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DARWINIAN CONFLICT THEORY AND EVOLUTIONARY SOCIOLOGY

A Reply to Critics and Fellow Travelers

Stephen K. Sanderson

Let me begin by thanking Tamás Meleghy, Peter Meyer, and Heinz-Jürgen Niedenzu for organizing the conference at Innsbruck in June 2006, during which the original versions of these papers were presented. It is certainly gratifying, to say the least, to know that there are scholars out there who think highly enough of one's work to devote most of an international academic conference to it. And I thank all of the critics for their contributions. They have forced me to go back and reexamine many of my arguments and, in some cases, to rethink them and to clarify them. It has also been a real pleasure to discover that there are European scholars of a Darwinian persuasion whose work I was unaware of. But without further ado, let me turn to the critics' comments.[1]

MICHAEL SCHMID

[1] This reply to critics is a condensed version of a much longer reply. The longer version is posted on my website: http://www.stephenksanderson.com

Michael Schmid's critique of Darwinian conflict theory (DCT) is perhaps the most incisive that anyone has ever written. His brief summary of it is so good that I could hardly have improved on it myself. I wish I had had him as a sympathetic critic prior to the publication of *The Evolution of Human Sociality*, because he raises so many excellent questions that he certainly would have helped me refine DCT and make the book a better book. It is difficult to overstate how welcome it is to have someone who has an excellent and very nuanced grasp of what I have been trying to say. He realizes, for example, that DCT is much more subtle than a simple sociobiological reductionism; as he points out, in DCT biological needs and capacities are predispositions, not hard determinants, and these needs and capacities must be actualized by circumstances. What follows is basically a response to some of the many insightful questions and suggestions that Schmid raises.

It would be desirable if Sanderson would distinguish more clearly between "energizing" and "constraining" factors as causal mechanisms. Indeed. As Schmid notes, the so-called energizing factors in DCT are primarily the interests and needs of human organisms. These are what I have called the deep wellsprings of human action, and these wellsprings are grounded in the fundamental theoretical principle of sociobiology, the modified maximization principle. These are the things that individuals are struggling to do, some of them consciously and others more or less unconsciously. But what humans strive to do and what they are able to do are two different things. Thus, enter what Schmid is quite rightly calling the constraining factors. I have now taken to calling these, when taken collectively, the socioecological context of human action, by which I mean the entire range of external (especially ecomaterialist and polimaterialist) contingencies to which human action must adjust itself. As Schmid notes, in my scheme these do not really produce outcomes so much as steer, restrict, or guide them. For example, one of the fundamental wellsprings of human action for the male of the species is the desire to copulate with a large number of young and attractive females. In many societies at least some of the males, especially the higher-status and more resource-endowed males, are able to achieve this goal, or at least approximate it. Polygyny is found in 85 percent of the world's known societies, and in some of these societies a surprisingly large number of males are at some point in their lives polygynously married. But not all societies permit polygyny. A good many forbid it by law, and modern Western societies are among the best known of these. There are several competing theories of this so-called socially imposed monogamy, and it is not clear which of them is the correct one (or if any are). But one thing is clear: Socially imposed monogamy is the result of constraints on what it is natural for most males to do, which is why Laura Betzig can say with perfect accuracy that although not all men marry polygynously, in every society they seek to mate polygynously.

Now the question is, does one of these types of factors, energizing or constraining, have a privileged causal status? Schmid apparently thinks that the real causes in my theory are the energizing ones and that constraining factors are not genuine causal mechanisms. I'm not sure I agree. What we regard as a causal mechanism depends on the question. If our question is, Why is polygyny so common throughout the world? then surely the causal mechanism is the typical heterosexual male sexual inclination. But if we change the question and ask, Why do some societies have socially prescribed monogamy? then our causal mechanism is the constraints that male sexual inclinations are subject to. The starting point of analysis should always be the energizing factors, but they can do their work only within a socioecological context. Thus, the energizing factors cannot in the abstract be regarded as more genuine causes as the constraining factors, and vice versa. But I agree with Schmid that it is important to keep these separate, and in a later installment of DCT I will seek to do so more systematically.

Sanderson does not refer to the Darwinian notions of "variation," "selection," "retention," "descent," or "modification," and thus in what sense are his arguments truly evolutionary? The answer to this question depends on recognizing that there are at least three different ways in which the term "evolutionary" can be employed, and thus three different types of evolutionary "explanations." These can be distinguished approximately as follows:

- *Type 1 Evolutionary Explanations*: Explanations that rely on sociobiological principles concerning the evolution of human nature to ground an explanation of any social phenomenon. *Example*: *The Evolution of Human Sociality* and all of the work of the sociobiologists and evolutionary psychologists.
- *Type 2 Evolutionary Explanations*: Explanations relying on any type of causal mechanism whose *explanandum* is social evolution. *Examples*: Sanderson's *Social Transformations* (1995; 1999a), but also Talcott Parsons's *Societies: Evolutionary and Comparative Perspectives* (1966) and Robert Bellah's "Religious Evolution" (1964). Probably we should stop calling these evolutionary *explanations*. They are not, because a theory can only be appropriately categorized or labeled in terms of its *explanans*, not its *explanandum*. These "explanations" are various and sundry attempts (materialist, idealist, eclectic, etc.) to account for *social evolution in the sense of directional sequences of social change*.
- *Type 3 Evolutionary Explanations*: Explanations of social phenomena and changes occurring therein that transfer classical natural selectionist concepts (variation, selection, retention, etc.) to the realm of human social life. In this case, evolution is the *explanans* rather than the *explanandum*. *Examples*: the very well known work of Donald T. Campbell (1965, 1975), which would have launched this tradition except that it was preceded by the very early work of Albert Galloway Keller, *Societal Evolution* (1931); Philippe van Parijs's *Evolutionary Explanation in the Social Sciences* (1981); Boyd and Richerson's *Culture and the*

Evolutionary Process (1985); and W. G. Runciman's *A Treatise on Social Theory* (1989, 285-450).

I use the term *evolutionary* only in the first two senses and rarely in this third sense, so my explanations cannot be described as evolutionary in this last sense. I have been reluctant to use natural selectionist reasoning to explain trajectories of social evolution because these kinds of evolutionary explanations are largely explanations by analogy and thus do not reveal to us any necessary or sufficient causes. They provide only a rough indication of how a process might be characterized, and even then the characterization can be misleading because of several important disanalogies between biological and social evolution (cf. Sanderson 2007c, 287-89).

Sanderson's attempt to ground evolutionary theory in a conflict theory has nothing to do with his materialist historical analyses. I am not completely sure what is intended here, but let me simply say that a conflict theory is not necessarily one in which individuals are in open conflict with each other and/or in which some are dominating or exploiting others. A conflict theory is a theory of interests, or at least that is how I use the term. Randall Collins is the preeminent conflict theorist in modern sociology, and he says that individuals are extraordinarily conflict prone. Indeed they are, if by this is meant that they have competing interests (e.g., bourgeoisie and proletariat in the classical Marxian sense) or that they have the same interests but the resources available to satisfy them are insufficient for everyone to fully realize their interests. My materialist historical analyses, best represented in my *Social Transformations*, are at the same time conflict analyses. Conflict and materialist analyses are simply two sides of the same coin. Schmid says in a related vein that I have no systematic theory to explain historical development. Well, I do. Actually, I have a general theoretical strategy, evolutionary materialism, and then several more specific evolutionary materialist theories devoted to explaining major social transitions (these are the single causal explanations – of the transition to agriculture, of the rise of the state, of the transition to modern capitalism – of which Schmid speaks). I think that Schmid must be combing my work looking for a single general mechanism that is a Type 3 evolutionary explanation that will account for all long-term social developmental dynamics, but he will not find one there. And I don't have one because I don't think there can be one. (However, if Schmid wants to regard, as seems to be the case, the mechanism of conflict as a selection mechanism, I have no objection, because social life, including social change, is all about people struggling to satisfy their interests. But I do not find this sufficiently interesting or specific to be intellectually satisfying as a single Type 3 evolutionary explanation.)

