

# Darwinian Conflict Theory and Evolutionary Sociology

## A Reply to Critics and Fellow Travelers

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Let me begin by thanking Tamás Meleghy, Peter Meyer, and Heinz-Jürgen Niedenzu for organizing the conference at Innsbruck in June of 2006, during which the original versions of these papers were presented. It is always gratifying 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 find out that there are European scholars of a Darwinian persuasion whose work I was unaware of. Some of these write in English (such as J. P. Roos and Anna Rotkirch) whereas others (such as Tamás Meleghy, Michael Schmid, Heinz-Jürgen Niedenzu, and Peter Hejl) write mostly in German. Unfortunately, I cannot read German, but I am thinking that it might be a worthwhile endeavor to consider translating some of these German works into English. Perhaps that can be a future project. These things having been said, let me turn without hesitation to the critics' comments.

### **Michael Schmid**

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 the publication of *The Evolution of Human Sociality*, because he raises so many excellent questions that he most

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. Well, 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 to which male sexual inclinations are subject. 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 than 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’s main concern is a theory of social forms and their dynamics of development.* No, not necessarily. Had Schmid made this point a dozen years ago, he would be right. At that time I was concentrating mainly on long-term social evolution. But *The Evolution of Human Sociality* and the general theory that undergirds it is broader in its scope of application. Its concern is

the sum total of sociocultural universals, similarities, and differences found in all known human societies, including the transition over time from one social form to another. Schmid is perhaps confused for good reason on this point, because in my work I use the term *evolutionary* in two rather distinct ways. Evolutionary can be used to refer to the evolution of societies or social forms, but also quite differently to the biological evolution of human nature and its expression in the entire range of human social behavior. Some years ago Alexandra Maryanski (1998) coined the term *evolutionary sociology* to refer to theoretical and research work of both types. At first I didn't like her term because, since it mixed together these two different senses of evolutionary, I thought it was potentially confusing. But gradually I have come to like it, or at least accept it, and in any event it fairly accurately describes what a lot of us are doing. Because of this dual meaning of the term, those who use it should specify in which sense they are using it.

*Why does Sanderson assign processes of economic production and distribution to "ecostructure" rather than to "structure"?* For the detailed version of my answer to this question, I refer the reader to Sanderson (2007a). But the short answer is that, although economic production and distribution are certainly social structural features, they have their own unique causal importance and therefore deserve their own special category. (Wilterdink raises a similar point, and I refer the reader to my remarks on Wilterdink below.) Concerning questions of classification, *Schmid also wonders why I put "feelings" in the superstructure rather than in the biostructure.* This point is very well taken. When I originally used the word "feelings" I had in mind collective sentiments regarding such things as impressionist art or contemporary rock and roll music. But obviously there is an entirely different sense of "feelings," which is *deep human emotions*. The basic wellsprings of human action contained within DCT are obviously deep human emotions and thus are "feelings" in this second sense. It was unfortunate that I used the word "feelings" in this one sense only, thus creating the confusion in Schmid's mind. It seems desirable to have separate terms for these two categories of feelings, but at the moment I am not sure what those terms would be.

*Sanderson at times speaks of "maximizing" and at other times of "satisficing," thus taking irreconcilable positions.* Actually, my position is that humans are *not necessarily* maximizers and that much of the time (perhaps even most of the time) they are satisficers. It depends on what goals are being pursued and what constraints are being imposed. Capitalists really are maximizers rather than satisficers; great tennis players like Roger Federer or golfers like Tiger Woods are also maximizers in the sense that they are not satisfied with being less than the greatest in their sport; and great womanizers like Bill Clinton or Jean-Paul Sartre may be maximizers of the number of women sexually conquered. But most of the time most people are satisficers because the constraints on maximizing are too great, and in any event the whole question is an empirical one.

*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?* Schmid's question indicates that there is yet a third meaning that can be assigned to the word "evolutionary," i.e., as a type of explanation, which is one in which the *explanans* is evolutionary, but not in a sociobiological sense. They can be stated 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 (material, ideational, etc.) whose *explanandum* is social evolution. *Examples*: Sanderson's *Social Transformations* (1995; 1999), 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. *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 now largely forgotten 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).

So the plot thickens, and if we are not careful we are going to step into a bottomless pit. I use the term evolutionary only in the first two senses and rarely in this third sense, and thus my explanations are not evolutionary in this way. But why not? Mainly 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. Since there is a contribution to this volume that *is* evolutionary in this third sense, the chapter by Runciman, I will postpone elaboration on this point to my discussion of him.

*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 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 (1975; 2009; Rossel and Collins, 2001) 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.)

*Sanderson does not tell us what the mechanisms are that produce the main forms of evolutionary dynamics, i.e., parallel, convergent, and divergent evolution.* The mechanisms are in all three instances in principle the same, and those mechanisms are discussed at length in *Social Transformations*. Parallel and convergent evolution occur when different societies are exposed to similar circumstances, whereas divergent evolution is the outcome when two or more societies are exposed to different circumstances or socioecological constraints. But whether the circumstances are similar or different, the main causal variables in all three forms of evolution are postulated to be largely of the same general type, i.e., ecostructural conditions and their biostructural substrates. (A truly adequate answer to this question requires much further elaboration, but that is beyond the dimensions of this paper.)

*How does DCT differ from rational action theories, which do not claim to be using or defending a general theory of evolution.* I have long been impressed with certain types of sociological rational choice theories and have sometimes used them myself. DCT assumes a kind of rational action theory in that much behavior involves individuals trying to achieve very favorable cost-benefit ratios with respect to their goals. But Schmid's way of asking the question contains the key to the answer: Rational action theories take individual cost-benefit calculations as the starting point of analysis, but these calculations remain merely unexplained givens. Rational action theorists do not seek to explain *why* humans should be cost-benefit calculators, nor do they seek to explain the *preferences* or *goals* that individuals have. These too are unexplained and unexamined givens. What DCT does is to take rational action theories to a deeper level by showing why individuals should be cost-benefit calculators, what their most fundamental goals and preferences are, and why they should have these preferences or goals. Rational action theories are themselves highly incomplete; they need Darwinian natural selection to ground them.

And in this connection I can answer another of Schmid's questions, namely: *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 people 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, all 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's conflict theory does not take into account highly diverse forms of conflict (e.g., zero-sum games, "battles of the sexes," "mixed motive games") and thus is in need of further specification.* This is a very good point and one that I shall try to address in later work.

*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. Probabilistic 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 pleased 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 I'm always going to be right, of course.

### Nico Wilderdink

Wilderdink 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 splitting apart things that belong together. Wilderdink 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 Wilderdink's attention to the etymology of the prefix "eco." It comes from 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.

Wilderdink 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. Wilderdink also contends that I provide no clear argument linking biostructure to ecostructure, ecostructure to structure, and structure to superstructure. I must refer Wilderdink 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 added italics to 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 Wilderdink 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 Wilderdink: "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 Wilderdink really believe this?

