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Dan Sperber. 1996. *Explaining Culture: A Naturalistic Approach*. Oxford: Blackwell. Pp. 175. ISBN 0-631-20045-2

Reviewed by Hiram Caton

This book is comprised of six essays, published over a decade, and now brought together to promote an evolutionary approach to culture. The author, an anthropologist by training, is Directeur de Recherche, Centre National de la Recherche Scientifique, Paris. The present offering complements previous books on symbolism, communication, and epistemology. Its objectives are to 'rethink anthropology' to give it a naturalistic basis; to generate the framework for describing and explaining the spread of beliefs and practices; and to promote collaboration between anthropology and evolutionary psychology.

The main objective is to make anthropology and psychology partners in the construction of a theory of culture centered on what the author styles 'the epidemiology of beliefs' (Chapters 3-5). Epidemiology examines the factors determining the frequency and distribution of diseases in a population. Similarly, the aspiring culturology will map the frequency and distribution of beliefs in a population. To explain culture 'is to [demonstrate] why some [beliefs] are more successful in a human population, more "catching," than others' (p. 58). Again: 'The culture of a given population is described as the distribution of mental representations and public productions'. Accordingly, the evolution of culture is the 'cumulative effect of differences in frequency between different possible transformations of representations and of productions in the process of transmission' (p. 118). Explaining differences in prevalence entails identifying the causality that links subjective experience ('mental representations') with public expressions of cognition ('public representations'). This causality is a two-way traffic (p. 62).

To move forward we must lift anthropology's taboo on psychology. When Durkheim and Boas banished it, they expelled a discipline that was then biological, medical, and in some measure evolutionary (especially in France). On this basis they erected the standard social science model of culture as an autonomous domain, with the corollary that mental representations are stamped into individual minds by public representations (one way traffic). Today there is a new evolutionary psychology, whose central motif is the mind's 'modularity'. The mind is modular because the brain is modular. The hemispheres are modules. So are the reptilian, mammalian and neocortex strata that together comprise the 'triune' human brain as a 'good enough' pastiche from the adaptive past. The hippocampus, hypothalamus, &c. are modules. In addition, anatomically continuous areas of the brain are nevertheless specialized for processing different

kinds sensory, motor and cognitive information. There are lots and lots of modules.

This broad description of brain modularity is the basis for a robustly developed neuropsychology that maps hundreds of psychological processing modules onto neurological correlates (Gregory 1987). To me it was obvious that anyone pursuing Sperber's project would commence by indexing this rich store of knowledge. To my astonishment Sperber doesn't even mention it. His starting point is Jerry Fodor's *The Modularity of the Mind* (1983), a cognitivist speculation that forges no links with evolutionary theory or with the empirical psychology just mentioned. However, Fodor's ideas were adapted to empirical psychology, and the sub-field called 'evolutionary psychology' made its debut in the Barkow, Cosmides, and Tooby edited volume, *The Adapted Mind* (1990). Sperber seems to align his new culturology with this sub-field. Psychology is meant to identify specialized cognitive functions, or modules, that bias the mind's thought processes. This in turn affects the probability that a given belief will be transmitted and preserved. Culture is explained when the determinants of the stability of beliefs are ascertained (pp. 65-70). The dynamics of belief transmission are not selectionist, Sperber thinks. Beliefs ('memes' in Dawkinsese) are not replicators because they are usually transformed by transmission. Hence the driving force of cultural evolution isn't a fitness competition but the stabilization of beliefs around modules, which act like 'attractors' of beliefs. This Sperber styles the 'attraction' theory of belief transmission.

What would such a culturology look like? Consider first what it will not look like. The author references Boyd and Richerson's *Culture and the Evolutionary Process* (1985), Cavalli-Sforza and Feldman's *Cultural Transmission and Evolution: A Quantitative Approach* (1981), Lumsden and Wilson, *Genes, Mind, and Culture* (1981), and Richard Dawkins' memetics. These studies assume that the equations of population genetics approximately model the transmission of culture items. The reasons supporting this assumption are complex. Suffice it that recently bridges have been built from population genetics to game theory, which in turn has become a tool for the analysis of the collective behavior of living systems (Sigmund 1993; Emmeche 1994, not referenced by Sperber). The author states: 'if and when we need mathematical models of cultural transmission, I doubt that we can borrow or easily adapt standard epidemiological models. Similar comments would apply to other biological models of culture [based on population genetics]' (p. 59). The trouble with mathematical models is that they inadequately explicate the qualitative biases that in Sperber's view are the actual mechanisms of cultural transmission (pp. 58-59).