Are the needs and dispositions with which Sanderson concerns himself only those that are genetically based? The short answer is no, although in truth I am more concerned with these kinds of needs and dispositions than with others. However, it is clearly essential to recognize that in the course of human affairs humans acquire new needs and dispositions. A simple example would be the "need for other-worldly salvation." I see no real evidence that religious behavior in most societies has this as a fundamental goal. It seems mainly to have arisen with the evolution of the monotheistic religions beginning some 2,500 years ago. And even then many people had no such need and many have no such need even today. Relatedly, I see no strong evidence that, as Weber seemed to imply, humans have a deep need for a sense of cosmic meaning. This seems to be a need of only some individuals at certain times and places.

Sanderson relies on a flawed method of testing hypotheses, which is the reliance on statistics and the amount of variance being explained. This means that his so-called explanations are necessarily false. This claim is quite surprising and I still do not understand it. I rely on a probabilistic model of causation, one that is widely accepted in the social sciences as the best we can do. Probablistic causal models are not false; they are simply incomplete. If I perform a multiple regression analysis and find that I have explained, say, 60 percent of the variance in my explanandum, then I am going to be extremely happy since that is far more than most pieces of sociological research explain. Schmid also believes that I regard high correlations as sufficient to establish causal relationships. No, certainly not. I learned in my first year of graduate school forty years ago that more is required to establish causal relationships than this. If A and B are highly correlated, A could be causing B, B could be causing A, or the relationship could be the spurious result of some third variable, C, which is itself highly correlated with both A and B. I am usually quite careful when inferring causation to have a reasonable basis for doing so – which doesn't mean my inferences are always right, of course.

NICO WILTERDINK

Wilterdink questions my fourfold division of societies into biostructure, ecostructure, structure, and superstructure. He suggests that this division is arbitrary because it does not correspond very closely to social reality, grouping together things that belong apart and setting apart things that belong together. Wilterdink questions in particular the coherence of my notion of ecostructure because it involves both the natural environment and economic relations that are in fact social relations. But I would call Wilterdink's attention to the origin of the prefix "eco." It is the Greek word *oikos*, which means household. What is a household but, among other things, a unit of economic production, and how does economic production take place but in a particular kind of natural environment with all its enabling resources and specific constraints. The "eco" refers to people engaged in one of the two most fundamental activities that they must carry out, namely the production of a living. That is hardly incoherent.

Wilterdink goes on to claim that my model implies a distinction between "social structure" and "culture," but this is problematic he says because structure (social relations) always implies culture (norms, symbols, etc.). This is an undemonstrated assumption that is part of the sociological heritage going back to

Durkheim and coming up through Parsons, Alexander, and other "culturalists." I find it not only unpersuasive, but utterly wrong and highly pernicious. Wilterdink also contends that I provide no clear argument linking biostructure to ecostructure, ecostructure to structure, and structure to superstructure. I must refer Wilterdink to my proposition 3.4: "The components of societies are related as they are because such causal dynamics flow from the deep wellsprings of human action. *The biostructure and the ecostructure have a logical causal priority because they concern vital human needs and interests relating to production and reproduction*." I have italicized the second sentence because it represents about as clear a statement of the linkages between the components as could possibly be made. But I suspect that Wilterdink does not really mean that my argument is not clear; I suspect he simply means that it is one that he does not agree with. Thus, we find the following (rather astonishing) statement from Wilterdink: "The biostructure, if it is defined as 'individuals themselves as biological organisms,' crucially depends on the social relations individuals have with one another (the social structure) as well as on the ways they have learned to cope with their environment (which is part of their culture): Human individuals 'as organisms' can only survive in groups that have patterned social relations and common social traits." Does Wilterdink really believe this?

To illustrate my conceptualization of the causal relations among my four components let me engage in a little *Gedankenexperiment*. Imagine that I have invented a machine that I point at one of my classes of, say, 50 students, that *decultures* them, i.e., removes all of their learned traditions and leaves them as naked as jaybirds, although with a rudimentary language of, say, 1,000 words relating to the most basic things humans need to talk about in daily life. Imagine also that I have a jet plane in which I place these students, which then transports them to a deserted island in the middle of the Pacific Ocean. Now, let's watch what happens. Two things would happen first, and rather quickly. Everyone would first busy themselves trying to figure out what kinds of plants and animals were available on the island that they could eat and, once they had figured that out, they would busy themselves devising tools and methods with which to capture and process them for eating. Once they had done these things – actually, in all probability in conjunction with them – they would (especially being typical 18-22-year-olds) begin to pair off and copulate, i.e., mate, and offspring would ultimately be produced. Enter stage left the modes of production and reproduction in accordance with biologically given predispositions, or, in my terms, biostructure and ecostructure. These patterned activities would then give rise to other concerns, those involving structure (providing leadership, maintaining order, establishing family and kin structures) and superstructure (crystallizing out certain norms and values on the basis of the highly adaptive behavior patterns already established). Thus a social system emerges eventually in full bloom with "culture" coming in at the end (although "material culture" would be there early on if the 50 individuals are to survive). But once a patterned social system had begun to form and new generations born, these new generations would be constrained in their behavior by the patterns created in the past. These are the environmental contingencies or socioecological context that are a crucial part of DCT, which Wilterdink (and some of the other critics in this volume) thinks I am ignoring or underplaying.

Now I submit that this is a realistic appraisal of how things would work themselves out if we could actually do the experiment "on the ground." And imagine that we could do it not just once, but 1,000 times: 1,000 groups of 50 decultured 18-22-year-olds placed in 1,000 different ecosystems. What we would see would be the same biological predispositions at work creating slightly different ecostructures adapting to slightly different ecosystems; and then slightly different structures and superstructures coming into play once the fundamental problems of human adaptation and survival had been met; and then finally the constraining effects of the sociocultural patterns so created. This illustrates in a very sketchy and rudimentary way the kind of argument my DCT is making. And, after all, something like it did happen way back when humans did not yet have this thing called "culture," which then evolved along parallel, convergent, and divergent lines to produce what we see today in the basic findings of sociology, ethnography, archaeology, and history.

And note also that in my little *Gedankenexperiment* there is nothing at all static about my societal components? They are dynamic dimensions of humankind and its creations, constantly adapting themselves to ever-different and ever-changing conditions. Wilterdink's claim that the concepts are static is an attribution entirely unsupported by any argument or evidence.