In order to illustrate these last points 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 behaviors and sociocultural traditions and leaves them as naked as jaybirds, although with a rudimentary language of, say, 500 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

uninhabited 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 with which to procure 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 adaptive behavior patterns already established). Thus a society 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 society 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 or socioecological contingencies that are a crucial part of DCT, which Wilterdink (and some of the other critics in this volume) think 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 deculturated 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 social 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, after it emerged in rudimentary form and became more elaborate, 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 in my societal components? They are dynamic dimensions of humankind and its creations, which are constantly adapting themselves to ever-different and ever-changing conditions. Where on earth does Wilterdink get the idea that my concepts are static? He has a vivid imagination and an unusual tendency toward unsupported attribution.

Let me now turn to some of Wilterdink's specific charges:

*Biological-evolutionary theory is unable to account sufficiently for the wide variety of sexual norms and practices.* Yes, but I make no claim otherwise. To account for such practices one has to invoke principles concerning the costs and benefits of certain sexual practices along the lines suggested by Richard Posner (1991).

*Why have some groups proclaimed and put into practice norms and ideals of chastity and sexual abstinence? Why was the West for a long time a “sex-negative” society?* These are excellent questions, to which I have no satisfying answer at present. One possibility concerns the origins of the sex-negative religions Judaism, Christianity, and Islam. These all arose in a very small part of West Eurasia characterized by some of the most patriarchal societies the world has ever seen. In those societies, at that time and even today, there was a tremendous emphasis on

social practices designed to protect female virginity, something highly desired by males throughout history: veiling, seclusion, genital mutilation, male chaperonage in public, and so on. It is well known that these religions have historically had a highly patriarchal character because they arose in highly patriarchal societies. The religions may also have incorporated the “sexual culture” of those societies, and in the case of religion once something originates it has a remarkable tendency to persist. But this is only speculation, and the answer may well be different.

*People do not have sex so much for purposes of reproduction but rather for the sensual pleasure it brings.* Yes, of course, but I never suggested otherwise. When I said that people have sex “to promote their reproductive interests,” I meant simply that that is the *ultimate purpose* of sex and that natural selection has made sexual intercourse an intensely pleasurable activity so that humans will engage in it frequently. That proximate mechanism has evolved to be in the service of something ultimate.

*The evolutionary explanation of sexual behavior goes astray when homosexuality is considered.* The evolutionary explanation of sexual behavior is not intended to explain homosexuality as an adaptive behavior, but rather would explain it as the result of genetic mutation. Homosexuality would only be problematic for my theory if preferential homosexuality were widespread in human populations, and it is not. Situational homosexuality is common in certain societies or segments of societies at certain times, but in ways entirely consistent with DCT (largely as the result of the absence or relative scarcity of heterosexual partners).

*A substantial and growing proportion of the adult population in Western societies, in particular women who wish to give priority to their professional careers, choose not to have children.* Yes, of course, but as Rotkirch’s paper (Chapter 13) makes clear, this is still only a small minority of the population, and women who choose childlessness often experience the emotion of “baby fever” later in life. It is important to stress that the unconscious drive to promote one’s inclusive fitness is not the only human drive. Humans also have drives for status, prestige, power, economic success, and so on, and these drives may conflict with the drive to maximize inclusive fitness. It is very important not to lose sight of fact that the maximization principle, so named by Joseph Lopreato, has been modified by him into the *modified maximization principle*: People act so as to promote their reproductive success *unless they are diverted from this aim by other aims that are especially prevalent in modern societies, such as the quest for status and creature comforts.*

*If people in modern prosperous societies were really intent on maximizing genetic reproduction, they would produce far more offspring than they actually do. The high level of material prosperity and medical care would enable them to have an extraordinarily large number of healthy children.* Wilterdink quickly forgets that resources are not unlimited and that modern societies are intensely competitive societies. He also forgets that children are extremely costly in modern societies. Children have to be fed and clothed for many years and then sent off to increasingly expensive colleges and universities in order to be successful in the labor market. To have an extraordinarily large number of children would be a colossal disaster for nearly all parents in terms of the economic and reproductive success of their children. Imagine what would happen if each couple would choose an extraordinarily large family. Wilterdink does not operationalize “extraordinarily,” but let’s set it at about the biological maximum for each woman, which is around 16 offspring. Imagine a society in which the level of competition for positions in the labor market (where many college graduates now hold ordinary jobs) *were suddenly increased some eight-fold.* This is not a strategy that would make much sense, except in a society with infinite resources, an infinitely expandable labor market, and a female

population with no interests or concerns other than pregnancy, childbirth, lactation, and child nurturance. Does Wilterdink know of any society like that anywhere?

*Throughout most of human history there was a positive correlation between a man's status and his reproductive success, but this has now been reversed.* This may appear to be so on the surface, but there are a number of studies suggesting that the positive correlation is still with us even today (cf. Sanderson 2001a, 163-64).

*Social developments in the long run are the largely unintended results of the actions of vast numbers of competing and cooperating individuals with their own intentions and interests.* Wilterdink makes an important point here. Unintended consequences of human action are constantly at work and have to be regarded as an important part of the socioecological context or external contingencies to which individuals must adjust their actions. However, despite such consequences, this does not mean that we cannot identify what these intentions and interests are, as Wilterdink seems to imply. Max Weber gave a significant role to the unintended consequences of individual action, but this did not prevent him from identifying important human interests.

Why has Wilterdink gone so badly wrong in his critique of DCT? Let me suggest two reasons. First, Wilterdink is a victim of his sociological education. As Pierre van den Berghe has commented, training in sociology 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 doesn't explain anything *but itself has to be explained*. Wilterdink is, in Homans's words, a “culture vulture.” Note that in trying to explain 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.” To stop there is to avoid entirely the crucial question as to why people make some choices rather than others or why they attribute certain meanings rather than others to their actions. This is not what sociology needs in the early third millennium CE. Eventually I hope to write a book on the concept of culture in which I will suggest a complete reformulation of the 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 context of modern socioecological contingencies have been moving 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

worshipping 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 a very 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. 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 for 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.

I have only a few quibbles. Hopcroft suggests that DCT is not quite the micro-macro theory that I claim it is, and that the real theory is really a micro theory – sociobiology or evolutionary psychology. No, not quite. The *starting point* for DCT may indeed be characterized in this way, but Hopcroft almost completely ignores my discussion of modes

of Darwinian conflict explanation, which include not only *biomaterialist* explanations but also *ecomaterialist* and *polimaterialist* explanations. I think she does this because she is simply choosing to pull out of my paradigm what she understands best and most wants to work with. But DCT is a genuinely multidimensional theory – not nearly as multidimensional as those produced by such sociological theorists as Randall Collins and Jeffrey Alexander, especially given DCT’s refusal to grant *ideational* (and thus nonmaterialist) explanations official status, but multidimensional nonetheless. The theory starts with microlevel biological principles and works up from there. But it is fully capable, with a certain amount of tweaking here and enhancement there, of explaining large-scale macrolevel phenomena, such as long-term social evolution, social revolutions, globalization, the rise and fall of states and empires, and the like. In many ways I think the micro-macro distinction in sociological theory has garnered far more attention than it deserves. There are simply individuals acting in accordance with their predispositions and interests, who then create a vast range of socioecological contingencies that establish a context within which future generations must act in accordance with their predispositions and interests.

