Reconstructing the Evolution of the Human Mind

Eric Alden Smith
U of Washington
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Introduction

I approach this topic as an anthropologist who draws primarily on behavioral ecology for my theoretical and methodological inspiration. I study contemporary hunter-gatherer societies with an eye to understanding decision-making and ecological adaptation in small-scale social systems, which would include those of our human ancestors. However, I do not frame my research as an attempt to reconstruct remote (e.g., Pleistocene era) selective environments or social behavior. Indeed, I am somewhat skeptical of such an endeavor, for reasons I will make clear in this chapter. Nevertheless, if the endeavor is to be undertaken, I believe it is best to grapple directly with the complexities and limitations we face.

There are at least three distinct and complementary empirical sources of information on the evolution of the human mind. First, there is the prehistoric record, consisting of archaeological, paleontological, and paleoecological components. This source is the only one approximating a direct record of past selective environments and evolutionary processes, but it is inherently incomplete.

A second source of evidence is the study of mental faculties in the artificially constructed situations utilized by laboratory psychologists, neuroscientists, experimental economists, and the like. These *in vitro* studies allow for considerable control over variables that might confound the investigation at hand, but at some (usually unknown) cost in loss of realism or "ecological validity."

A third source of evidence is behavioral data gathered *in vivo*, through ethnographic or ethological study of functioning social systems, be they contemporary hunter-gatherers, urban dwellers, or chimpanzees. This produces the most fine-grained and contextually rich kinds of data, but to the extent that present environments differ importantly from ancestral ones in features relevant to evolved cognitive abilities, it also raises issues of ecological validity.

Each of these data sources or research foci, then, has strengths and corresponding weaknesses. Archaeologists and others who directly study records of the past necessarily engage in a great amount of inference in going from stones and bones to statements about cognition, evolution, and behavior. They literally cannot observe past human behavior or environments, but only their residues. Those who study contemporary humans (or their primate relatives), either *in vitro* or *in vivo*, can study behavior and its environmental context directly, but can only inferentially address the relation of these to ancestral humans and environments. Thus, each body of data and form of inquiry is seriously incomplete, and on some topics, we must either remain silent or engage in unreliable speculation. Obviously, an integrated approach that constrains inference from one evidentiary source with reference to findings from the others offers the best hope of advancing our understanding. At present, however, those who specialize in one of the three approaches just outlined rarely know much about the other two.

What are we studying?

I assume that the primary object of explanation is human behavior in its adaptive context. The study of "mind" is of interest only because minds (cognitive processes) interact with environmental information to produce behavior. Philosophers and novelists may be interested in mind in the sense of inner thoughts, but scientists qua evolutionists are interested in its observable products (behavior), because that is what produces effects in the world that translate into adaptive consequences and allow natural selection to act on heritable variation.

There seem to be at least three ideal types of mind-behavior processes that interest evolutionists. Type I consists of relatively unconscious cognitive processes that produce behavioral tendencies—for example, preference for individuals who have high levels of facial symmetry. Type II involves forms of

decision-making that are conscious and deliberative, involving either individual or social learning—for example, selection of which prey to pursue or how reliable an ally someone is. Type III includes patterns of behavior that appear to be primarily a product of cultural traditions, such as the forms of subsistence technology or which offspring will inherit the family estate. Type I is the favored domain of evolutionary psychologists, Type II of behavioral ecologists, and Type III is the special focus of cultural evolutionists.

Despite my heuristic examples, I doubt that much human behavior can be classified into one of these pure types. Rather, any given behavioral pattern is likely to be generated by a mixture of two or three of these processes. Take the prey choice example: Hunter-gatherers clearly engage in cost-benefit decision-making in the course of foraging trips, sometimes deciding to settle for low-return prey when higher-return prey prove scarce (Kaplan & Hill 1992); but these prey rankings are guided by unconscious algorithms for calibrating time expenditure and nutritional value, and they also depend on extensive cultural transmission of knowledge concerning prey behavior, foraging methods and technology, dietary traditions and taboos, and so on.

If my general argument against purity of process is correct, it follows that there is no single path to understanding the adaptive function of the human mind. Particular claims of evolutionary psychologists, behavioral ecologists, and cultural evolutionists may be erroneous or misguided, but we cannot afford to dismiss any framework categorically. They are complementary, and we need all three to understand human behavioral evolution (Smith, 2000).

Adaptive Novelty

The question of adaptive novelty is one that dominates many debates among evolutionary social scientists. According to one influential view, our cognitive systems evolved under a specific set of selective conditions—the "environment of evolutionary adaptedness" or EEA—and may routinely produce maladaptive output under modern conditions (see Symons, 1989, among others). Along with other critics (e.g., Foley, 1996; Irons, 1998; Strassmann & Dunbar, 1999), I find the EEA/adaptive lag thesis not so much wrong as ambiguous and oversimplified (Smith, 1998; Smith et al., 2001). Because I have addressed this and related issues at length in the cited articles, I will be very brief.