I see two reasons why Wilterdink has gone wrong in his critique of DCT. First, he is a victim of his sociological education. As Pierre van den Berghe has commented, most sociological training is actually an occupational hazard for the understanding of human social life. Wilterdink is still in thrall to an old-fashioned sociology that looks amazingly like the kind of sociology established by Talcott Parsons. Wilterdink invokes again and again "culture," "symbolic communication," "social norms," and so on in the manner laid out by Parsons and his epigones. What we need instead is a complete reinvention of sociology that radically reconceptualizes "culture." As George Homans (1984) has famously said, culture does not explain anything *but itself has to be explained*. Wilterdink is, in Homans's words, a "culture vulture." Note that in trying to explain the reduced fertility rates in modern societies, and the voluntary childlessness of some couples, Wilterdink merely falls back on a very vague notion of "humans as cultural animals who make choices in accordance with meanings." This tired old refrain is the last thing sociology needs in the early third millennium CE. Soon I hope to begin writing a book on the concept of culture, tentatively entitled *Culture Vultures: Darwinian and Pre-Darwinian Theories of Mind and Behavior*, in which I will suggest a complete reformulation of the culture concept in sociology and anthropology.

Wilterdink's other mistake is to implicitly reduce DCT to sociobiology and represent sociobiology as some sort of simplistic biological determinism. But DCT is a complex, multidimensional theory that gives a major role to a wide range of environmental contingencies in shaping human behavior and social patterns. Wilterdink totally ignores my highly stressed point about the *facultative* nature of nearly all human behavior: Humans have complex brains that allow them to assess their social environments and to respond to the contingencies they find with behaviors that are adaptive *within the context of those particular sets of contingencies*. That is why, for example, even though most societies have been polygynous, some have been monogamous and a few polyandrous. And it is why fertility levels in the modern socioecological context have been adjusted downward.

As the title of his chapter indicates, Wilterdink thinks my materialism is a *metaphysical* materialism in the sense that it is based on *a priori* assumptions rather than empirical evidence. Wilterdink is partially right: My materialism is metaphysically grounded. However, it is not metaphysically grounded in the pejorative Comtean sense intended by Wilterdink, but rather in the classical philosophical sense of metaphysics as *a concern to establish first principles*. This is something that very few sociologists and anthropologists, in their worshiping of the notion of emergence, seem to understand. One must always have first principles, or grounding principles beyond which it is not necessary or even possible to go in formulating explanations. One of our leading sociological theorists on the world scene today, Randall Collins, would do better if his version of conflict theory had some metaphysical grounding. Humans, Collins avers, are extraordinarily conflict-prone organisms. Indeed, he is quite right, but he *fails to explain why humans are conflict prone, taking this as an unexplained given*. Collins has given us an extremely useful conflict sociology, but it is a sociology with no first principles, and thus is terribly incomplete and often inaccurate. In real science, one does not try to be as *emergentist* as possible, but rather as *reductionist* as possible. This point Durkheim got completely backwards. And so do most sociologists, Nico Wilterdink among them.

ROSEMARY HOPCROFT

I found reading Hopcroft's paper especially pleasurable for several important reasons, not least of which is her contention that "as a set of macrosociological orienting statements, it [DCT] is probably the best sociology has to offer." It is seldom that one receives such praise, so I am going to bask in it for the short time that it is available. But Hopcroft's paper is also quite admirable because she is one of the few contemporary sociologists to have genuinely and fully embraced neo-Darwinian evolutionary theory, whether we call it sociobiology, evolutionary psychology, or Darwinian social science. Moreover, she knows what she is talking about. There are no mere caricatures or distortions here, but a real understanding of the key theoretical principles and how they can be put to use to understand many features of human social life, social and gender stratification and family relationships in particular. And Hopcroft has done something else well worth noting: She has taken a number of pieces of empirical sociological research that were never written from a Darwinian standpoint (or necessarily from any systematic theoretical standpoint at all) and shown how they are highly consistent with Darwinian principles: the importance of family and kin networks to personal physical and psychological well-being ("marriage is good for you"), the adverse psychological consequences of divorce for children, the greater parental solicitude of mothers compared to fathers because it is the human female that produces the rarer and more precious gamete, the greater likelihood of fathers compared to mothers disengaging from their children upon divorce, the different characteristics that men and women are looking for in their mates, the sexual double standard (in all likelihood a true cultural universal), the universal human drive for status and resources that results in sharp economic inequalities when socioecological conditions permit, and the virtual obsession that parents have with promoting the reproductive and productive careers of their children.

Hopcroft's list is long, but I am sure she would agree that were more space available to her it could have been much longer. There is also sociological research in other topical areas that is highly consistent with DCT. Hopcroft mentions Weeden's (2002) article on the role of professional monopolies in producing higher incomes for professionals, and I quite agree. Indeed, the whole neo-Weberian tradition that emphasizes *social closure* is highly compatible with DCT, and to this I would even add theories like Bonacich's split labor market theory of racial and ethnic antagonism, which was driven by a kind of neo-Marxism.

Hopcroft says that my *Evolution of Human Sociality* is incomplete. Indeed, she is completely right, but then what book is not? Any book that covered its entire topic without any loose ends sticking out and nothing more of importance to be said would be either focused on an awfully trivial topic, or else written by someone of genius far exceeding the greatest scholarly geniuses the world has seen so far. So of course my book is incomplete. But Hopcroft has a solution: I should write another book in which I survey sociological knowledge of contemporary industrial societies and integrate this knowledge into my paradigm. Not only do I like this suggestion, but I fully intend to follow it. A few years from now I hope to begin writing a book tentatively entitled *Foundations of Darwinian Sociology: Steps to the Dream of a Final Theory*. This second book would do what Hopcroft is asking for, but it would go further: It would refine and expand DCT, update the discussions of existing topics, and apply the updated theory to phenomena not discussed in *The Evolution of Human Sociality* – religion, ethnicity, conformity and deviance, organizations, law, art, music, literature, science, and even the microsociology of the self, norms, and social roles. In this book I shall attempt the task of actually reinventing sociology. What unbelievable hubris! Can I be serious? Indeed, I am completely serious. Can it be done? Well, there can of course be no such thing as a truly final theory, but we can move toward one.

CHRISTOPH ANTWEILER

Antweiler's chapter is short but pithy and he raises a lot of useful points for discussion. I very much appreciate the fact that he notes that my DCT is a genuine explanatory theory. Indeed, that is precisely what it is intended to be. I follow the sociologist George Homans, who said that that is what theory is: Theory is explanation. Too often in sociology and the other social sciences we get concepts, typologies, and other conceptual schemes passed off as theories, but the identifying mark of a theory in science is that it explains (or offers an explanation).

Antweiler has a special interest in human universals, a topic that takes up much of his discussion. There is some inconsistency here. He starts by defining a human universal as a trait that is found in all societies, but then retreats from this by distinguishing between two kinds of universals, *diachronic* and *newly emergent* universals. I take it that so-called diachronic universals are "true" or "genuine" universals, that is, traits found in all known societies throughout prehistory and history. Newly emergent universals are traits, such as all-purpose money, that are universal to only one particular type of society, such as modern industrial society. I would caution against using the term universal in this way because it significantly weakens it. A universal is a trait that must be found in all known societies. This is a critical point, because if something is found in all places and times that is a fascinating fact, that cries out for explanation. I think this explanation must be biological, or at least make reference to a biological predisposition. Antweiler, however, is not so sure, and in fact is critical of me for assuming that universals must always have biological explanations. But my response would be that, given the striking variations in human societies, the discovery of characteristics that every single society possesses strongly suggests to me the likelihood of a biological predisposition to create that characteristic. At the very least this provides us with a warrant to study the trait with this idea in mind.

