

mentally privileged. For example, children might first be disposed to distinguishing life forms. It is the level at which perceptual similarity is maximized for midwestern adults and it corresponds to the basic level of Rosch et al. (1976). Atran deserved credit for pointing out that it is at the generic species level that the induction of biological information is maximized, but is it maximized for very young children? And what do they consider to be biological information? Atran's induction studies were conducted with adults and involved inferences about disease, a phenomenon understood only incompletely by children (Solomon & Cassimat 1996). Might it be that it is only after children understand biological principles that they come to expect that relevant biological information will maximally covary by generic species? These are empirical questions.

In any case, even given an initial set of classes, the construction of a folk biology might proceed by processes that are not unique to the domain. For example, Gelman and her colleagues (Gelman & Hirschfeld, in press) may be right that a general essentialist bias could provide the inferential engine that drives children to discover causal mechanisms that underlie a coherent folk-biological domain. In short, I submit that it is not implausible that children could come to construct an adult folk biology, with the universal characteristic Atran has described, from more limited innate constraints and more general learning mechanisms.

A final comment: Mention of Roger Brown's (1958) concept of the level of usual utility has been conspicuously absent in recent discussions, even though it introduced to psychology the notion of a privileged level of naming and classification. His work is worth noting, even beyond historical fastidiousness, for his emphasis on "the functional structure of the . . . world" (p. 16) is still relevant to research on cultural differences in classification judgments.

Are folk taxonomies "memes"?

Dan Sperber

CREA, Ecole Polytechnique, 75005 Paris, France.
sperber@poly.polytechnique.fr

Abstract: This commentary stresses the importance of Atran's work for the development of a new cognitive anthropology, but questions both his particular use of Dawkins's "meme" model and the general usefulness of the meme model for understanding folk-taxonomies as cultural phenomena.

The American "cognitive anthropology" of the 1960s and 1970s (reviewed in D'Andrade 1995) focused on the study of individual "cultural competence" as revealed in folk classifications. Notwithstanding some major advances achieved from this early cognitive anthropology (the most famous being Berlin & Kay [1969] on color classification; see also Saunders & van Brakel: "Are There Nontrivial Constraints on Colour Categorization?" *BBS* 20(2) 1997.), its cognitive dimension was shallow and its anthropological dimension amounted to little more than doing cross-cultural comparisons, with little interest in social-cultural mechanisms. In particular, hardly any attention was paid to domain-specific cognitive mechanisms on the psychological side, or to processes of cultural transmission on the anthropological side. In the 1970s and 1980s, biologically oriented researchers developed Darwinian models of cultural transmission but paid little or no attention to cognitive mechanisms (see Boyd & Richerson 1985; Cavalli-Sforza & Feldman 1981; Dawkins 1976; Durham 1991; Lumsden & Wilson 1981). In the 1980s, developmental psychologists (e.g., Susan Carey, Rochel Gelman, Susan Gelman, Frank Keil, Alan Leslie, David Premack, Elisabeth Spelke), evolutionary psychologists (e.g., David Buss, Leda Cosmides & John Tooby, Donald Symons), and some cognitive anthropologists (Scott Atran, Pascal Boyer, Lawrence Hirschfeld, Dan Sperber) converged on the idea that the human mind contains several, possibly many, domain-

specific conceptual mechanisms (comparable to Fodor's input modules, but at a conceptual level). The relevance of this view of the human mind to the understanding of human culture, and the need to integrate it with the study of cultural transmission was highlighted at the conference on "Domain specificity in cognition and culture" held at Ann Arbor, Michigan in 1990 (Hirschfeld & Gelman 1994, see also Tooby & Cosmides 1992). Atran's work (along with that of Boyer [1994] and Hirschfeld [1996]; see also Sperber [1996b]) is an outstanding contribution to this new wider and deeper cognitive anthropology.

One particular merit of Atran's contribution is that it articulates the individual cognitive dimension with a macro-historical dimension. He shows how the historical development of natural history is rooted in a cognitive disposition to order living kinds in a domain-specific way. However, although he makes some interesting suggestions, he glosses over the articulation of individual cognition and micro-processes of social transmission, and this is where I would like to propose some constructive criticism.