Hopcroft says that my *Evolution of Human Sociality* is incomplete. Indeed, she is most certainly 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 final theory, but we can at least move toward one.

### Christoph Antweiler

Antweiler’s chapter is short but pithy and he raises several 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). I also appreciate Antweiler’s point that I present things clearly. This is very gratifying because I work very hard to be as clear as possible.

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 calls for a special type of explanation. I think this explanation must 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 in *The Evolution of Human Sociality* I have a list of only 43 universals, there being many more, and that I give no argument for limiting myself to this number. Actually, my justification was simple: I merely borrowed the list developed by Donald Brown in his book *Human Universals* (1991). I made no claim that this was a complete list, and in fact clearly indicated that it was not. Had I been devoting an entire book to human universals, then I would certainly have done what Antweiler advises: Consult other lists and try to come up with a comprehensive list that left virtually nothing out. This would still be a useful exercise that might engage my mind some day, but in *The Evolution of Human Sociality* it was not my aim.

And incidentally, there is a closely related concept that we should take note of: the *near universal*. A near universal is a trait that is found in the vast majority of known societies (95 percent, say) but not in every single one. If we find something that is a near universal it is almost as good as finding a true universal, because the presumption of a biological foundation to the trait would still be warranted. The tiny handful of societies that would be counted as not having the trait might not be genuine exceptions but simply the result of measurement error: The ethnographer didn't notice it, it was recorded under other terminology, the ethnographer simply got it wrong, and so on. A good example of a near universal that is likely to be a true universal is romantic love (cf. Jankowiak and Fisher 1992).

Antweiler's point that my work almost completely ignores social institutions 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 is less a critique than an intellectual drive-by shooting. He is resolutely hostile to Darwinian social science and to any notion that it might bear similarities to Harris's cultural materialism and thus be potentially synthesizable with it. Hakami also reveals himself to be both an uninformed and a misinformed critic who starts one fire after another. I shall try to put out these fires, and thus correct the terrible disservice he does to

sympathetic or potentially sympathetic readers, by way of responding to what I take to be his major points.

*Sanderson attempts to bring together two old and traditionally opposing strategies, and the resulting synthesis is Sanderson's evolutionary materialism.* Hakami has blundered right out of the gate. *Evolutionary materialism* is the name I gave over a dozen years ago to the strategy I developed for the special analysis of 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.

*As Hallpike has shown, it is impossible to apply Darwinian principles to the evolution of human societies.* Darwinian principles can be applied to human social life in two quite different ways. One is the way I have tried to do it by means of DCT; the other is the application of the Darwinian principles of *variation* and *selection* to understand not social life in general, but trajectories of social evolution. This is the kind of Darwinism that is exemplified in this volume by W. G. Runciman, the leading sociological representative of this approach, and by such anthropologists as Robert Boyd and Peter Richerson (1985; Richerson and Boyd 2005) and William Durham (1991), and that was perhaps first proposed in its contemporary version by Donald T. Campbell (1965; 1975; but see Keller 1931 for what is probably the very first version of this approach). Although I obviously disagree with Hallpike on the merits of sociobiology, I actually am in substantial agreement with him when he suggests that the classical Darwinian notions of variation and selective retention are difficult to apply to social evolution. However, as indicated in my reply to Schmid, I want to postpone further discussion of these difficulties until I comment on Runciman's chapter.

But because Hakami apparently does not know when to stop talking, he stumbles into Blunder Number Two. Given his endorsement of Hallpike's opposition to natural selectionist theories of social evolution, what on earth are we to make of Hakami's comments, later in his critique, that “natural selection can work on everything, not only on individual organisms,” and his quotation of Carneiro 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). Hakami has shifted in only a page or two from agreement with Hallpike – he quoted Hallpike to the effect that there “is no significant resemblance between the *mutation*, the basic source of variation in the Darwinian scheme of things, and social *invention*, which is purposeful, responsive, and can be diffused” – to apparently strongly endorsing use of the principle of natural selection as a social evolutionary principle. Something is more than just slightly amiss here. But things get even worse. Hakami clearly does not understand the central point of the article Carneiro wrote on the principle of natural selection as a social evolutionary principle, namely, that although social evolution may look like a process of natural selection and be described in natural selectionist imagery, the principle of natural selection is inadequate because it cannot identify the genuine *causes* of social evolution. In other words, *Carneiro's article is mainly a critique of natural selectionist theories of social evolution!* I think that Hakami needs to (a) figure out just what Carneiro is saying regarding natural selection and social evolution, then (b) decide whether he is for or against

natural selectionist theories of social evolution, and finally (c) decide whether he wants to use Hallpike's comments on such theories against or for me. Then perhaps the debate can be continued in a way that makes sense.

*Methodological individualism is wrong because no single Kwakiutl developed the potlatch, no single Kachin created a complex alliance system, no single Australian bushman set down a section system, and no single Trobriand [sic] invented the kula ring.* Except for the introductory clause “methodological individualism is wrong because,” Hakami is exactly right: No single member of any society could be said to have developed single-handedly one of that society's characteristic social patterns. But here Hakami completely misunderstands methodological individualism in the most simplistic manner. Cultural patterns are obviously collective creations; no methodological individualist ever claimed otherwise, and no sociologist or anthropologist would be so foolish as to make such a claim.

*Hunter-gatherers are really gatherer-hunters.* Blunder Number Three. I don't know where Hakami is getting his data, but this is just flat out wrong. The idea became popular in the 1970s and 1980s and even had a whole book written in support of it, Frances Dahlberg's *Woman the Gatherer* (1979). In the first edition of my *Macrosociology* (Sanderson, 1988) I said the same thing. But then I discovered Carol Ember's (1978) article on the matter. She shows that the notion that gathering is more important than hunting in the average hunter-gatherer society is a myth based on ethnographic information derived from African hunter-gatherers. Ember shows that, although gathering may be more important than hunting in most African hunter-gatherers, *in the world as a whole* the traditional wisdom that hunting is more important than gathering in foraging societies – that is, that meat makes up a majority of the diet – is in fact true (cf. Sanderson 2001a, 260).

*Optimal foraging theory is wrong and is based mainly on the study of nonhuman animals.* Blunders Four and Five. Optimal foraging theory is derived from evolutionary ecology and was originally based on studies of animal foraging patterns. However, 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. The interested reader can consult my discussion in Sanderson (2001a, 260-64) and the references cited therein for details (and see also Kennett 2005 for a detailed archaeological example).

*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. I provide a wealth of references to this effect in *The Evolution of Human Sociality*, and many more supportive studies have accumulated since that book was written. I encourage readers to take a look at the last few years of such journals as *Evolution and Human Behavior* and *Human Nature* and see for themselves.