These statements come close to throwing in the towel. If mathematics isn't a suitable tool of culturology, why define culturology as a description of belief distribution? Besides, Sperber's criticisms aren't valid. Lumsden and Wilson's 'epigenetic rules' are psychological predispositions, aka 'modules', 'biases'. Boyd and Richerson model basic behaviors, such as altruism and cheating, which translate into the terminology of other authors. Evolutionary psychologists strive to quantify 'Darwinian algorithms' (aka 'epigenetic rules', 'biases') that are domain

specific rules for information processing and transformation. Nonlinear game theoretic simulations distinguish replication from transformation and indeed excel in the graphic representation of transformations flowing from iterated games (Nowak & Sigmund 1992).

So much for population genetics and game theory. But what about Sperber's archetype science? Isn't epidemiology quantitative? He says of it: 'Epidemiologists have constructed sophisticated mathematical models of the transmission of disease, and it is tempting to try and apply them to various forms of cultural transmission This is the line taken by Cavalli-Sforza and Feldman (1981). While their work is worth paying attention to … they underestimate the important differences between the transmission of diseases and cultural transmission. At the same time they fail to appreciate deeper similarities between the epidemiology of diseases and that of [beliefs]' (p. 58). The disanalogy that Cavalli-Sforza and Feldman neglect is the difference between microbe reproduction and belief transformation. The similarity they overlook is that the distribution of microbes/diseases can't be understood 'without taking into account the manner in which they affect the organism', i.e., the specific pathology. Pathogenic conditions are like those roadways on the cognitive map where the predispositions (aka epigenetic rules, Darwinian algorithms) are located. —This, it seems to me, is a misunderstanding. The population perspective is meant to abstract from the particularities of pathology. If we find ataxic gait among Groot Islanders but not on neighboring islands, we look for a site-specific etiology and find it in the toxic tailings of an open-cut mine.

The choice of epidemiology as the model science seems to be based on nothing more than the insinuations of English idiom. Idiom likens the spread of ideas to contagion. We say that ideas, moods, personalities, and fads are infectious. Rumor and disaffection spread like fevers through the body politic. Cheerfulness is contagious—smile and the world smiles with you. But usage provides no clue to causality. It is equally content with mechanical metaphors, such as the 'band wagon effect' and the 'climate of opinion', while outbreaks of frenzy, mania or hysteria are likened to floods, cyclones and wild fire. Idioms are heedless of the vast difference between plague and weather as transmission mechanisms. Oddly for an anthropologist, Sperber takes no notice of these clues to how the natives perceive thought transmission. An assessment must be made, because we must avoid confounding 'good enough' idiomatic analogies with causal mechanisms. Unfortunately some have confounded them. In Aaron Lynch's *Thought Contagion*, 'memetic science' consists of a metaphorical germ theory that suppresses empirical psychology so that personal speculations may flourish (Lynch 1996).

My suspicion that epidemiology is a red herring deepened on reading Sperber's account of what the new culturology does look like. This happens in Chapter 5, Selection and Attraction in Cultural Evolution. On pages 109 and 112 he introduces graphs representing the spread and transformation of beliefs under the influence of 'attractors'. Attractors are characterized in two ways. In one statement, an attractor is 'an abstract, statistical concept, like a mutation rate or a transformation probability' (p. 111). Not much is said about it. A cultural attractor,

however, is a specific practice or model. Manners, rituals, architectural styles, and resource-rich environments illustrate. Sperber has more to say about cultural attractors. A piece of culture is likely to become an attractor to the extent that it is the shortest distance between an initial condition and a beneficial outcome. This concept is usually called 'optimality', but the author calls it the 'effect-effort balance', where the 'processing of any given piece of information determines its degree of relevance' because behavior tends toward actions in which 'the intended effect can be achieved at minimal cost' (p. 114). Many attractors are unique to individuals; others, as gene-linked algorithms, cut deep channels through all populations, e.g., critical learning times and courtship strategies. The stability of cultural practices is due to the fact that they are 'attracted' to these natural psychological channels and their presumed neural or genetic substrates.

Let's go back to the abstract concept of attractors. Sperber provides a three page exposition meant to illustrate the difference between replication and transformation, and the stable combination of replication and transformation processes in a population. The combinatorial space is represented by a cellular matrix. He assigns cell types in some arbitrary quantity, and combinatorial possibilities to each type. The matrix now describes a combinatorial state space. An engine is needed to activate cell 'growth'. Sperber doesn't say what the engine is, but once it starts, the initial random distribution of cells in the matrix begins to alter. With each generation (or turn of the engine's wheel), the distribution of cell types changes. Patterns emerge as iterations continue; eventually we see patterns aggregating around two attractors. What is happening here? Sperber's matrix reminded me of cellular automata, the discovery by Cambridge mathematician John Conway that led to nonlinear interpretations of game theory. Cellular automata with simple combinatorial instructions programmed into computer graphics are capable of remarkable behavior (Nowak & Sigmund 1992). Some instructions yield homogeneity, some express fractal self-similarity, and still others cross the boundary between stability and chaos to bifurcate into ramified local structures in the basins of chaotic attractors. The engines of these transformations are recursive nonlinear equations. Could this be the inspiration of attraction theory? In footnote 34, p. 158 he writes: 'Sophisticated notions of attractors . . . have been developed in complex systems dynamics [aka nonlinear theory, chaos theory, self-organization theory, fractals theory], and may well turn out to be of future use in modeling cultural evolution, but a very elementary notion of an attractor will do for the present purpose'. I surmise that Sperber's cell matrix abridges the cellular automata concept to eliminate quantification. But that also eliminates the possibility of applications.

