of cultural selection. Both Distin and Richerson and Boyd are fascinated with biological analogies whenever these appear to apply to a cultural process. They also willingly jettison the use of biological metaphors whenever they seem out of step with a cultural-learning process. One gets the sense, however, that they keep a close tally of the number of relevant metaphorical applications they can claim and regard their theories as strengthened by such successes. I would argue that these metaphors are irrelevant and distracting, and generally lead us in a non-useful direction. A case in point would be the issue of whether some memes are like viruses instead of genes. Dawkins (2003) developed this metaphor to characterize what he considers to be the pernicious effects of religion. Distin, who closes her book with a quotation from Ecclesiastes, takes issue with the virus metaphor. Distin argues that Dawkins is mistaken to characterize 'bad' memes such as religious ideas as viruses and oppose them to 'good' memes like science, which are not like viruses. The virus idea is bad because viruses always are parasitic on DNA replication. No such relation is possible when it comes to memes because memes do not create their own means of replication and so there is nothing for a virus to hijack (SM, p. 76). In other words, memes are not like viruses and they are not like genes either! It is significant that the only time Richerson and Boyd endorse the selfish-meme concept is when they compare certain cultural-transmission phenomena to viruses. For them a virus is a rogue cultural variant, a socially pernicious meme that manages to perpetuate itself (NBGA, pp. 154–5, 174). The lesson to be learned is that it might be best to just do away with these inherently flawed and misleading metaphors.⁴

Finally, I want to comment on the division of labor between scientists. Evolutionary biologists have long drawn a useful distinction between *proximate* and *ultimate* causes. The job description of evolutionary biologists (and evolutionary psychologists) is to figure out the evolutionary history of proximate mechanisms for whatever species they study. Neurobiologists' and learning and conditioning psychologists' job descriptions obligate them to figure out how these proximate mechanisms operate. Each discipline has a separate toolbox, which contains tools appropriate for its given tasks. Does it make sense to go into the Darwinian toolbox and pick out tools designed for other purposes in order to reinvent the operant-conditioning wheel? The operant-conditioning toolbox already exists. Its tools are useful for telling us a great deal about learning and conditioning, but these tools cannot tell us 'why sugar is sweet'. They cannot give us the evolutionary history or the physiology that underlie reinforcement mechanisms. Determining the nature of the motivational endowments that guide and constrain learning requires a different set of tools. These latter tools are located in the evolutionary and neurobiological toolboxes. It's time we used the right tools for the right tasks. Aren't we supposed to be the species that can learn from the cumulative experience of others?

Alex Walter is currently a part-time lecturer in the Writing Program at Rutgers University. Recent publications include 'The Anti-Naturalistic Fallacy: Evolutionary Moral Psychology and the Insistence of Brute Facts', *Evolutionary Psychology* 4: 33–48 (2006) [available online at: www.hbes.com]; he is also the co-author of 'The Westermarck Effect and Early Childhood Cosocialization: Sex Differences in Inbreeding Avoidance', *British Journal of Developmental Psychology* 21: 353–65 (2003). *Author's address*: 54 Hassart Street, #B4, New Brunswick, NJ 08901, USA. [*email*: alex.walter@rutgers.edu]

Notes

1. See Sahlin (1976) and Geertz (1973, ch. 2 and 3; 1980).
2. By polythetic, Lumsden and Wilson (1981) refer to a cluster of similarities such as those revealed through the use of the statistical technique of multidimensional scaling.
3. By contrast Lumsden and Wilson (1981: 16) explicitly link culturgen success to genetic success.
4. It should be noted that Sperber's (1996) 'epidemiology of representations' is based on the virus model.

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