Antweiler complains that my work almost completely ignores social institutions. This is not quite right. In *Social Transformations*, a book that Antweiler includes within the purview of his essay, I discuss several institutions, in particular economics and politics, but also education and science, and in *The Evolution of Human Sociality* there are ample discussions of economics, politics, and family and kinship. Antweiler is right to point out, though, that religion is completely neglected in these works, as are media. But I will be rectifying this situation in the years to come, as I am now engaged in a major project on the evolution of religion and eventually hope to take up the evolution of science, art, music, and literature.

KHALED HAKAMI

Hakami's chapter seems more like an academic version of a drive-by shooting than a closely reasoned critique. Mostly it is a series of blunders which show that Hakami is both an uninformed and a misinformed critic. I would summarize these blunders approximately as follows.

Blunder Number 1: Hakami thinks that the theoretical strategy I have called *evolutionary materialism* is a synthesis of two opposing strategies, cultural materialism and sociobiology. Certainly not. Evolutionary materialism is the strategy I developed over a dozen years ago to study long-term social evolution. It is not an opposing strategy to cultural materialism at all, but merely a modification of it along certain lines. The synthesis I created out of what are usually thought of as opposing strategies, cultural materialism and sociobiology, is *Darwinian conflict theory*, itself developed several years after evolutionary materialism and intended as a more general strategy that takes the principles of evolutionary materialism to a deeper level and grounds proximate explanations in ultimate explanations. Strangely, Hakami does not even use the name Darwinian conflict theory in his critique, and therefore does not really address it. He basically reduces me to "just one of those sociobiologists" and as a result distorts and grossly oversimplifies what is a much more nuanced and complex theoretical approach.

Blunder Number 2: Hakami uses the arguments of C. R. Hallpike to demolish the notion that Darwinian natural selectionist thinking can be applied directly to social evolution and criticizes me for failing to cite Hallpike. But, having done this, he then turns right around and *endorses* natural selectionist approaches to social evolution. This time he appeals to the authority of Robert Carneiro, quoting him to the effect that the concept of natural selection "is just as valid, fruitful, and essential in explaining cultural evolution as organic evolution" (Carneiro 1992, 117). Although this is bad enough, things get even worse. Hakami has quoted Carneiro completely out of context, because the article of Carneiro's that Hakami refers to *is mainly a critique of natural selectionist theories of social evolution*! We could hardly fault anyone for concluding that Hakami has made a total mess of this matter.

Blunder Number 3: Hakami contends that methodological individualism is wrong because no single Kwakiutl developed the potlatch, no single Kachin created a complex alliance system, and so on. Of course not, but such a preposterous claim has never been made or would ever be made by a methodological individualist.

Blunder Number 4 is Hakami's argument that hunter-gatherers are really gatherer-hunters. The most comprehensive survey of the proportions of meat and plant matter in the diets of foragers has been carried out by Carol Ember (1978) in an article of which Hakami appears ignorant. She shows that, although gathering may be more important than hunting in most *African* hunter-gatherers, *in the world as a whole* meat makes up a majority of the diet among foragers (cf. Sanderson 2001a, 260).

Blunder Number 5 is the claim that optimal foraging theory is based mainly on the study of nonhuman animals. Although optimal foraging theory is derived from evolutionary ecology and was originally based on studies of animal foraging patterns, it started to be applied to human foraging nearly thirty years ago and there is an extensive literature involving human applications, most of which supports the theory (cf. Sanderson 2001a, 260-64).

Blunder Number 6: Hakami contends that, although I was not a staunch sociobiologist at the time that I formulated evolutionary materialism, things have not changed. I thought the problem was that *things have changed*, and for the worse because now *I am* a staunch sociobiologist. Actually, this is really two blunders, because I have never been a staunch sociobiologist and am not now. I am a Darwinian conflict theorist, which means that I make use of key sociobiological principles but go considerably beyond them.

In addition to these outright blunders, Hakami makes several other very dubious claims. For example, he argues that the members of hunter-gatherer societies are not more altruistic toward their offspring than towards other members of their bands. Not only is this untrue, but *in every known type of human society* humans are more altruistic toward kin than toward nonkin, and toward close kin, especially offspring, than more distant kin. In addition, he attacks Marvin Harris by claiming that he was not a true evolutionist. He was a pseudoevolutionist who strung together a bunch of synchronic studies, calling the result a theory of social evolution, and he wrote only one book on social evolution, *Cannibals and Kings*. This is a bit like saying that Darwin wasn't a real evolutionist because he made numerous observations in South America and the Galapagos Islands on present-day species, and, besides, he wrote only one important book on organic evolution, *On the Origin of Species*. Finally, Hakami avers that Harris and I are sitting in the same boat, which, if we put the biological baggage aside, is just cultural materialism. Actually, Marvin's boat was called the *Maddy Sue*, and he liked to sail it off the coast of Maine, where he spent his summers. I knew Marvin only slightly, and certainly not well enough to be invited to sit beside him in his boat. Therefore, I have had to build my own boat, the name of which at least the other contributors to this volume know.

PETER MEYER

Meyer seems to be saying that I don't give enough emphasis to human cooperation as springing from a genuine feeling of sympathy, citing Adam Smith's famous discussion of natural human sympathy in his *The Theory of Moral Sentiments*. Even though I do not discuss Smith in *The Evolution of Human Sociality*, I have no difficulty agreeing that the emotion of sympathy is an innate human emotion, and that it is often extended beyond close kin or even distant kin; humans often feel sympathy for unrelated individuals, including those they have never seen before. Let me try to clarify my position.

A careless mistake I made in formulating the theoretical propositions of DCT was to have used the word "selfish" when what I meant was "self-interested." This is an important distinction. I behave selfishly if, say, I have a jar of black currant jam, my very favorite jam, which I dearly love, and I refuse to share it with anyone. I either keep it out of sight or refuse to allow anyone to have some of it upon request. All of us act selfishly from time to time, and some of us act selfishly most of the time. But all of us all of the time act in accordance with our self-interests. I have felt sympathy on many occasions for a number of individuals, but in particular for my children. When they were still very young I was deeply distressed when I saw that they were suffering in some way. Their suffering was indeed my own suffering. My own mother many years ago explained that she had these same feelings; I understood her point at the time but did not *feel* the force of it until I became a parent myself. So of course sympathy is a primordial human emotion that drives much behavior, and of course people cooperate with others because they want to and it gives them pleasure. Cooperation is not always the result of individuals making careful calculations of the costs and benefits of cooperation and deciding accordingly. Natural selection has built into us fundamental emotions of unselfish behavior, but these emotions originally evolved because they benefited the individuals who felt them, at least over the long run.

Meyer also suggests that I am too much of a materialist, a criticism of me made many times by many scholars, including Nico Wilterdink, as we just saw, and Heinz-Jürgen Niedenzu, as we shall soon see. But I am completely unrepentant. Meyer suggests in particular that "mentalistic approaches" are fully compatible with evolutionary theory and that I should be more open to them. However, he does not explain what he means by "mentalistic." This can mean two very distinct things. First, it can refer to those things that Marx and the Marxian materialists (and Marvin Harris as well) place in the ideological superstructure: beliefs, values, philosophies, art, literature, religion, and so on. Or it can be used very differently to refer to the basic mental architecture of the brain. I believe that it is this second meaning that Meyer intends. If so, I fully agree with him, but I would not use the term "mentalistic" in this case because the brain is about as materialistic an entity as we can find. Just as the heart is a material object or structure, so is the brain; both are parts of the body. Brain functioning involves billions of neurons and their synapses and the transmission of neurochemical messages across those synapses. So if this is the meaning of mental, than I am all for mental, and of course this kind of mental is perfectly compatible with evolutionary theory. But this mental is material, and it evolved by natural selection because it benefited the organisms that contained it.