Sperber appears to be the victim of a predicament shared by nonmathematicians attracted to evolutionary theory. He wants to mine it for the purposes of his own theory-building. But how is one to integrate formalized biology into qualitative theory? Collaboration with a mathematician would be ideal, but that rarely happens. For the most part we sniff at formalizations, digest what we can, absorb the qualitative descriptions of structure and outcomes, and finally dumb it down to qualitative 'theory'. This seems to be what Sperber has done. In Cavalli-Sforza he found a daunting quantification of the common sense perception of thought

transmission (epidemic). Years later, he discovered cellular automata and attractors. The mind-microbe analogy mutates to pacman activity on a computer screen. As far as I can judge, Sperber simply laminates the late (1995) discovered attractors onto the earlier encounter with epidemiology. The author adapted, but did not update his earlier thought to make it consistent.

Sperber's guess that nonlinear theory may eventually simulate the microprocesses of belief transmission is behind the times: it is a going concern. Simulations began in engineering, informatics, and the physical sciences decades ago (Gleick 1988). Applications to the study of animal behavior commenced not long after—Nigel Franks and associates have achieved stunning results in their studies of ant nesting and foraging patterns (Franks 1989). At about the same time, Chris Langton and Stuart Kauffman at the Santa Fe Institute launched the now burgeoning field of artificial life, which simulates multi-agent intelligent behavior, whether the agent be robots, biods, animals, or humans (Emmeche 1994). Applications to human beliefs include collective decision making, imitative replication, social diffusion, and macroeconomics (Helbing 1992, Helbing 1994, Aoki 1994, Akimov & Soutchanski 1994). Indeed the rapid spread of nonlinear analysis is itself an instance of thought 'contagion'. The reasons are not far to seek. Nonlinear systems discover geometries and combinatorial possibilities undreamt until now. Exploring them is enormous fun if you are a mathematician. As applied science, nonlinear analysis finds deterministic order in apparently chaotic phenomena unresovable by conventional analysis. This is exciting and can earn you a good living. The theory is apposite as a tool for the social sciences because it shows that unpredictability—a hallmark of social phenomena—is compatible with deterministic processes. This combination is paradoxical to most social science theorists because they don't know nonlinear concepts.

I cannot feel hopeful about Sperber's proposed marriage of anthropology to evolutionary psychology. Now that television has ended the isolation of the last savages, anthropologists have no fields to plow. They have no distinctive interpretation of culture; Sperber notes that 'anthropologists don't agree on anything'. Even if they did agree, the discipline has no capacity to transmit an authoritative concept of culture to the other social sciences, especially now that the roots of culture are firmly located in nonhuman species (Gardner, Gardner, Chiarelli & Plooij 1994). Ethnography will endure as an important if unreliable record, but there seems to be no point in resuscitating a conceptual apparatus whose weaknesses were clearly exposed by Derek Freeman's examination of Margaret Mead's classic work.

Evolutionary psychology is also time-warped. Modularity is not a new idea. Together with the experienced unity of consciousness, it is one of the most obvious facts about mind. Its empirical elucidation may be dated to 1861, when the French surgeon and anthropologist Paul Broca discovered the localization of active speech function (Broca's area). This discovery was soon followed by the discovery of the localization of motor function (Frisch & Hitzig) and receptive speech (Carl Wernicke). To 'discover' this central principle of neuropsychology is to reinvent the wheel—and to march backwards to a work space in which the rich

store of knowledge of modularity is obliterated. Evolutionary psychologists have merely devised a cognitivist approach to psychological modularity that displaces brain wetware with questionnaires and lab experiments on that notoriously biased sample, undergraduate psychology students.