In section 1.3, Atran chooses to borrow Dawkins's (1976; 1982), term "meme," giving it a new twist. He proposes distinguishing two kinds of memes: "core memes," such as folk-taxonomies of living kinds, and "developing memes," such as scientific research programs. Core memes, he argues, replicate more easily and with a higher degree of fidelity than developing memes. Unlike developing memes, core memes do not need institutional support or the help of metacognitive abilities to replicate. Their existence helps explain commonalities across cultures that are not the result of common descent or diffusion.

Dawkins's original "memes" are genuine replicators: They are items in the mind of individuals that cause behaviors that cause replicas of those very items to be constructed in the minds of others. There may be more mutations of memes than there are of genes, but for the whole idea to be a serious explanatory proposal rather than a loose and potentially misleading analogy, it is essential that the rate of mutation still remain low enough for the Darwinian model of selection to apply. I have argued (Sperber 1996b, Ch. 5) that most transmission of information among humans involves some degree of transformation of the information; hence the replication-cum-selection model is not generally applicable. The stability of cultural phenomena such as myths, rituals, techniques, folk-taxonomies, moral codes, and so on is, in general, caused not by genuine processes of replication, but by the fact that the transformations involved in transmission tend to cancel one another out by gravitating toward the same point in the space of possibilities (hence I have argued that cultural evolution is to be explained in terms of attraction rather than selection). The taxonomies Atran discusses are good examples of this.

As examples of "core" and "developing memes" Atran cites folk-taxonomies and scientific programs, that is, large conceptual systems. Their transmission to any one individual typically involves repeated interactions with many people over many years. Moreover, although it is true that a folk-taxonomy is a highly stable component of a people's culture, this does not imply that all or even most adult members of a society have the same mental taxonomy. On the contrary, degrees of competence vary greatly, and most individuals have only an incomplete and idiosyncratic version of the global folk-taxonomy of their culture. Not all people know the same plants, for example, or the same things about the plants they know. Even experts often disagree. In oral cultures (e.g., the Dorze of Southern Ethiopia, where I did my anthropological fieldwork), experts typically disagree more than they are aware, because their respective mental taxonomies are only confronted occasionally, a propos of specific plants or animals, and cannot be matched to any permanent, written, canonical version. The stability of folk-taxonomies is not an effect of "high-fidelity copying," but of the fact that most "failure of copying" results in mere lacunae rather than divergences, and divergences among experts tend to concern only marginal instances. This is very different from genetic replication, where failure to copy chromosome fragments typically amounts to mutation.

The meme model could be defended here by arguing that the real memes – that is, the real replicators – are not whole taxonomies, but individual taxons such as the concept of an oak, or that of a sparrow (whereas whole taxonomies are only “memelike” by extension). With oaks and sparrows, indeed, a single interaction (e.g., pointing and naming) between knowledgeable individual and an ignorant one may be enough to cause the latter to acquire a version of the concept of the former. Individual words, and, among words, proper names and living-kind terms, are possibly the best candidates for meme status. Showing that they themselves are not very good replicators should cast doubt on the validity of the whole meme model.

Word phonology is copied from speakers by language-learners. Moreover, to learn, say, the word “oak” (not just the sound, but also the meaning) is, arguably, to deploy, even if imperfectly, the ability to refer to oaks and only to oaks as “oaks.” So it might seem, that not just the sounds of words, but also their meaning gets faithfully copied. Well, not quite. From the causal-explanatory point of view, which is the only one that matters here, two individuals may use the same phonetic form to denote the same things and yet do this by means of mental structures that are otherwise functionally quite different in that they have different possibilities for making inferences.

I am convinced by Atran’s claim that users of the word “oak” mentally represent it as a taxon of specific rank, denoting a kind with an underlying essence. However, individuals’ views of the essence of oak determine their inferences, expectations, and so forth; in other words, much of the role “oak” plays in their mental lives and, in particular, in their decision to communicate about oaks, and so forth. Different individuals may have different views of the oak essence. The same denotation does not amount to the same concept in the psychological sense of the term where a concept is best seen as a knowledge structure. The stability of folk taxonomies may be caused on the one hand, by the innate disposition Atran discusses, and on the other by the relatively high discriminability of living kinds in local environments. This is enough to secure identity of reference and of the general format for living kind terms. However, actual mental concepts thus anchored in their referents may well vary from individual to individual in their content. If so, the actual concepts do not replicate, they merely generate, through communication, versions gravitating around some prototype of their common referent.