HEINZ-JÜRGEN NIEDENZU

Niedenzu alleges that the central problem with my DCT is that it does not give any systematic role to human constructivity and creativity; correlatively, it makes ecomaterialist and polimaterialist explanations dependent on biomaterialist explanations and, as such, derivative. Let me address each of these concerns.

Niedenzu acknowledges that I do not deny the reality of human constructivity, but he apparently feels that I underplay it. As I read and reread his chapter, I came to the conclusion that his central concern is to make human constructivity *autonomous* from human biology. This is an argument that has been made many times by social scientists ever since the beginnings of social science, and most humanists would make it even more strongly, denying the biological organism any role at all. I do not think that it works, however, and I doubt that there can be any such thing as *pure constructivity* or *pure creativity* entirely free from biological constraints, either in the case of individuals taken alone or in the case of groups of individuals taken as collectivities. In this regard I suppose I am a firm determinist.

Several major realms of human creativity are literature, art, and religion. Because space is short, let me limit myself to the last of these. At the moment I am working on the long-term evolution of religion, with a special focus on the evolution of the major world religions during the Axial Age (the last six centuries BCE). All of the new religions that developed during this time were very different in several crucial ways from the polytheistic state religions that preceded them, and yet they were very similar to each other, especially in their conception of a *transcendent* reality. It is remarkable that throughout much of the Old World very similar religions arose in a very concentrated period of time, even though it is not likely that these religions had much direct influence on each other (Bentley 1993). Where did the new religious ideas come from? As Max Weber has argued, such ideas spring from religious virtuosi, individuals who have special religious skills and insights. I accept this, but would add that such individuals are very likely persons with a very unusual brain organization combined with special social experiences. And then of course the ideas have to be accepted – have to catch on among the masses. This suggests another kind of determinism, viz., the right kinds of socioecological conditions that would make such ideas attractive. Currently I am trying to identify what these conditions most likely would have been (Sanderson 2007d, 2007e).

So I think it is doubtful that human constructivity and creativity are ever autonomous. They only appear autonomous because we have not yet been able to identify the conditions under which the various forms of constructivity and creativity emerge, both in the special brain wiring of uniquely creative individuals and in the socioecological conditions that make entire groups or societies receptive to creative acts. This of course makes me appear to be a very hard-headed determinist, but I think such a view will be vindicated in the end, although how long it will be before we reach that end is very difficult to say. It also makes me a resolute antidualist, since I would contend that ideas can only reside in the brain, whether one brain, several, or many. (If they are not in the brain and derivative from it, where on earth could they possibly be?) And why do I myself take such stances? Are they purely creative constructions? This is doubtful. Ever since I was a small boy I always thought in very scientific, materialist ways and had little interest in those things that preoccupy the humanists. My brain is probably wired such that the left hemisphere is highly dominant over the right. Science is quite easy for me and extremely interesting to me, whereas art has always been mostly a complete mystery.

Let me conclude by addressing Niedenzu's point that DCT makes ecomaterialist and polimaterialist explanations derivative. This is true only in the most general sense that such explanations must refer back in some way to the human organism and its nature. Actually, in most of my work I have given ecomaterialist and polimaterialist explanations pride of place. In the first edition of my book *Social Transformations* (Sanderson 1995), which presents a general theory of social evolution, such explanations occupy the entire theoretical space. In the second edition of the same book (Sanderson 1999a), I added an Afterword in which I discussed the role of biological *constraints* on social evolution. Niedenzu is surely right when he says that social evolution cannot be reduced to and is in many ways quite different from biological evolution. I couldn't agree more (cf. Sanderson 2007c, 287-90). In 2005 I published two books, *World Societies: The Evolution of Human Social Life* (coauthored with Arthur Alderson) and *Revolutions: A Worldwide Introduction to Political and Social Change*. Neither of these books makes any reference whatsoever to humans as biological organisms. The explanations found in *World Societies* are mostly ecomateralist in the broadest sense (invoking demographic, ecological, economic, and technological factors), and the explanations offered in *Revolutions* involve combinations of economic and political factors.

In the end, whether our explanations are to be biomaterialist, ecomaterialist, or polimaterialist (or some combination of these) depends mostly on what it is we are trying to explain and on how fine-grained a level. Although all ecomaterialist and polimaterialist explanations have to be grounded in human biology, such explanations are often quite fundamental and much more than merely derivative.

TAMÁS MELEGHY

Meleghy wishes to advance Lévi-Strauss's structuralist theoretical agenda by adding to it the principle of inclusive fitness. I quite agree with Meleghy's argument that in matrilineal societies the greater investment of men in their sisters' offspring than in their own offspring makes sense in terms of higher levels of paternity uncertainty. In preliminary research I did a few years ago, and as yet unpublished, I used a cross-cultural sample of 60 preindustrial societies and cross-tabulated a measure of paternity certainty with a society's mode of descent. I found that 92 percent of matrilineal societies had low paternity certainty compared to 20 percent of bilateral societies and just 18 percent of patrilineal societies. I conclude that matrilineal descent should basically be conceptualized as a strategy of investment by men in their sisters' offspring whereas patrilineal descent should be conceptualized as a strategy of men's investment in their wives' offspring. And matrilineal descent is a lot less common than patrilineal descent because men have fairly high levels of paternity confidence in most societies.

To me this is a satisfying explanation, but Meleghy doesn't want to stop there. He wants to tack this onto Lévi-Strauss's basic explanation of exogamy rules, which he takes to be based on a fundamental law of reciprocal exchange. However, there are at least three problems with this recommendation. First, as Marvin Harris (1979) has pointed out, the empirical evidence strongly contradicts this so-called law. As he says, reciprocal exchange is "the basis of marriage systems only in egalitarian societies. To the degree that a society is stratified into politically and economically superordinate and subordinate groups, marriage systems function to prevent reciprocal exchange" (1979, 173). Exogamy rules have much more to do with whom one may *not* marry than with some sort of reciprocal relationship between groups. Of 752 societies in Murdock's *Ethnographic Atlas* known to practice exogamy, only 188 (or 25%) have any of the forms of preferential cross-cousin marriage discussed by Lévi-Strauss, and reciprocal exchange of marriage partners is not found in all of these.

Second, the paternity confidence explanation is itself probably a sufficient explanation of matrilineal descent, and therefore I recommend that we use Occam's Razor and discard all unnecessary concepts or hypotheses. Third, trying to mix the idea of reciprocal exchange with the idea of inclusive fitness produces, at least in this particular case, theoretical incoherence. Meleghy contends that reciprocal exchange is for Lévi-Strauss a fundamental principle of human thought that is genetically fixed. There does seem to be a universal human sense of reciprocity that is part of the basic human sense of fairness, but this is a notion that applies to the moral sense rather than to marriage practices. Meleghy imagines that Lévi-Strauss's "law of reciprocal exchange" is essentially a kind of biomaterialist principle that fits well within DCT. But what Lévi-Strauss is talking about is utterly alien to DCT and unsynthesizable with it.

JOHAN VAN DER DENNEN

The focus of van der Dennen's chapter is my Darwinian conflict analysis of war. Let me say at the outset that war is one of the phenomena discussed in *The Evolution of Human Sociality* that I have studied the least and concerning which I am least knowledgeable. I have much more to learn, and I hope to accomplish that in the years to come. However, I suspect that any changes in my viewpoint with be matters of detail and nuance rather than any fundamental change of perspective.