*Marvin Harris was not a true evolutionist, but rather a pseudo-evolutionist who strung together a bunch of synchronic studies and called the result a theory of social evolution, and he wrote only one book on social evolution, Cannibals and Kings (1977).* I wish Harris were alive to answer this one, but since he isn't I will have to do it for him. Let me be brief: This is a bit like saying that Darwin wasn't a real evolutionist because he collected a lot of evidence from 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*.

*Although Sanderson was not a staunch sociobiologist at the time he formulated evolutionary materialism, things haven't changed.* Blunder Number Six. I thought the problem was that *things have changed*, and for the worse because now *I am* a staunch sociobiologist.

*Harris and Sanderson 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

There are three aspects of Meyer's chapter on which I would like to comment. First, he seems to be saying that I do not give enough emphasis to human cooperation as springing from a genuine feeling of sympathy. In this regard he cites in particular Adam Smith's famous discussion of natural human sympathy in *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 is 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, 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 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 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 different 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.

With respect to Meyer's point that Max Weber's insights on religion should be incorporated into a general theory of social conflict, I completely agree. And not only do I agree, but I have said similar things in previous publications, and at the present time am working on the evolution of different forms of religious life and drawing extensively on Weber's pathbreaking work. Randall Collins (e.g., 1975; 1986; 2009) and Michael Mann (e.g., 1986; 1993) have made important extensions of Weberian analyses of religion in a conflict-theoretical manner, and I admire the work of both (although by no means always agree with it).

Finally, just a small point. Meyer is setting up something of a false dichotomy in opposing Milgram's (1974) famous work on obedience to authority and my claim that disobedience is frequently rather than rarely encountered. There is no conflict between Milgram's results and my statement. Humans are primed both to obey and to disobey, and it is those by now familiar socioecological contingencies that determine which way they will go. The human brain, as Milgram argued, has a natural tendency to obey that has evolved by natural selection (Milgram was well ahead of his time). This is obvious through even a cursory examination of conformity and the political obedience of subjects in hierarchical societies. But humans are not built to obey when obedience would work against their interests. When people are subjected to extremely high levels of exploitation and oppression, and when disobedience in the form of rebellion, revolt, or revolution is thought to have a good chance of success, they often revolt (Sanderson 2005).

### Heinz-Jürgen Niedenzu

I very much appreciate Niedenzu's thoughtful critique. I am gratified to see that he recognizes that sociology in the late twentieth century has been far too "culturalist," and that it remains so in the early twenty-first. He clearly recognizes a role for biology in human behavior and the construction of human societies, but he wants to steer a middle ground between what he supposes is the reductionism of sociobiology and the excessive voluntarism of culturalist modes of explanation. On the surface, this seems eminently reasonable and sensible, but I think that in practice it would probably leave us with an unsatisfying eclecticism in which, despite nods to the role of biology, explanations that insist on the autonomy of the cultural realm are still given a role in sociological explanation that they do not deserve.

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 polymaterialist 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, however, that it works, and I doubt that there can be any such thing as *pure constructivity* or *pure creativity* entirely free from

biological constraints. And I would say that this is so both in the case of individuals taken alone and in the case of larger groups of individuals taken as collectivities. In this regard I suppose I am a firm determinist. I have been asked several times, “Can your Darwinian paradigm explain Shakespeare?” Usually this is framed as a knock-down question, or one in which the questioner thinks the answer automatically has to be no. But, in fact, it can, and in two ways. I know little of Shakespeare the man, but my best guess would be that this man had an extremely rare form of brain wiring that gave him extraordinary linguistic abilities, which he combined with unique insights into human nature and the subtleties of human interaction. He put these skills to great use in his plays. But of course Shakespeare could not have succeeded had there not been an appreciative audience. Shakespeare lived in a time and place (seventeenth-century England) which had a class structure and a “culture” that could be uniquely appreciative of his contributions. But can we apply Darwinian thinking to literature as a whole? Most humanists and social scientists would undoubtedly say no, but to those naysayers I recommend the interesting works of Joseph Carroll (1994; 2004) and David and Nanelle Barash (2005), among other Darwinian analysts of literature. And what of another major realm of human creativity, art? Surely the paintings of, say, a Jackson Pollock or a Picasso have much to do with the unique brain wiring of those great artists, although of course their recognition as great is highly dependent on social conditions. Both would have been thought absurd in the time of Rembrandt and laughed off the face of the earth. To those who say that Darwinism has nothing to say about art, I recommend the works of Ellen Dissanayake (1990; 1992) and Geoffrey Miller (2001).

Another realm of human constructivity and creativity is religion. 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 what went before, and yet they were very similar to each other, especially in their conception of a *transcendent* reality (Eisenstadt 1986). It is remarkable that throughout much of West, East, and South Asia very similar religions arose in a very concentrated period of time, even though the East and South Asian religions did not appear to have had much influence on the West Asian ones, and vice versa (Bentley 1993). Where did the new religious ideas originate? As Max Weber has argued, such ideas sprang from religious virtuosi, individuals who had special religious skills and insights. I accept this, but would add that such individuals are very likely individuals with a very unusual brain wiring 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 work out a systematic theory of these conditions (Sanderson 2007b; 2007c).

So I would contend that it is doubtful that human constructivity and creativity are ever autonomous. They only appear autonomous to many observers 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 is a complete mystery. I don't understand how anyone can "see" in a painting some deep meaning beyond just what is there on the surface. How anyone could possibly "read" or "interpret" a painting is completely beyond my comprehension and I find it uncomfortable even to discuss such things. I like Pollock and Picasso simply because of the colors and the shapes. (Many people feel that such hard-headed determinism is cold and calculating and takes away the very essence of our humanity. To them I say, nonsense, it shows precisely what our humanity consists of. Of course humans make choices and have intentions, but those choices and intentions are determined. The idea of free will is just a human conceit, and itself a determined one at that!)

Let me conclude by addressing Niedenzu's point that DCT makes ecomaterialist and polymaterialist 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 polymaterialist 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 1999), 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 2007d, 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 ecomaterialist 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 polymaterialist (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 polymaterialist explanations have to be grounded in human biology, such explanations are often quite fundamental and much more than merely derivative.

### **Tamás Melegly**

Melegly believes that my criticisms of Lévi-Straussian structuralism in *The Evolution of Human Sociality* were too severe and that there is much in that theoretical strategy that can be salvaged. Indeed, Melegly wishes to advance the structuralist theoretical agenda, and to do so by adding to it the principle of inclusive fitness. I quite agree with Melegly'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 not published, I used a cross-cultural sample of 60 preindustrial societies and cross-tabulated two measures of paternity certainty with a society's mode of descent. Using the measure of paternity certainty developed by Gaulin and Schlegel (1980), I found that 80 percent of matrilineal societies had low paternity certainty compared to 50 percent of bilateral societies and only 36 percent of patrilineal

societies. I obtained even more powerful results when I substituted a measure of paternity certainty developed by Mark Flinn for Gaulin and Schlegel's measure. In this case, 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 (1969) 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 rather than 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, at least of matrilineal descent, and therefore I recommend that we follow the basic dictum of 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. If Meleghy doesn't believe me, I would urge him to try his idea out on a bunch of structuralist anthropologists, such as Marshall Sahlins (a major critic of all forms of materialist explanation, Darwinian materialism in particular; cf. Sahlins 1976), rather than on the evolutionary social scientists represented in this volume. The reaction he would get would be extremely cold if not downright impolite.

