

## Some Caveats about Cultural Transmission Models

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The symposium papers at hand apply a population-biological modeling approach to the problems of cultural transmission and cultural evolution. The papers by Boyd and Richerson, by Chen *et al.*, and by Pulliam are conceptually very similar, each advancing a variant on the additive transmission model of Cavalli-Sforza and Feldman (1981), in which the probability that pseudoparticulate "traits" [essentially similar to Dawkins' (1976) "memes" and Lumsden and Wilson's (1981) "culturgens"] will be replicated in a focal individual is an additive function of their separate presences in that individual's "culture parents." Durham characterized this approach well by noting that the models view "cultural change as the outcome of an individual-level process of differential transmission." (p. 304). Perhaps many customs, techniques, neologisms, and so forth are propagated by processes resembling those modeled, but for the sake of injecting a little controversy into the discussion, I will argue that this approach is based on a weak analogy. Such analogic models may be heuristic at early stages of analysis, but they can become at best irrelevant and at worst impediments to progress as the analysis of process continues.

The second set of critical comments applies only to Durham's paper. Despite his laudable and sometimes persuasive efforts to analyze real examples of complex cultural change, I believe that Durham's analysis of "coevolution" is fundamentally wrong-headed.

Finally, I will briefly suggest that evolutionary biology indeed has something to offer students of social influence and cultural change, something other than an analogy with evolutionary population genetics.

Invited to comment on the four papers in this symposium, I feel compelled to a dialectical stance: such commentary, if it is to be at all constructive, must be critical.

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### WHAT IS "CULTURAL TRANSMISSION"?

According to Boyd and Richerson, "There are . . . forces that act on the distribution of a culturally transmitted trait that do not have analogs in the usual models of population genetics. These forces arise for two reasons" (p. 335). The first is that innovation is not random in its effect like genetic mutation, but can be Lamarckian. The second is "biased transmission." Fair enough, but these are by no means the only place where the population genetic analogy fails. I wish to draw attention to three others.

#### There Are No Segregating Particles Underlying Culture

Not only do cultures have a functional interrelatedness, but a single change in the beliefs or practices of an individual can lead to a major cognitive reorganization (see e.g., Cialdini *et al.*, 1981), with the number of "traits" that are correlatively modified limited only by the imagination of the investigator. Adding a "linkage" concept will not save the analogy, since the transmission of traits as correlated packages is only part of the problem. To a population geneticist, genes are partial determinants of phenotypic traits. But to what element in this mode are the "traits" in the cultural transmission models analogized? Memes and culturgenes are clearly explicit analogs of the gene; Cavalli-Sforza and Feldman (1981) appear sometimes to follow a similar model (e.g., pp. 70-71). Yet, surely it is more appropriate that the *traits* in the classes of models correspond; they really seem to refer to the same attributes. Durham recognizes this in referring to genetic and cultural "instructions" as codeterminants of traits. What, then, is to be the gene's analog? What transmissible "particle" can we envisage, distinct from the traits themselves, with "polygenic" (polymemic?) effects on a variety of traits? The model of Mendelian genetics remained successful because there really *was* such a particle. The cultural model has none.

#### Traits Are Not Immutable

An individual is stuck with his genotype, and the present models saddle individuals with immutable, "culturally transmitted" traits as well. This is one point at which the trait seems to be analogized inappropriately to the gene, as discussed above. No doubt this is a simplification for analytic convenience: the models might become impossibly complex with lifelong "reeducability" added. But the simplification is so unrealistic as to prejudice seriously the utility of the models. It leads the various authors to treat the human lifespan as consisting of two nonoverlapping stages: influencee and influencer. This in turn makes the models unidirectional and generational.

Chen *et al.* may protest that their incorporation of sibling influence adds “horizontal” channels that are not generational, but the unidirectional nature of modeled influence makes the siblings “culture parents” of a former “generation.”<sup>2</sup> These features, in conjunction with the further analytic conveniences of limiting the number of culture parents (unrealistically restricting networks of social influence) and plucking these influential individuals from a pool (denying “opinion leaders” and innovation “legitimizers”), produce models in which cultural evolution is rate-limited by species-typical “transmission bias” parameters and by transmission channel viscosity (especially the partial social isolation of “familial” influence and the necessity for generations of “exogamy” to diffuse the trait, properties that are not alleviated by letting people who are not genetic relatives be “culture parents”).

People are more facultative strategists than these models allow. The spread of cultural change must often be limited not by network limitations and transmission biases but by the extent to which individuals perceive their self-interest to lie in the maintenance of tradition or in its subversion. Cultural options are not always (nor perhaps even usually) things like alleles that “compete” for limited “loci” in individuals. Such an analogy obscures the fact that individuals are commonly aware of several options, decide which to practice, and can switch (and switch back) when their assessments of the costs and benefits of the options change.