Van der Dennen says that I do not distinguish between genocidal wars, instrumental (coercive) wars, and ritualized wars, and that I also fail to distinguish between ambush-like or raiding warfare and disciplined, phalanx-like warfare. He is right, I do not, at least in those terms. I doubt that there is any such thing as ritualized war, despite the claims of some anthropologists. War is much too serious a business for that. I certainly recognize genocidal wars, but were I to discuss them and try to explain them I would do so under the heading of ethnic conflict. I am not sure what van der Dennen means by instrumental or coercive wars. As for ambush-like war versus combat-type warfare, I certainly recognize the distinction even if I do not use that specific language. My discussion of tribal warfare is basically a discussion of ambush-like war. Combat-type warfare is characteristic of chiefdoms and, especially, states, and I offer a very different explanation for it. This type of warfare is devoted primarily to political conquest of other societies, the main purpose of which is economic gain – of land, resources, tribute, slaves and other types of coerced workers, and so on. In this connection van der Dennen contends that I have fallen victim to the "great war figures hoax." It may well be true that numerous scholars have overestimated the number of war deaths in agrarian states and empires, but there can be no serious doubt that war is perhaps the single most important activity pursued by the rulers of these kinds of political systems (cf. Kautsky 1982; Snooks 1996, 1998).

What then of van der Dennen's own preferred explanations of war in all of its varieties? He is not easy to pin down. He seems to object to all types of materialist explanations (regarding them as "vulgar") and even criticizes Brian Ferguson for claiming that the desire for security or safety is a material desire. But what on earth could be more material than one's own physical safety? Is one's living body not a material object? Van der Dennen objects to single-factor explanations, regarding them as too simplistic. I read him as a type of eclectic who wishes to entertain a wide range of explanations.

However, in the concluding section of his paper he claims that the most important proximate cause of war is fear. People fight with neighboring bands and villages primarily because they are afraid of them and wish to protect themselves against their own extermination. The problem with this explanation is that it begs a crucial question: Why should people fear being exterminated by their neighbors? Is it because *they have good reason to fear the intentions of their neighbors* and, if so, is this because *their neighbors have acquired a reputation for belligerence*? We are thus right back where we started from: The fear of attack by neighbors does not explain anything so much as it must itself be explained. Societies at all levels of political complexity often fear their neighbors because they have something to fear.

C. R. HALLPIKE

I read Hallpike's chapter with great interest and enjoyed it in spite of the fact that he is not the least bit sympathetic to any of my ideas – or at least to what he presumes them to be. Hallpike is an antimaterialist and anti-Darwinian. He is supremely antagonistic to applying Darwinian thinking to social life, either in terms of sociobiological principles or as "variation-and-selective-retention" theories of social evolution. Hallpike is, nonetheless, an evolutionist, and he has written a whole book on the subject entitled *The Principles of Social Evolution* (1986). But he turns out to embrace a type of evolutionism that is of the cultural idealist variety. Unlike most social evolutionists, Hallpike rejects the concept of adaptation as useful, especially when applied to preliterate bands and tribes. Such societies, he contends, are under little or no pressure to produce social arrangements that are highly adaptive; just about anything is workable for people living at this "cognitively undemanding" level, and the "survival of the mediocre" rather than the survival of the fittest is the order of the day. It is societies at more advanced levels of sociopolitical organization that are under much greater pressure to produce adaptive solutions to the problems they face. I rather think that the evidence is overwhelmingly against Hallpike's notion that competitive pressures are mild in bands and tribes. Indeed, the evidence, much of it reviewed in *The Evolution of Human Sociality*, seems to point in exactly the opposite direction: Competitive pressures in terms of the struggle for survival and the competition for mates are actually *more intense* in these types of societies than in others.

So it is not difficult to see why Hallpike would strenuously object to both my evolutionary materialism and my DCT. In his contribution to this volume, he uses the example of the development of science, especially modern science, to refute me. I have written very little on science (cf. Sanderson 1988, 410-31, 1999a, 317-32), and, truth be told, this is the most difficult of all social phenomena to explain from a biomaterialist or an ecomaterialist perspective. The human brain is not exactly "wired for science" in the strict sense of devising hypotheses and submitting them to demanding empirical tests. Only a small number of people have either the necessary brainpower or the capacity for objective and dispassionate reasoning that science requires, and there are several biases of the human mind that seem to be a real hindrance to scientific understanding (e.g., the strong tendencies toward teleological and essentialist thinking, the tendency to attribute agency not only to individuals but also to other animals and even to inanimate objects or forces).

What then of ecomaterialist explanations of science? Hallpike contends that our biological needs do not exert a constant pressure for invention. He is indeed correct so long as we note the qualification "constant." I have never asserted otherwise. Throughout history and prehistory inventions have come in fits and starts, and there are long periods where little is happening. And of course I recognize that there was a tendency for technological invention to stagnate in the ancient world, and for the very reason that Hallpike gives: the dominance of parasitic aristocratic classes that valued brain work and devalued practical knowledge and that did not stand to benefit economically from technological advance. I also agree with Hallpike concerning many of the preconditions for the development of modern science and industrial technology, in particular the shift from a feudal to a capitalist economic system in which the dominant capitalist class, unlike ancient aristocratic landowners, could benefit enormously from the technological applications modern science made possible. These conditions came together in the seventeenth century, which is really the takeoff point for the development of modern science. And Hallpike is surely right to suggest that it took the buildup of many historical antecedents to provide a foundation for this takeoff point.

Hallpike seems to think that science is largely a matter of smart people thinking smart thoughts, and that both scientific advance and its corollary, technological advance, have little to do with biological or economic needs. But Hallpike's historical vision is remarkably compressed: He focuses almost exclusively on the last few centuries, which constitute a very unusual period. He either ignores earlier technological advances or sees them as having little or no practical significance. He contends that the development of metallurgy, for example, was stimulated by ornamental rather than practical needs. Perhaps, but the new metals were quickly put to use in the development of more sophisticated tools and weapons, first of bronze and then of iron. Hallpike makes no reference at all to the invention of the plow, which was first a wooden plow and then later came to be made of metals. This was an enormously practical invention, and was indeed developed for "materialistic" reasons (Pryor 1985), mainly the need for greater economic productivity to feed expanding populations. Nor does Hallpike make any reference to the great transition from hunting and gathering to early agriculture beginning about 10,000-11,000 years ago and occurring largely independently all over the world. Archaeologists used to think of this as the result of a smart person thinking a smart thing, whose brilliant idea then spread, but this theory has now been almost totally abandoned in favor of theories emphasizing the role of population pressure and ecological change. The transition to agriculture was a "materialistic" process if ever there were one.

Hallpike is surely right when he suggests that "nature does not merely impose itself on our senses," but must be interrogated. This volume alone is proof of that, since there is wide disagreement among many of the contributors on how to interpret a wide range of empirical data, and indeed on whether these data are "facts." But I have never suggested that scientists merely look at nature and then know how to explain it. When Hallpike quotes me as saying that "the empirical world acts as a powerful constraint on scientific beliefs" he fails to mention the context of that quotation. In making that statement I was arguing against the postmodernists, who claim that science is a largely social or political process in which empirical evidence plays no important role.