So no, I do not accept the idea that DCT is somehow a missing link in the structuralist program, or that any of Lévi-Strauss's ideas could be missing links in DCT. I do agree with Lévi-Strauss (and with Meleghy) that the human mind (read: human brain) does have a tendency to think in terms of binary oppositions, but this says something about universal human cognition and not likely very much about patterns of human social organization.

**Johan van der Dennen**

Van der Dennen focuses entirely on my Darwinian conflict analysis of war. Since he is a specialist on war, this is unsurprising. He objects to materialist explanations, whether of the biomaterialist or the ecomaterialist type, which, of course, are the explanations that I favor. Van der Dennen seems to think that we materialists are “vulgar,” and that materialist explanations leave little room for human decision-making. This charge was made countless times against Marvin Harris, and now I see that it is being flung at me. But Harris answered this at length, and so have I (Harris 1979; Sanderson 2001a). Let me also note that van der Dennen is critical of Brian Ferguson for claiming that the desire for security or safety is a material desire. What on earth could be more material than one’s own physical safety? Is one’s living body not a material object?

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 will be matters of detail and nuance rather than any fundamental change of perspective. I used to accept the old Divale-Harris theory of primitive warfare, which holds that tribal societies like the Yanomamo are fighting over one particular scarce resource – animal protein – and that war is functional (adaptive) in spreading out populations relative to resources. Divale and Harris never did present anything more than circumstantial evidence in support of this theory, and when Napoleon Chagnon attempted to test it by measuring in kilograms how much meat the Yanomamo were eating he found that they were getting plenty. I have now abandoned this theory in favor of one that combines the “fighting over women” argument of Chagnon with the ecomaterialist suggestions of Brian Ferguson, in which he lists six material reasons for tribal war: (1) eliminating competitors for fixed resources; (2) capturing movable goods; (3) imposing an exploitative relationship on previously autonomous groups; (4) conquering and incorporating other groups or societies; (5) enhancing the power and status of those who make war; (6) defending against attacks by other groups. Van der Dennen says that to explain tribal warfare by singling out one factor, such as the capture of women, is too simplistic. I completely agree. And I am not wedded to the scarcity of women hypothesis. I only hold to it provisionally as one of the principal causes and would gladly give it up were I to be presented with strong evidence that it is false.

Van der Dennen says that I do not distinguish between genocidal wars, instrumental (coercive) wars, and ritualized wars, and that I 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. What appears to some observers as some sort of ritual is a matter of opposing groups facing each other from a distance and making a lot of bluffs, doing a lot of shouting, issuing a lot of threats, and brandishing a lot of weapons. But this is serious business, not a mere ritual. One group hopes it can scare the other off without having to resort to actual fighting. It is simply a collective version of one man threatening another with violence in hopes he doesn’t actually have to resort to it.

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, on which there is no chapter in *The Evolution of Human Sociality*. 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 terminology. My discussion of tribal warfare is basically a discussion of ambush-like war, which prevails in bands and tribes. 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 (Ferguson's material interests 3 and 4), 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).

Van der Dennen also charges me with conflating general violence with war. Indeed, the characterization seems fair, but I do not regard this as a deficiency in my argument. Since writing *The Evolution of Human Sociality* I have encountered the work of Azar Gat (2006), who notes that in bands and tribes it is often very difficult to distinguish between homicide and war; the one often shades into the other. The Yanomamo are a classic case in which violence pervades individual villages and the relations between villages. Men kill other men within their villages, raid other villages and kill as many men as they can, and also invite the members of other villages to what Chagnon has called “treacherous feasts,” which are invitations to form alliances that turn homicidal once the guests are inside the host village and preparing to dine. Are “treacherous feasts” warfare? Murder? It is almost impossible to say. But need we?

What then of van der Dennen's own preferred explanations of war in all of its varieties. 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: The fear of attack by neighbors does not explain anything so much as it must itself be explained. As I am writing these words there is concern among many American leaders about Iran obtaining nuclear weapons. There is real fear. Some leaders recommend that the United States needs to strike Iran before it can build such weapons, because if it acquires them it will be too late to protect ourselves. I take no position on this issue, but simply note once again that fears are seldom groundless. Bands, tribes, chiefdoms, and states often fear their neighbors because they really do have something to fear.

### **J. P. Roos**

Roos's article is extremely important. He shows conclusively what I myself have been learning over the past few 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, Emile Durkheim. I have known of Westermarck for years, but until recently I thought he was only important for a theory of incest avoidance that is turning out to be very 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. Durkheim held the rather

absurd view that one could develop a general theory of religion simply by studying a single case, the Arunta of Australia, an atypical human society if there ever were one. And did Durkheim ever visit the Arunta? No. His analysis of religion was pure armchair theorizing. In fact, Durkheim never did any fieldwork in any society of any type, unlike Westermarck, who spent many years studying tribal societies in Morocco and wrote several very large ethnographic works. Westermarck was also a comparativist in the best sense of the term: He marshaled an enormous amount of data on societies of every conceivable type and used these data to support provocative Darwinian hypotheses about a wide range of behaviors and social patterns.

It is a sad commentary on the field of sociology that Durkheim, a scholar inferior to Westermarck in almost every respect, won their rivalry and today is considered one of the three leading classical sociologists of all time, whereas Westermarck is little more than a historical curiosity. Can anyone name a single textbook dealing with classical sociological theory that has any discussion of Westermarck at all, let alone a full chapter or two? Of course no one can, because there isn't one. To the best of my knowledge the last textbook on classical theory to discuss Westermarck was Harry Elmer Barnes's *An Introduction to the History of Sociology*, published in 1948. This book contains a chapter on Westermarck written by a then very young C. Wright Mills, and Mills dismisses Westermarck as a minor thinker who had no overarching theory was little more than a "stamp collector." It is written in the sarcastic and disrespectful tone for which Mills was to become famous.

The only reservation I have about Roos's chapter is his strong claim that on every important issue where he 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 (e.g., Shepher 1983; Wolf 1995), 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 (e.g., Arnhart 1998; Hauser 2006; de Waal 2006). But these are only two issues. What of all of the other issues that both scholars 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 did not 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 did not 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 insightful. 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 the novelist Rebecca Walker. The author, a feminist and career professional, explains that for many years in her 20s and early 30s she had a strong desire – this desire may actually have begun as early as age 18 – to have a child but put this thought aside in order to concentrate on her career. Finally, in her mid-30s, she gave

in to her desire, became pregnant, and gave birth to a child at age 35. 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 this experience as the greatest experience of her life. And a couple of years ago the long-running American news program *60 Minutes* featured a segment about four women who were apparently quite brilliant 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.