### Cultural “Transmission” Need Not Be Replicative

A further artificiality of the present models is that the only kind of influence permitted to culture parents is that of passing on replicas of their own traits. If ever the admonition to “do as I say, not as I do” is observed, then we have a fresh complication. Perhaps more importantly, social influence also involves negative modeling and observation of failure: the bad example is a culture parent to those who choose not to follow in his or her footsteps.

Even where influence is imitative, the result need not be replicative. In a chapter entitled “The limits of transmission,” Reynolds (1981) notes that transmission models treat recipients as “simply vessels to be filled,” whereas

<sup>2</sup>Chen *et al.* argue that “the existence of a temporal order and the hierarchy of ages in the family suggest that the estimated ‘effects’ can be considered to deserve this name not only statistically, but also practically.” They go on to speak repeatedly of “sibling influence.” But it is easy to imagine how a sibling’s score might add to our power of prediction of the focal individual’s score without any “sibling influence” at all. Consider households in which both parents share attribute A and in which knowledge of whether an elder sibling is or is not also A alters the likelihood of A in the focal person. The sibling’s score may simply be an indicator of parental proselytizing effort.

in fact the modifications that can be expected to result from instruction and observational learning are critically dependent upon the recipients' present conceptual and behavioral repertoires.

Social influence, then, is sometimes rather a different process from that implied by the term "transmission," and the same may be said of the processes of cultural change. Julian Steward (1977) illustrated this point in a discussion of "modernization" processes:

Once the people become dependent upon the larger society, certain processes of internal economic change may be initiated, although these are in no sense part of the trade situation. That is, these consequences clearly do not result from diffusion as conventionally understood. Societies which may sell natural products through a trader or a market may become so dependent upon their commerce that they cease to produce their own foods and must purchase what they eat. The productive and consuming corporate unit is reduced to the nuclear family. Many Amazon tribes have fragmented into family units which trade wild rubber for manioc, which they formerly grew, and for other outside goods (p. 317).

Not only is the changed family structure not transmitted from without the society ("diffused") but such changes need not spread within the society by the present models' routes; the new practices apparently are simultaneously invented by many people reacting to the same adaptive problem.

Arguing that these models are unrealistic is not to say that they are not without value. The additive model used by Chen *et al.* provides a promising metric for cross-cultural studies. Pulliam has a theory that can be tested on a cross-cultural sample and refined. Empirical implications of Boyd and Richerson's modeling effort, however, are less clear. They apparently believe that altruistic disdain for "egoistic" self-interest is so prevalent and obvious as to need no documentation, and that this confronts sociobiology with a paradox. This proposition is very far from obvious, however, if sociobiological "egoism" is construed to mean the pursuit of abstract psychological goals and gratifications that have evolved because of their historical contribution to the promotion of inclusive fitness. From this point of view, Boyd and Richerson have used a highly artificial model to argue the theoretical adequacy of a particular escape from a nonexistent paradox. I agree wholeheartedly, however, with their concluding paragraph.

### **WHAT IS "GENETIC DETERMINATION"?**

Theorists who contrast biology and culture are in effect disinterring the oft-buried dichotomy of innate vs. acquired. Despite Durham's own caveats on the nature-nurture issue, he has fallen into an old trap, which consists largely of a confusion about what it means to call a trait "genetically determined."

The problem is clearest in Durham's "genetic mediation" model in which "genetic structures mediate in the process of cultural transmission (p. 314). These structures include things like "genetically determined peripheral sensory filters" and "built-in predispositions and limitations to learning" (p. 313). Mediation by such "genetic structures" allegedly "constrains actual phenotypes . . . to a small portion of the possible range" (p. 314). If we accept this concept, it is difficult to understand why Durham believes it does not apply everywhere. All sensation, all information processing, all action are constrained by the properties of organisms. Whence the dotted curve in Durham's Figure 3, "subject only to the mechanics of cultural transmission"? The conception of a finite, specifiable "possible range" of phenotypes, extrinsic to the constrained nature of the culture-bearing organism, is meaningless.