Hallpike concludes his chapter by saying that the origins of such modern inventions as steam power and electricity entirely contradict any ecomaterialist explanation. But they do not, since such inventions occurred within a new economic context, that of capitalism, and in fact in the second half of the seventeenth century more than half of the scientific investigations undertaken by members of the British Royal Society were directly or indirectly stimulated by economic concerns (Merton 1957). The ups and downs of scientific advance actually closely track the ups and downs of commercialism. Ancient Greek science arose within one of the world's first highly commercialized civilizations, and the bursts of scientific activity in the Arab world and in China between approximately the eighth and fourteenth centuries seem to have been closely tied to commercial expansion. Chinese science, in fact, was much less theoretical than either Arabic science or later Western science, having preponderantly practical and technological aims (Huff 1993).

It is important in this connection to distinguish two different dimensions of scientific activity and the motivations that underlie them. Many scientists themselves are often purely intellectual in their concerns; they are interested only in how the world works. But science requires patronage, else it cannot proceed very far, and patronage requires wealth and a belief on the part of the patrons that scientific findings will have important technological spinoffs. When I originally classified science as part of the superstructure, I was thinking only of this first dimension of science: the concepts and theories. But the rest of science might well be considered part of the ecostructure, since that part is technological knowledge. This is another emendation in DCT to be made in a future installment.

J. P. ROOS

Roos's extremely important article shows conclusively what I myself have been learning over the past couple of years, namely, that Edward Westermarck was a major classical sociologist whose thinking was both deep and broad and whose ideas are turning out to be much more empirically accurate than those of his leading rival, Émile Durkheim. I have known of Westermarck for years, but until recently I thought he was only important for a theory of incest avoidance, one that is turning out to be extremely well supported by numerous lines of research evidence. But Westermarck did more than that – much more! As Roos points out, he had a well-developed theory of moral emotions rooted in Darwinian principles, and he contributed many insights regarding a wide range of marriage and family patterns. And methodologically Westermarck was vastly superior to Durkheim in that he drew on a far greater range of historical and ethnographic data.

The only reservation I have about Roos's chapter is his strong claim that on every important issue where Westermarck and Durkheim disagreed Westermarck got it right and Durkheim wrong. I suspect that this is largely true, but Roos does not present any real evidence to document his claim. With respect to incest avoidance, there is now a great deal of evidence to support Westermarck, whereas Durkheim's argument is completely speculative and, in fact, highly implausible. And concerning morality, there is now a rapidly growing literature on the evolution of the moral sense that is highly consistent with Westermarck. But these are only two issues. What of all of the other issues that both thinkers investigated and theorized about? Even though I suspect that Westermarck will have gotten the better of Durkheim on most of these, all the evidence is not yet in. Durkheim didn't get everything wrong. His analysis of suicide was a meticulous piece of first-rate sociological research and there is little doubt that suicide rates and levels of social cohesion are related. And Westermarck didn't get everything right. For example, he was highly critical of Darwin on sexual selection, and yet this is turning out to be one of Darwin's most important ideas. So the jury is still out concerning whether Westermarck got the better of Durkheim on *all* major issues.

ANNA ROTKIRCH

Anna Rotkirch's chapter on baby fever is fascinating and brimming with insights. Prior to reading her paper I had never heard of the term "baby fever," but I immediately recognized the phenomenon she is discussing because I have observed a number of instances of it among friends and acquaintances. And consider the following much more public examples. Recently a book entitled *Baby Love* was written by a woman by the name of Rebecca Walker. The author, a career professional, explains that in her 20s and early 30s she had a strong desire to have a child but put this urge aside in order to concentrate on her career. Finally, in her mid-30s, she gave in to her desire, became pregnant, and gave birth. More recently, one of the actresses on the popular American television program *Desperate Housewives* gave birth to her first child in her mid-40s and described the experience as the greatest experience of her life. And a year or two ago the long-running American news program *60 Minutes* featured a segment about four women who were apparently extremely talented and educated at some of the United States's finest universities only to abandon their professional careers completely in order to stay home full-time with their children. All four women told the interviewer, Lesley Stahl, that they fully intended to remain home with their children full-time and not resume their careers.

A central concern of Rotkirch's paper is why people have children. Why do they? For a long time I endorsed the view of such scholars as Marvin Harris

(1989; Harris and Ross 1987) and Ester Boserup that people have them largely for their economic benefits. However, in research I did a number of years ago, I discovered that the economic value of children's labor seemed to make little if any difference in why people in some societies have many and in other societies have few (Sanderson and Dubrow 2000; Sanderson 2001d). Moreover, if the economic utility hypothesis were true people in modern affluent societies should not have any since children are extremely costly in such societies and provide virtually no economic benefits at all to the vast majority of parents. And yet people continue to have children, and I suspect the reason is Darwinian: People have children because that is what they have been designed by nature to do. Of course, how many they have is very sensitive to environmental cues, especially the survival rate of infants and young children.

Rotkirch distinguishes four kinds or "levels" of explanation of childbearing and fertility, and she calls my explanation a population level explanation. Actually, it is more correctly labeled a household (or even individual) level explanation. It is individuals, usually within households, who make decisions about childbearing, and what happens at the population level is simply the aggregate effect of individual choices. So this is part of the answer to Rotkirch's question as to whether my explanation applies to the other levels. Yes, and to all of them. Rotkirch also refers to the so-called fertility opportunity hypothesis, suggesting that it contradicts other explanations, which predict that increased wealth leads to declining fertility. But I don't think there is necessarily any contradiction, because it depends on how much wealth and on the circumstances under which wealth may be increasing. In Western industrial societies after World War II, in the United States in particular, there ensued a period of great and rather rapid economic affluence, and this was associated with the famous "baby boom." Working-class and middle-class people were able to increase their fertility because of a rather sudden and unexpected affluence, and the baby boom only lasted about ten years before fertility leveled off and began to decline. The fertility opportunity hypothesis may be simply a special case of a more general phenomenon. (The fact that people previously of limited means may have more children during a period of sudden affluence means that the economic argument is not totally without merit. But again, this is a special case, not a general phenomenon.)

In her concluding section Rotkirch says that human fertility decisions are highly sensitive to environmental cues, especially levels of infant and child mortality and the availability of economic resources. She is exactly right, and Sarah Blaffer Hrdy's wonderful book *Mother Nature* (1999), which Rotkirch discusses, exemplifies this beautifully. Human males and females are primed for reproduction and parenthood, but they seem to be exquisitely sensitive, often in unconscious ways, to a broad range of environmental contingencies that make producing (or not producing) children a good (or bad) bet, and that help to determine the number of children it is optimal to have. Hrdy's book is highly compatible with DCT and in fact is actually a type of DCT analysis. This shows once again what I said earlier in my replies to Wilterdink and Niedenzu: DCT presents no simplistic biological determinism, but on the contrary takes full account of a host of socioecological conditions that interact with the evolved psychology of the brain to generate specific patterns of behavior at various times and places. I find that one cannot emphasize this too much, and I am extremely pleased to see that Rotkirch is someone who is already fully aware of it.