The central concern of Rotkirch's paper is why people have children. When I was in my early thirties I myself did not quite understand on a purely personal level why people had them. I actually rather liked children, but I had no particular desire to have any myself. In 1978, after my wife and I had been married about three years, she came to me one day and said, "I think it's time to have a baby." Oh, I said, that's interesting; why do you want to do that"? She had no specific answer, simply saying that she just did. She asked me to think about it. I did, but only for a month or two, and ended up agreeing to have a child. My wife became pregnant quickly, and nine months later our son, now 29 years old, was born. I have to tell you, it was one of the most thrilling days of my life. And then three years later our daughter, now 26, was born, and that was pretty exciting too. Consistent with what Hopcroft says in her paper about the greater parental solicitude of mothers, my wife took a much greater role in rearing these children than I did. However, I was a very willing and very engaged father. I loved my children intensely (and still do) and loved playing with them for hours on end. I can honestly say that being a father has been one of the greatest experiences of my life.

My original relative indifference to having children, especially when combined with my great enthusiasm for parenthood after my children were born, raises an especially important question concerning the extent to which people's decisions to have children and a specific number of children are fully conscious decisions, which is to say whether they are actually and truly "decisions." Although family planning is widely and extensively practiced in modern industrial societies, it is not altogether clear the extent to which this occurs in preindustrial societies. Harris and Ross (1987) point out that fertility is regulated in all societies, and indeed it is. But it is not regulated in quite the same way. In this regard I am reminded of Theda Skocpol's highly nonvoluntaristic stance on the causes of revolutions: Revolutions are not made, she says, quoting Wendell Phillips; they come. To a large extent this is also true of children: They are not made so much as they simply come.

Rotkirch quotes Westermarck to the effect that the desire for children is universal, the frequency of contraception, abortion, and infanticide notwithstanding. Yes, and this desire seems virtually equivalent to an instinct. People have children because that is what they are built to do, just as other organisms have as their main goal or purpose for existence the replication of themselves. In preindustrial societies virtually no one ever remained voluntarily childless, and even in advanced industrial societies, where people seek all kinds of selfish gratifications that the presence of children can interfere with, only a small segment of the population remains voluntarily childless.

But I didn't always think this way. For many years I endorsed the common argument, made perhaps most forcefully by scholars such as Harris and Ross (1987) and Ester Boserup (1981), that the number of children people have is determined primarily by their economic benefits or costs: People have many when children are economic assets, and few when children's economic costs exceed their benefits. I set out to test this argument

several years ago using multiple regression analyses of cross-national data (Sanderson and Dubrow 2000; Sanderson 2001b), and to my considerable surprise the data provided almost no support for it. My results showed instead that fertility levels were determined mainly by infant mortality levels. I concluded that what people seem to be doing is having many children when many of them die early so that they end up with two or three children who survive into adulthood, but having only two or three when the chances of all of those children surviving into adulthood are very great.

Actually, I should have seen this coming. The huge fly in the ointment of the economic argument is the fact that people in modern industrial societies continue to have children even though the economic costs of children are now immense and children provide few if any economic benefits. If people are calculating fertility merely according to economic advantage, then it makes no sense for people in affluent modern societies to have any at all – ever! And yet they do, and they love them and provide for them and do everything they can to help them be successful. The strong feelings of love that parents have for their children and their pride in their children may be the reasons that parents themselves give to others when asked why they have children, but these are simply proximate mechanisms that operate in the service of an ultimate mechanism, the promotion of reproductive success. They are two parts of the same biogram, one conscious the other unconscious.

In her paper 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, it applies to all of them. Rotkirch also refers to Abernethy’s 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 people have and on the circumstances under which wealth may be increasing. In Western industrial societies after World War II, the United States in particular, there ensued a period of great and rather rapidly growing 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. Indeed, 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 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. Salter provides a very useful counterpoint to the standard sociological wisdom that individual achievement and social mobility have no (or only a trivial) 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, and Salter's theory certainly qualifies as one such theory. 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 Francois 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. In discussing these results with Nielsen, I posed to him the following observation: "Since shared environment is the stock-in-trade of traditional sociological models of status attainment, your results, if valid, pretty much blow traditional sociology out of the water, don't they." He just smiled broadly and replied that, indeed they do.

I also think that Salter is to 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. My own mother, a person engaged in high-intensity parenting if there ever were one, practiced this strategy on her only daughter, my sister, and my sister followed it. Her first husband was a banker, her second a lawyer. My sister has three daughters, all now married, and all three have followed this strategy (two are married to lawyers). A second recommendation is, if I may paraphrase: "Create a warm family atmosphere in which your children are likely to bond strongly to you and want to please you." This makes it more likely that children will be able to take advantage of the greater experience and cognitive competence of their parents and listen to their parents' advice.

I conclude where Salter concludes, and with a quote that seems to express the core of his approach:

A complete theory is not proposed here but it is argued that such a theory will be a type of Darwinian conflict theory. A Darwinian conflict sociological account of the mate choice strategies embedded in family traditions should conceptualize families as lineages that produce individuals who enter the wider society where they compete with other individuals for resources. Individual qualities are a major factor determining the outcome of this competition, and families largely produce these qualities. The theory should explain society-wide trends in social mobility but also account for possible exceptions.

### Peter Hejl

Hejl avers that I am a kind of dualist in that I choose only one side of the “individual/society,” “materialism/idealism,” “naturalism/culturalism,” and “universalism/relativism” dichotomies. He is basically correct for three of these, but not for “individual/society.” Not only do I agree with Hejl when he says that “sociological explanations need to include not only the individual and the sociocultural environmental levels, but their interactions as well,” but I made the same point in my reply to Wilterdink. There I pointed out that individual predispositions and interests always occur within a context of environmental contingencies, many of which are the products of past generations of social interactions. Marx was clearly onto something when he said, “Men make their own history, but they do not make it exactly as they please; the traditions of all the dead generations weigh like a nightmare on the brains of the living.”

But let me not dwell excessively on Hejl’s criticisms of me, because he is by and large a sympathetic critic. Let me address his point 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. I have already read some of the evolutionary literature on the origins of language (e.g., Pinker and Bloom 1990; Pinker 1994) and find it fascinating.

Hejl refers to the so-called social intelligence hypothesis – the idea that the tripling of brain size in the course of hominid evolution from *Australopithecus* to anatomically modern humans was due more to the increased need for intelligence to negotiate the *social* environment containing other humans than to the challenges thrown up by the *natural* environment – and notes that if this hypothesis is correct this shows the crucial role of communication in human sociality. Indeed, that is a very sensible conclusion, because the social intelligence hypothesis assumes that a great deal of human communication is devoted not to the communication of correct information, but rather to communicating *disinformation*, or information deliberately designed to deceive rivals. Hejl also makes a convincing argument when he contends that the transition from small hunter-gatherer groups of a few dozen or a few hundred people to societies numbering in the thousands or millions – the transition to ultrasociality – required much more efficient modes of communication to coordinate the complex human activities that ensued.