What then does it mean to say that a trait is or is not "genetically inherited" or "genetically determined"? Many would insist that this proposition can only mean that extant phenotypic variance in real populations is or is not attributable to genotypic variance. The argument has been advanced repeatedly that calling a trait such as eye color genetic is intelligible only with reference to differences between individuals; thus species-typical attributes (or the attributes of individuals considered in isolation) cannot meaningfully be called genetic (e.g., Dobzhansky, 1964; Lehrman, 1970). Instinctivists have repeatedly found this argument absurd (e.g., Ewer, 1971, p. 803), countering that genes are real entities, transmitted from parent to child, with real causal roles in development. But the causal developmental meaning of genetic determination is quite different from the variance-partitioning populational meaning (Lehrman, 1970; Daly, 1980a). By this second meaning, "neurophysiological processes" are no more "genetic structures" than are folk songs. Both are endpoints of developmental processes in which both genes and environment have played essential causal roles. Folk songs are considered nongenetic not because gene action has no part in their causation, but because variation in existing genotypes seems to be irrelevant to the explanation of their taking one form as opposed to another.

Durham's interest surely lies in sources of variance rather than in mechanisms of gene action. He is interested in how phenotypes are constrained to a subset of a larger universe of possibilities. When that universe is a real one, we may specify what proportion of the extant variance is "attributable" to genetic variation (although, as Cavalli-Sforza and Feldman, 1981, pp. 216-218, discuss, all we generally have are genotype-phenotype correlations, which need not imply that the genotypic variants are in any sense causal to the phenotypic variants). However, when the attribute is invariant and species-typical, the larger universe of "possible" traits is entirely hypothetical and open, so that the "constraints" on actual

phenotypes cannot be quantified or partitioned. This is precisely why Thorpe's (1963) and Lorenz's (1965) attempts to resurrect the nature-nurture dichotomy, based upon a separation of genetic and experimental "sources of information about the environment" were without impact upon ethologists. "Information" can only be quantified with respect to a closed universe of specified alternatives, and even those who found the sources-of-information dichotomy appealing could derive no testable implications from it.

### WHAT IS THE RELEVANCE OF DARWINISM?

The social sciences abound with research and theory concerning social influence and tuition ("cultural transmission") as well as innovation, diffusion, and historical process ("cultural evolution"). Those who construct mathematical models of these processes borrow eclectically, and I see no reason to expect them to find a special affinity with population geneticists. However, evolutionary biology has a contribution to social science more essential than its population genetical models, namely the evolutionary model of man.

This model proposes that all evolved traits of all organisms serve a single goal: the replication of the focal individual's genes (the promotion of inclusive fitness). Fitness-promotion entails both (1) accrual of those resources convertible to fitness—being an effective reproductive competitor—and (2) the conversion itself—being an effective dispenser of benefit to kin, especially but not exclusively offspring, and hence an effective "nepotist" (see e.g., Alexander, 1979; Flinn and Alexander, this volume). This theory of organismic, including human, nature can provide an heuristic metatheory for many areas of social science (Daly, 1980b).

Take the subject of "conformity," which Boyd and Richerson build into their model as majority advantage in trait transmission. Social psychologists have perpetrated a large body of research on the topic, investigating effects of the size and degree of unanimity of the majority, the prestige and expertise of the models, the personality characteristics of the subjects, and so forth. But treatments of conformity (e.g., Kiesler and Kiesler, 1969) are curiously devoid of reference to kinship or other individualized relationships between the parties. (No doubt this omission derives from the practice of doing research with captive groups of undergraduates.) People do not simply conform; they conform with certain people on certain matters. Can we determine how the various actors in a situation of social influence pursue their partially congruent and partially conflicting interests? Are the interests pursued intelligible within the logical framework of fitness-promotion strategies? An evolutionist, concerned with adaptive function, would pose some novel questions about conformity, perhaps beginning with the relevance of kinship.

Perhaps the largest body of empirical research on “cultural evolution” concerns the diffusion of innovations, especially technical improvements (e.g., Rogers and Shoemaker, 1971). Researchers in this field have empirically described communication networks, but seem not to have been concerned to explain their boundaries. They have identified “opinion leaders,” but seem not to have been concerned to understand those individual’s motives in assuming that role. The evolutionary model of man could contribute to explanation of networks and leadership behavior.

The papers in this symposium, by modeling social transmission within the family, have drawn attention to kinship, a central variable in the evolutionary model of man. However, they treat kinship as mere access: family members happen to be those who interact enough to transmit culture to one another (or who are variously similar and different in beliefs and practice, and hence variably in conflict: Pulliam). This is an impoverished view of kin relations. It ignores the fundamental commonality of interest of genetic relatives, as well as the conflicts of interest between those who transmit social influence and those who comply and resist. It ignores the life-historical strategy changes of individuals as dependents, breeders and postreproductives. Above all, it ignores the evolutionary analysis of familial interactions: the concepts of nepotism, parent-offspring conflict, sibling competition, and parental manipulation. This is really my major misgiving about these transmission models: that the treatment of people as passive recipients of social influence and cultural transmission is a misrepresentation of the strategic behavior of self-interested individuals.

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