As I mentioned, living kind terms are, *prima facie*, among the very best candidate for meme status; yet a careful look at micro-processes of transmission and acquisition suggests that there is much more idiosyncratic construction and variation than the meme model would predict. Most other candidates for meme status are even much less “memelike.” What Atran calls “developing memes” such as scientific programs, are, just like folk-taxonomies, transmitted in a piecemeal fashion, and with much poorer replicability, as all science teachers know. Moreover, it is part of the scientific enterprise to try not to replicate but to augment, correct, or subvert previous scientific concepts, theories, and so forth. Atran’s own version of Dawkins’s “meme” is best described, if not as a “failure to replicate,” then as a deliberate modification of the original idea. The idea of a meme in its many forms has done much work for us as an insightful metaphor. Now the insight should be converted into some serious theorizing in which the strict notion of a meme (i.e., a cultural replicator) may have little role to play, and where a loose version of it is best avoided altogether.

Cognitive universals, hierarchy, and the history and practice of biological systematics

P. F. Stevens

Harvard University Herbaria, Cambridge, MA 02138.

pstevens@oeb.harvard.edu

Abstract: The hierarchical reach of Atran’s cognitive universals is unclear, and some of the key concepts used to discuss them are notorious for their imprecision. Although ideas of class hierarchy pervade Atran’s discussion, other ways of thinking are also allowed. The history and practice of systematic biology suggests that a nonclass hierarchical and continuity-based way of thinking has been common there until recently.

If there are cognitive universals that structure biological classification in particular, rather than classifications in general, then our understanding of the history of systematics, as well as its present practice, will need revision. For Atran, a cognitive universal for humans is the existence of a rank of generic species, a “causally essential category” (sect. 1.3, para. 14). Generalisations and predictions are made about members of this rank, as shown by experiments on the Itzaj and Michigan students that Atran reports; indeed, he explains away those parts of his results that do not quite fit (sect. 1.2.2.3, para. 1). But Atran repeatedly suggests (sect. 1.1, para. 1; sect. 1.3, para. 1; conclusion, para. 1) causal relations between different hierarchical ranks. Although the general structure of classifications is similar (Holman 1992), exactly how deeply Atran expects cognitive universals to extend into the hierarchy is unclear.

Furthermore, several concepts important for understanding the ideas and experiments discussed are imprecise. Thus, phenomenal salience is linked with phylogenetic isolation (sect. 1.2, para. 2), and attempts made to compare “readily perceptible evolutionary gaps” (sect. 1.2.2.3, para. 3) across environments. However, cacti (for example) are not genealogically isolated and can be grafted with plants of two other families; so what is meant by phylogenetic isolation or evolutionary gaps?

Although the distinction between life and nonlife is critical if folk biology is a core domain, Atran is uncertain exactly how folk make this distinction (sect. 1.3, para. 8). Western systematics took a long time to distinguish between animate and inanimate objects and between plants and animals. My reading of the literature in which children’s categorisation of the world is explored is that plants are not conceptualised in exactly the same way as animals, although they are certainly not treated like inanimate objects, either.

Atran is interested in the extent to which folk practice is evident elsewhere. He discusses the differences between Michigan students and Itzaj in diversity-based (hierarchical, taxonomic) reasoning; Americans commonly reason this way, and the Itzaj do not (sect. 2.1.2.3-4; sect. 2.1.3). Atran also suggests that science has a marginal role for American folk; they do not have the theories to make successful diversity-based predictions. However, diversity-based reasoning is itself broadly consistent with evolutionary theory, and may have been one thing the student picked up from their classes. Their classification – also theories, but at a lower level – may be wrong, but that is another matter. So scientific theory may affect taxonomy via how classifications or relationships (used in a general sense) are interpreted – which is in part how evolutionary theory affected systematics.

The last two paragraphs introduce a recurring theme in systematics. The cases of nondiversity-based reasoning are basically “folk” examples (Note 14), but our understanding of the living world is clearly not mediated by hierarchical relationships alone. O’Hara (1996) suggests that beginning college students often see relationships as being directly between extant groups, not hierarchically. Similar ways of understanding nature have been prevalent in systematics (Cuerrier et al. 1996; Stevens 1994b); the *scala naturae* is only one example of this. Indeed, in the eighteenth and early nineteenth century philosophers and naturalists like Adam