Evolutionary psychology's programmatic approach to evolution is also doubtful because it requires the construction of a picture of human behavioral evolution. Paleontologists and physical anthropologists have been at this for a century, with mixed success. The knowledge base consists of islands of intricate facts connected by oceans of speculation. The predicament is hard to mend because the object to be described, Pleistocene behavior, has vanished forever. Method has devised a way around this inconvenience, in the assumption that modern hunter-gatherers are living fossils of the Late Upper Pleistocene. But this period is only one time slice from a two million year evolutionary phase. It takes no account of the cascade of cultural evolution between 15,000-5,000 BP. Hunter-gatherers did not participate in the cultural revolution and they experience difficulty adapting to the civilized conditions it wrought. Is there something about hunter-gatherer genotypes that confers 'immunity' to culture expansion? We do not know, but the question shows the precariousness of the standard assumption that hunter-gatherers fossilize our species' past.

If Sperber's effort does not attain its theoretical objective, does it present some concrete insights on the transmission of thought? I'm afraid the answer is No, at least for me. I found no discussion of recognized types of transmission—panics, crazes, cults, sports mania, medical scares, propaganda, advertising, mobbing, and the like. As for identifying the transmission microprocesses, his message is confused. Germ theorists, I have explained, do not identify the somatic process corresponding to infectious disease. But Sperber has an alternative cognitivist position: he proposes that inferences mediate cognitive processing (pp. 85-90). But what do inferences operate on? On sensorimotor information, as he acknowledges. It is a truism that many inferences are already 'in' the senses. Here, I suggest, is the clue to the fugitive microprocesses obscured by germ theory. The correct, nonmetaphorical term is 'communication'. Communication is not pathogenic and medical models are relevant only to actual diseases or injuries to organs used in communication. The literature on the microprocesses of animal and human communication is massive (Caton, Salter, & van der Dennen 1993). Let me mention a few points that Sperber should have discussed. •The human use of language is built upon archaic communication mechanisms, generally called 'nonverbal'. Their antiquity is the reason why we can communicate with many vertebrates, even with some invertebrates, and they with us. •Nonverbal communication is comprised of sensorimotor responses and rapid feedback, most of which is unconscious. Ethologists and neurologists have compiled a rich literature of description and causal elucidation. •Verbal communication expresses the 'language instinct' and it works because, like nonverbal communication, its signal channels are characterized by substantial automaticity and redundancy. Thus, from conscious experience alone, we have no clue to how we generate speech or recall the past. It 'just popped into my mind'; or 'it's on the tip of my tongue'. Techniques for elocution and recall automate these processes by forming

good habits. The habituation method probably reflects underlying neural automaticity. •Studies of inference show them to be childlike leaps of faith. The leaps are often patterned, e.g., the pattern that constructs belief in a just world as an 'attractor', or the leap of faith to 'luck'. This cognitive dumbing down can be organized into systems of social exchange, as Las Vegas exploits the Toad-to-Prince fantasy to make asymmetric economic exchanges fun, and as the inferences pertaining to the just world belief are ingredient to many belief systems. By putting together the facts of nonverbal communication with the facts of 'attractor' inferences, we make a good start on explaining thought transmission. But we need one more element: recognition of the evolutionary functions of communication. The principal function is the self-organization of the traffic of social interaction. It is self-organizing because communication is what it is in virtue of feedback; and feedback is the signature of nonlinearity This is not a guess about future directions. Information theory and the technology of parallel processing depend on nonlinear interpretations of the physical processes involved. Applications of nonlinear systems to animal and human communication have been mentioned.

Have I perhaps have overplayed the importance of the nonlinear revolution? I think not. The evolutionary theory deployed by most investigators and popularized by Richard Dawkins derives from the formidable conceptual apparatus assembled in the population genetics of R. A. Fisher. It is the acknowledged basis of current orthodoxy (Dawkins 1996). Yet it is out of date, partly owing to progress in genetics and molecular biology (symbiosis, gene transposition, reverse transcriptase, directional mutation, apoptosis, somatic hypermutation, hierarchy), and partly because nonlinear systems readily handle processes inaccessible to Fisher's linear mathematics. He had no choice but to postulate that genes are in a linear relationship with phenotypes, even though he knew that this simplification is not generally true. Today we know that it is hardly ever true. Yet this assumption founds current orthodoxy and its signature doctrine that species and taxa emerge by cumulative changes at alleles, so that macroevolution is just allele substitution indefinitely continued. As long as linear models were the only show in town, it was hard to reject this implausible idea. But today there is another show in town. Alternative to adaptation by selection is growth by self-organization. Self-organization is a genuine theory of emergence, in contrast to 'just so' stories that conjure up the evolution of the elephant's trunk from mere word pictures constructed from dumbed down physiology (Kauffman 1993). Most of the basic concepts of orthodox Darwinism have been reformulated in nonlinear language (Emmeche 1994; Sigmund 1993; Kauffman 1993; Holland 1995). For a readable introduction, I recommend Karl Sigmund's *Games of Life*, which showed me how to think of evolution as nature's self-organizing play.

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