In his discussion of the media of communication Hejl makes a distinction between 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 (1986; Goody and Watt 1963), which shows that dramatic social consequences followed from the invention of writing. The general point is undoubtedly correct. Indeed, in recent research I have undertaken on the long-term evolution of religion (Sanderson and Roberts 2008), 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* written by Neil Postman (1979). 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 one of the leading figures in this field. Postman defined media ecology as the study of information environments, and went on to say that it “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”(1979, p. 186). Postman was highly critical of certain pedagogic practices that had begun to enter higher education, especially the use of television videos and film 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 massively exposed to 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 or film watching are easy and highly passive, so young people quickly gravitate to them 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 in his paper. The question is, with the massive technological changes that are now occurring, what is waiting for us several decades down the road?

Finally, let me comment on Hejl’s research on Hollywood and Bollywood films. He and his collaborators obtained two major findings: There are striking similarities in the content of Hollywood and Bollywood films (consistent themes of danger, mate selection, acquiring status and resources, group conflict, and revenge), but there are also differences based on differences between Indian and Western cultures. The similar content of films in both industries shows that there are a fairly small number of themes, deriving from human nature, that will hold people’s attention, whereas the differences have to be explained in

terms of environmental or socioecological contingencies. For example, mate selection and kinship were more important in Bollywood films than in Hollywood films, which makes perfect sense in terms of India's much lower level of technological and economic development and the greater persistence of extended kin networks (and their control over mate choice) in that society. These results seem to me entirely consistent with the predictions that would be made by DCT.

### 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, *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 *more intense* in these types of societies than in others.

Since Hallpike's book title refers to social evolutionary principles, it would be good to know what some of these principles are. One of them, of course, is the “survival of the mediocre” principle encountered above. A second and perhaps more crucial principle might be called the “future possibilities principle.” According to Hallpike, the most important thing in evolution is not the immediate usefulness of a social trait or pattern, but rather its long-run evolutionary potential – whether or not it provides a basis for the development of patterns at a later point. But this principle could not possibly work for organic evolution, since what matters is what any organism is doing in the very here and now vis-à-vis other organisms. A trait that would help an organism do better later on but confer no advantage at the present time will not persist for very long. It will be strongly selected against. And much the same is true with respect to social evolution. If the social arrangements that people develop are not advantageous in the struggle for survival and reproductive success, they will not last long enough to provide a foundation for future possibilities. This is rather elementary.

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; 1999, 317-32), and, truth be told, this is the most difficult of all social phenomena to explain from a biomaterialist or an

ecomaterialist perspective. Is the brain “wired for science” in the same sense that it seems to be wired to seek status, wealth, desirable mates, and so on? Some anthropologists think so, most notably Scott Atran (1990). Atran shows that folkbiologies, especially folktaxonomies, correspond remarkably closely in principle to what was developed under the Linnaean system of more recent times. People have an inherent sense of how to classify plants and animals accurately, Atran contends, because of the obvious survival advantage of understanding these crucial aspects of their environment. This seems quite sensible, but classifying species is a far cry from doing science, which means constructing *theories* intended to *explain features of the world* and then *designing empirical tests of those theories*. As Hallpike points out, even the legendary ancient Greeks, including Aristotle, were not very good at those things by modern standards. Science, in other words, is an extremely difficult undertaking; it not only requires unusually sophisticated minds, but sophisticated minds working collectively, which is why it has developed so slowly and fitfully until recent centuries. On this point Hallpike and I would be in strong agreement, I think.

And there are also certain biases that the mind seems to have that hinder scientific understanding. Two of these biases are *teleology* and *essentialism*. People everywhere in all types of societies have a very strong tendency to think that “everything that happens happens for a purpose,” and by purpose they seem to mean some sort of grand cosmic purpose. Teleology and essentialism are closely related. Essentialism is the notion that everything has some inner nature than can explain why it is what it is and why it does what it does. The teleology and essentialism biases are related to a third bias, that of *agency*: Something happens because someone or something (perhaps a god or spirit) wants it to happen and therefore causes it to happen. All of these cognitive biases have been severe impediments to doing real science. The teleological bias was first overcome in the physical sciences, when it was discovered that physical bodies have external causes. And the essentialist bias was a huge barrier in the nineteenth century to the acceptance of natural selection as the mechanism of evolution. Natural selection was rejected as the mechanism even by nearly all of those who accepted evolution as a factual occurrence (Mayr 1991), and it was rejected because it destroyed the idea of purpose in the universe, an idea to which even the most sophisticated thinkers of the day were deeply attached. Even today there is great resistance among the wider nonscientific community to ideas that are seen as undermining purpose, and teleology is even staging a comeback in physics and cosmology in the form of the *anthropic cosmological principle*: The idea that the universe is the way it is because we are here to observe it and thus it exists in order to lead to intelligent life as its ultimate purpose (Barrow and Tipler 1986). In this way of thinking, the universe had a purpose from the beginning of its formation or “creation,” which is to lead to us.

Hallpike wishes to see primitive societies as dominated by Piaget’s preoperational thinking because the environments of these societies are cognitively undemanding. Only later did humans make the transition to Piaget’s cognitive stages of concrete and formal operations. But I am suspicious of this argument for at least two reasons. First, many of the environments in which bands and tribes are found can be extremely cognitively demanding – the difficulties of finding food and building shelter in especially cold or dry environments, for example, surely has to call forth some serious thinking. Moreover, it has been discovered that most of the members of modern industrial societies are still thinking much of the time at the preoperational level, and only a small handful really engage in formally operational thinking much of the time. Hallpike’s Piagetian analysis of the development of human cognition harkens back to the completely discredited ideas of Lévy-Bruhl (1923).

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 and would not do so. 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 metal 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 thought, whose smart 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 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. That context was one in which I was arguing against the postmodernists, who claim that science is a largely social or political process in which empirical evidence plays little or no 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 CE 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 payoffs. 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.

### **W. G. Runciman**

It is instructive to discuss Hallpike and Runciman together because they have radically opposing perspectives and have recently engaged in some heated disputations (Hallpike 1999; 2000; Runciman 1999; 2001). Hallpike, of course, rejects entirely the very project in which Runciman has been engaged for over twenty years, that of a "variation-and-selective-retention" model of social evolution. He contends that such basic Darwinian concepts as mutation, competition, adaptation, and fitness have no relevance to social evolution. I agree with some of this. Hallpike is right that in social evolution there is no unit of social reproduction that is equivalent to the gene, and also that in evolutionary biology the concept of fitness is a statistical concept that has no close parallel in social life. There is also a disanalogy regarding the source of variation, which in evolutionary biology is random genetic mutation but in social life is much more likely to be purposive invention. Hallpike is quite critical of the concept of memes as well, seeing it as a nebulous concept that adds little or nothing to our understanding, and that is part of "a futile search for the elementary units of culture." Moreover, memes cannot possibly be replicators in the sense of genes, despite Dawkins's (1976) original intent that they be so. However, I would part company with Hallpike regarding the usefulness of the concepts of competition and adaptation, quite obviously, since these are critical elements of DCT.