FRANK SALTER

Frank Salter's contribution is especially welcome for a variety of reasons, but mainly because he dares to violate what Charles Murray calls "the inequality taboo": the injunction against assuming that individuals are anything but the same in their propensities and abilities. As such, Salter provides a very useful counterpoint to the standard sociological wisdom that individual achievement and social mobility have no genetic basis. But Salter is no genetic determinist: Although rejecting pure environmentalist theories as inadequate because they are one-sided and contradicted by much evidence, he opts for a genetic-environmental interactionism that, I think, is what the evidence tells us is happening. Salter identifies his theory as a Darwinian conflict theory, and I accept this without reservation. DCT is a broad theoretical and research strategy that permits numerous theories of stratification and individual outcomes in industrial societies. I also accept Salter's contention that I myself have not really developed a Darwinian conflict analysis of modern industrial stratification systems. This is indeed true, and I have always considered this a lacuna that has needed to be filled. I am very grateful to Salter for starting the ball rolling, and perhaps at some future point I can extend what he has started.

I was pleased to see Salter refer to François Nielsen's (2006) important behavior genetic study showing a very large genetic effect on several measures of individual achievement, a moderate effect for unshared environment, and a quite small (sometimes vanishingly small) effect for shared environment. I also think Salter should be congratulated for actually using his own understanding of DCT to provide a set of recommendations – a sort of "user's manual" – regarding the mate choice strategies most likely to produce high levels of success down through the generations. One of his recommendations is: "Parents should encourage children to choose spouses with genealogical evidence of distinction in activities related to resource acquisition." Indeed, the evidence shows that many parents do this already.

W. G. RUNCIMAN

I have long been an admirer of Runciman's treatment of social evolution in the second volume of his *Treatise on Social Theory* (Runciman 1989, 285-450): He is an evolutionist and an adaptationist; he brings to the table an excellent command of historical and comparative materials, especially on the ancient world;

and he gives us a theory of social evolution that is explicitly a kind of conflict theory in its focus on the selection of social practices that will be advantageous to dominant social groups. Elsewhere (Sanderson 2007c) I have discussed what I feel is the main difficulty with Runciman's approach to social evolution, viz., its tendency to offer explanations that, although in many instances likely to be correct, remain empirically underdetermined and dangerously close to being "just-so stories."

His essay in the present volume seems to set forth two main arguments. The first is his contention that there was a second major transition in human sociality that followed the transition from nature to culture, which is *the transition from culture to society*. This second transition, allegedly occurring around 10,000 to 12,000 years ago, involved the emergence of a new form of human sociality involving roles and institutions. Prior to this time, Runicman contends, humans were cultural animals and lived in societies, but these societies had no "positions in a multidimensional social space whose incumbents are required to act consistently and predictably in consequence of the rule-governed practices which define them"; in other words, they had no roles, and thus no institutions as complexes of roles. But how could Runciman possibly know that there were no roles or institutions when everyone was still living by hunting and gathering? It would be exceedingly difficult to infer the absence of roles and institutions from archaeological materials alone. In any event, I very strongly doubt this because the period between 10,000 and 12,000 years ago marked the beginnings of the first transition to agricultural (horticultural) societies, and these early cultivating societies were only slightly more differentiated than their hunter-gatherer predecessors, a fact that provides little or no warrant for assuming some qualitatively new form of sociality. Besides, ethnographies of surviving hunter-gatherers reveal that they have roles and institutions, and thus there is a strong presumptive case that earlier hunter-gatherers would have had them too.

The other major idea in Runciman's chapter is the notion of *evolutionary dead-ends*. The example he gives is the Archaic Greek *polis*, an example that he discussed at greater length in the second volume of *A Treatise on Social Theory*. There are indeed dead-ends in social evolution just as there are in biological evolution, but what Runciman is calling a dead-end seems to me more like a preparatory stage. The Greek *polis* was not a dead-end but a way-station on the path to a more developed state. Runciman has also referred to Melanesian "big man societies" as dead-ends, but they were not dead-ends at all. They did not lead to an evolutionary cul-de-sac, which is what a dead end would be, but rather to further social evolution, in this case the chiefdom. A much better example of an evolutionary dead-end would be twentieth-century Communism, an utter ruin that has had to be completely abandoned before any further social evolution could proceed. This was perhaps the greatest evolutionary dead-end in all of human history.

PETER HEJL

Hejl's chapter reveals that he is by and large a sympathetic critic. His most important criticism is that my DCT omits human communication and is therefore missing a vital element. He is certainly correct that I simply assume human communication rather than explicitly consider it. I must plead guilty. How serious an omission this is I am uncertain, but when I get around to reworking and updating my DCT in that second book that Rosemary Hopcroft wants me to write, I will seek to look into some of the important literature and see what I come up with.

In his discussion of the media of communication Hejl distinguishes three types, which he calls primary, secondary, and tertiary media. I take him to mean by primary media of communication the use of language in face-to-face interaction, as well as the use of nonlinguistic modes of communication: visual, olfactory, gustatory, tactile, and acoustical. By secondary media he means "the representation of knowledge by objects outside human memory," in particular writing. Here Hejl refers to the work of the anthropologist Jack Goody, which shows that dramatic social consequences followed the invention of writing. The general point is undoubtedly correct. Indeed, in recent research I have undertaken on the long-term evolution of religion (Roberts and Sanderson 2005), my collaborator and I found that writing was a critical prerequisite to the development of ecclesiastical religions of the monotheistic variety.

Tertiary media for Hejl include such modern inventions as the telegraph, telephone, phonograph, film, radio, and television. He seems to be making the point that these new media produced dramatic consequences, and who could seriously disagree with that. But let me add an example of my own of a less obvious consequence. Many years ago I happened to read a book entitled *Teaching as a Conserving Activity*, published in 1979 by Neil Postman. One of the things I learned, much to my surprise and delight, was that there was a field called *media ecology*, and that Postman was apparently one of this field's leading practitioners. Postman defined media ecology as "the study of information environments," and went on to say that the field "is concerned to understand how technologies and techniques of communication control the form, quantity, speed, distribution, and direction of information; and how, in turn, such information configurations or biases affect people's perceptions, values, and attitudes." Postman was highly critical of certain pedagogic practices that had begun to enter higher education, especially the use of television videos and films in the classroom. He argued that modern television, film, and electronic media had already had dramatically negative effects on students' abilities to read the printed word and that the last thing university professors should be doing was allying themselves with these modern media. As an example, he mentioned the National Education Association's giving an award to the originator of the children's television program *Sesame Street*, noting that what it teaches is the same thing that Burger King commercials teach!

Postman was writing these things nearly thirty years ago, but the situation is far worse today. People wonder why today's college students cannot read or think at appropriate levels, but it has never been a mystery to me. From an early age they are inundated by tertiary media that have come to replace the secondary medium of the printed word. Reading and writing are hard work that do not come naturally to humans because writing is only 5,000 years old at best; in the ancestral environment it did not exist, and therefore there would have been no selective pressures exerted on reading or writing abilities. But television and film watching is easy and highly passive, so young people quickly gravitate to it and learn to dislike reading and writing. Most undergraduate students today cannot read anything of any degree of sophistication at all, cannot dissect or make intelligent arguments, and cannot write coherent sentences let alone entire paragraphs or papers. All of this is the effect of the new tertiary media that dominate our communicative world. I believe that this is a good example of one of the major arguments that Hejl is making. The question is, with the massive technological changes that are now occurring, what is waiting for us several decades down the road? Actually, I don't think I want to know.

CONCLUSION

In conclusion, let me simply reiterate what I said at the beginning of this reply: I am delighted to have had this opportunity to engage a number of Western sociologists on key issues of mutual concern. Evolutionary sociology is still in its infancy, but I hope this volume will go some way toward launching it into its early childhood. We are still a long way from evolutionary sociology's adolescence and adulthood, but at least we are starting to take the necessary steps to getting there.