People regularly solve adaptive problems that their ancestors never had to solve—how to deal with neighbors who have nuclear weapons, how to apportion access rights to a declining fishery, whether to try and lose weight by joining a health club. Our ancestors also solved many novel adaptive problems—whether (and how) to hunt wooly mammoths, what to do about neighbors who possess novel weapons or trade goods, whether to abandon an area and travel to new lands. No doubt there are problems whose resolution lies outside our evolved capabilities and are also resistant to adaptive solution via cultural evolution. But the widely propounded view that we possess "stone-age minds" ill-suited to the novelties of the modern world, that we endlessly replay Pleistocene scripts in urban jungles regardless of their maladaptive consequences—that we are, in effect, prisoners of our evolved adaptations to past environments—strikes me as a fundamental misconstrual of human adaptation.

Our genus evolved in the context of radically fluctuating environments driven by the stochastic nature of Pleistocene climate. Our ancestors, even at the *Homo erectus* stage, managed to colonize a far broader range of habitats than any other primate species. By the time modern forms of *Homo sapiens* emerged, but long before the development of agriculture, we were able to flourish in every major terrestrial habitat on the planet, from Amazonia to Greenland (ice caps excluded, as they are still today). In order to accomplish this, our ancestors must have been able to radically refashion their diets, technologies, social organizations, mating systems, and cosmologies to adapt to each new environment. In fact, the ethnographic and archaeological evidence makes clear that they did so rapidly and repeatedly, creating pre-agricultural societies as diverse as Northwest Coast chiefdoms stratified into three distinct classes (including slaves), small bands of arctic hunters that moved every few weeks and lived in snowhouses built on sea ice, and wild grain-gathering sedentary villagers in the Zagros foothills.

Human behavioral diversity is immense, and utterly dwarfs that of other species. The spatiotemporal patterning of this variation, plus the ease with which people adopt the norms, beliefs, and practices of others, make it abundantly clear that very little of it is due to varying genetic endowments (with obvious

exceptions, such as adult lactose tolerance). Instead, this behavioral diversity is due to evolved capacities of the human psyche to generate novel responses to adaptive problems. This set of capacities has been termed "open programs" (Mayr, 1974) and "the cognitive niche" (Tooby & DeVore, 1987). Though recognizing the complex and specialized set of cognitive mechanisms that must be implicated in human behavioral ontogeny, like most social scientists I ascribe much of the generation of human behavioral variation to language and culture.

It is impossible to explain the evolutionary success of *Homo sapiens* without reference to culture (i.e., socially-acquired information). Indeed, cultural transmission is necessary to complete the human mind. Without cultural input, a human organism is not, and cannot become, a functioning and competent person. It follows that the search for a human nature in the form of a set of algorithms that produce human behavior without any culturally specific input is quixotic (Richerson & Boyd, 2005).

What methods?

If (as I have argued) there are several valid and complementary frameworks for studying the evolution of the human mind, then there are multiple useful methods for conducting such study. I am partial to the methods employed in behavioral ecology, suitably modified for the special attributes of human subjects (e.g., linguistic communication, ethical restraints). The overall research strategy in behavioral ecology can be described as hypothetico-deductive: Formal models are developed, they are manipulated to generate hypotheses, and these hypotheses are then subjected to empirical test. Such a schematic summary does little to convey the particulars of the research strategy, but it does highlight the use of formal theory to deductively generate testable hypotheses, an approach that is surprisingly rare in other branches of evolutionary social science (in which hypotheses are either generated in a more informal manner only loosely linked to theory, or formal theory is developed but rarely tested).

The models that are most useful for understanding adaptive variation in human behavior are ones that (a) specify a few key parameters that (b) vary across social or natural environments and (c) are likely to have important fitness effects. For example, a simple model of mating systems (the "polygyny threshold" model, Orians, 1969) specifies the degree to which members of one sex (usually males) can monopolize resources needed by members of the other sex as the key parameter that will determine the degree of polygyny versus monogamy. Although obviously insufficient, this model is a very useful starting point for more sophisticated explorations of adaptive variation in human mating systems (Borgerhoff Mulder, 1992; Voland, 1998). The focus of models such as these—using socioecological factors to explain behavioral variation—turns us away from any attempt to specify "the ancestral pattern" of human mating systems.

Formal models are useful for at least two reasons. First, they force us to make our assumptions explicit (and, thus, subject to critical scrutiny). Second, they generate predictions that we might not arrive at via intuition. Even though many of these predictions might be wrong, at least we know that they follow directly from our assumptions. This allows us to make more rapid progress in falsifying hypotheses that don't pan out, increasing the chances of generating hypotheses that will be supported. While the hypothetico-deductive method is not foolproof nor universally applicable, it has proven far more productive than unsystematic and informal methods of studying nature, including human nature. It is much needed in a field that has been too full of plausibility arguments and intuitions about selective pressures.

Although formal theory and deductive hypotheses are useful elements of a research strategy, careful attention to empirical evidence (including the archaeological and ethnographic record) is indispensable. My main caution would be to avoid the common view that knowledge of one or two societies (e.g., the !Kung San and the Yanomamo) is sufficient to generate empirical constraints on evolutionary hypotheses. As argued above, the record of preagricultural societies indicates a remarkable amount of variation, some ecologically correlated and some apparently not (Kelly, 1995). Systematic attention to this variation is necessary in order to avoid a blinkered view of the range of behavioral patterns and social systems generated by our ancestors. In this endeavor, various forms of the comparative method (Borgerhoff Mulder, 2001) will prove indispensable.

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