Let me hasten to add, however, that 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). There are several reasons: Runciman dares to be an evolutionist in these fairly antievolutionary days; he is an elegant writer in a discipline known for mediocre and sometimes dismal writing; he brings to the table a wide 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. Social life is largely a matter of individuals and groups competing to realize their interests even if they are not fully consciously aware of those interests. He is an adaptationist, is against any teleological

conception of social evolution, explicitly recognizes retrogression and societal collapse, and so on. There is in Runciman much to like – very much indeed.

But my main difficulty with Runciman is, apart from the difficulties noted above, that he provides us with precious little in the way of direct evidence in support of many of his specific explanations, correct though they may very well be. He is a sort of teller of just-so stories. When societies change it is because the old practices are no longer satisfactory (adaptive) to the incumbents of dominant social roles; and when they do not change this indicates that the existing practices are adequate to the task. Perhaps so, but this is dangerously close to tautology. In some instances Runciman is more convincing than others, but in almost no case does he really marshal the evidence needed to support his argument. Or perhaps it would be more accurate to say that he uses evidence in the manner of the typical historian – he narrates cases and you either accept what he says as compelling or you don't.

Now what of his essay in the present volume? Unfortunately, it does not help his case and in some respects only makes matters worse. Runciman has now dug in his heels, no doubt in response to prior criticisms, and insists that “to deny that there is a process of heritable variation and competitive selection at work which is continuous with natural selection is by now not so much skeptical as perverse.” Moreover, Runciman admits in response to criticism that “it is true that we do not know what the units of cultural transmission are in the way that we know what the units of transmission are,” and goes on to say that we can continue to use the term “memes” as “bundles of information” *whatever those items or bundles of information actually are* [!], and that we really do not need to linger too long over the far from complete analogy between genes and memes. The term meme can also be used as a “term of convenience” because it allows us to avoid the problems of a “cumbruous periphrasis” and an “imprecise preexisting usage.” Now, I openly confess that I when I read the former of these phrases I had to check it out in my *Webster's Encyclopedic Unabridged Dictionary of the English Language*. Cumbruous, I guessed correctly, means cumbersome, whereas a periphrasis is a circumlocution, or an unnecessarily roundabout or long form of expression. Using the term meme therefore helps us avoid awkward forms of writing in which we are using too many words to get to the point.

I appreciate Runciman's intent, which is noble, but in this case more problems are created than solved. A meme is apparently “anything that the members of a society or some segment of it regularly think or do.” So polygynous marriage would be a meme if it is regarded as a good thing and practiced when circumstances permit. Not much harm there. But what then would we do with differential investment in sons and daughters among the Mukogodo (is “differential investment in sons and daughters” a cumbruous periphrasis, I wonder?). When the Mukogodo are asked whether they favor sons or daughters, they almost unanimously insist that they favor sons. But their primary ethnographer, Lee Cronk (2004), found that the Mukogodo do not behave in accordance with their stated preference. He found irrefutable evidence that, in fact, the Mukogodo actually favor daughters. What is the meme here? Actually, there have to be two memes, the meme as stated preference and the meme as actual behavior. What then is gained by using the term meme? Nothing. Then why not simply use words or phrases that sociologists and anthropologists have been using since the beginning of their disciplines, such as ideas, verbal behavior, actual behavior, social practices, and so on?<sup>1</sup> If the term meme covers too much territory, conflates things that need to be kept apart, and accomplishes nothing but confusion, then why use it?<sup>2</sup> I am totally unconvinced. Moreover, Dawkins coined this term in 1976, I am sure, to rhyme with gene, and he insisted that memes were replicators much as genes are. However, over thirty years

later we have still learned absolutely nothing about the unique mode of replication memes supposedly have.

There are two other main arguments in Runciman's paper. 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, 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 as "rule complexes" 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 10,000-12,000 BP marked the beginnings of the first transition to agricultural (horticultural) societies, and these early cultivating societies were in most instances 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 Runciman's roles and institutions, and thus there is a strong presumptive case that earlier hunter-gatherers would have had them too.

In any event, I am no happier with the concepts of roles and institutions than I am with memes. These are old-fashioned sociological concepts that reached their apex of development (or was it their nadir?) when sociology was dominated by Parsonian functionalism and its rule- and norm-obsessed way of thinking about social life. For DCT, rules are for the most part distillations or crystallizations of social practices. In my view George Homans (1984) got it mostly right when he said that norms are ideas in people's heads that emerge on the basis of what people are already doing. When everyone (or nearly everyone) is engaged in Behavior X, people look around and notice that, and this observed statistical regularity in behavior gets distilled or crystallized in people's heads as an "ought": People begin to think that what most of them are doing is what they *should* be doing.

The other principal idea in Runciman's chapter is the notion of *evolutionary dead-ends*. The example he gives is that of 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 almost completely abandoned before any further social evolution could proceed. (China might be considered something of an exception, but it really isn't. Although it has retained its Communist single-party government, the economy was largely converted to a market system beginning in the late 1970s.) This was perhaps the greatest evolutionary dead-end in all of human history.

But let me end on a positive note. Runciman's evolutionary sociology, despite its difficulties, is still much better theoretical sociology than most of what today's sociological

theorists are producing. Runciman is a first-rate scholar and many of his specific ideas are probably on track; even when they may not be correct, they provide a good foundation on which to build slightly different scaffolding. Read Runciman.

### 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 North American and European 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.

### Notes

1. Runciman apparently wants to exclude "social practices" from memes, thus making meme a purely ideational concept, but I include practices because at least a few other meme enthusiasts seem to do so. In any event, since meme is a unit of culture, and since I have always resisted purely ideational definitions of culture, if I were to use the term meme I would include practices as one kind of meme. If Runciman insists that memes have to be units of thought, then the understanding of the Mukogodo case is only made worse, since Mukogodo behavior (clear preference shown to daughters) is obviously highly inconsistent with one of their cultural memes ("we prefer sons"), and the concept of meme produces only negative understanding.
2. Another neologism of Runciman's, which he has coined in order to avoid cumbersome periphrasis, is *systact* (Runciman 1989, 20-24, 97-113). A systact, as it turns out, is any category or group of persons sharing similar roles and interests, such as a social class, a racial or ethnic group, a gender, or an age grade. It may be useful to have such an umbrella term, but systact? Sociology is already awash in pretentious jargon and do we really need more? If sociology had achieved the status of a genuinely successful and highly regarded science, it would do what the most successful and admired scientists, the physicists, do: It would use ordinary language as much as possible. Physicists use such concepts as work, energy, force, and momentum, terms that are widely used in everyday life, but they give these terms precise definition. I suspect there is an inverse correlation between pretentious jargon and actual scientific accomplishment. (The biological sciences and medicine use nonordinary concepts, to be sure, but they have a solid Latin foundation, are easy to understand and even correctly guess the meaning of (e.g., hypoglycemia, hyperthyroidism, antibiotic), and are not pretentious but rather highly informative and useful.)

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