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Evolution, Social Behavior, and Ethics

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Introduction

NOTHING SEEMS LIKELY to influence analyses of the relationship between science and ethics as much as would a significant revision of our view of either science or ethics. Yet refinements of evolutionary theory within biology during the past twenty years seem to me to have provided a compelling new model of culture and human sociality which dramatically alters our interpretations of all human activities, including both science and ethics. This model has been developed elsewhere and the findings responsible for it described;¹ here I shall only summarize the attributes of the model, and the way in which it departs from earlier views, before discussing its apparent meaning for the current confrontation, or interaction, between science and ethics.

Culture theorists, philosophers, and historians have always wrestled with two related problems in their efforts to develop grand theories, the relationship between individual and group interests and the identification of function. Although various combinations of interpretations have been tried, the only one apparently consistent with modern evolutionary theory has not. Function, as *raison d'être*, has characteristically been divided

into proximate and ultimate forms. Proximate forms, such as satisfaction, pleasure, happiness, and avoidance of their alternatives, are more likely to be visualized as significant at the individual level; partly for this reason, psychology has developed with an emphasis on the individual. Some functions, such as efficiency of organization or operation, which could be regarded as either proximate or ultimate, are usually interpreted as group-level phenomena because social theorists have not commonly been concerned with genetic or physiological efficiency; this is especially true in anthropology, where explaining culture has been a principal focus; it is not so true in psychology, as Freudian theory indicates. Survival has often been regarded as the ultimate function and interpreted at either individual or group levels, although, because of frequent conflicts of interest, it obviously cannot always be interpreted at both levels.

The model recently developed within modern biology involves three assumptions: (a) proximate functions are never their own reasons for existence, but, in evolutionary terms, exist to serve ultimate function, hence, take their particular forms because of their contribution to ultimate function;² (b) ultimate function is invariably reproduction of the genetic materials because (i) no alternative to natural selection (differential reproduction of genetic alternatives) exists to explain the history of form and function in living things; (ii) natural selection proves both logically and empirically necessary and sufficient (in its present theoretical form); and (c) effects of natural selection on function are realized almost entirely, if not entirely, at the individual level or lower.³

As discussed later, and elsewhere,⁴ this model returns the concept of function to survival, but to survival of genes (or polygenes, supergenes, and chromosomes)⁵—not of individuals (which clearly have not evolved to survive), and not of groups (which, however, give more of an illusion than do individuals of having the function of facilitating only their own survival).⁶ Genes evidently have promoted their survival through effects leading to finiteness of individual lifetimes,⁷ and, in social species, sometimes to indefinite prolongations of identifiable social groups. That this conclusion is discomfiting to an organism with consciousness only at the individual level is not an appropriate reason for denying it.

The main element of the new view of sociality I have just described is clearly a return to a kind of individualistic and utilitarian view of history. It is, however, a view of individualism or utilitarianism never before held or advocated in efforts to explain human behavior and culture. Previous explanations of culture and human striving, as the outcome of individuals seeking to realize "their own best interests" or to "maximize their outcomes"⁸ have never explicitly identified these "interests" and "outcomes," or else they have defined them in terms of either (a) proximate rewards, like happiness or (b) survival.⁹ The view from evolutionary biology identifies "own best interest" in terms of reproductive success (or, in our current, novel, rapidly changing environment, surrogates of reproductive success), and hypothesizes that hedonistic rewards relate solely to such returns if they are interpreted in terms of the environments of history.¹⁰ Growth, development, aid to reciprocating friends, and the acquisition of power are seen as the accumulation of resources; assistance to offspring and other relatives represents the redistribution of accumulated resources. Culture is seen as the cumulative effect of this "inclusive-fitness-maximizing" behavior¹¹ by all of the individuals who have lived during history. Culture, then, is the result of endless compromises, conflicts, power interactions, cooperative events, and formation and dissolution of coalitions. According to this view, there would be no single "function" of culture as a whole, as some anthropologists have supposed; nor should we expect even a few identifiable functions.¹² Aside from those rare issues on which everyone agrees because everyone is aware (or behaves as though aware) that all of our interests are the same, I am suggesting that culture is an incidental effect of our separate, conflicting strivings in which success tends to be (in historical or evolutionary terms is invariably) measured in relative not absolute terms; that culture is the environment into which we are born and according to which we must achieve our goals; that cultural inertia, giving it the quality or appearance of being something greater than the humans responsible for it, is largely owing to the simultaneous effort of every one of us to use and manipulate it to serve our own interests, to keep everyone else from so using it when their efforts conflict with our own, and to extend (temporally and otherwise) our ability to redistribute resources according to our own interests.

At least eight major issues have clouded efforts to develop culture theory in this direction. First, reproduction involves altruism to other individuals, and in humans a bewildering array of genetic relatives of varying degree and varying needs is socially available to each individual. As a result, the altruism of nepotism gives an illusion of group function. On the other hand, the complexity and accuracy of knowledge of *differences* among kin support the idea that nepotism has been a major avenue of reproduction by *individuals*.¹³

Second, effects of the peculiarly human mode of reproductive striving, through group-living, persist as culture and technology long past individual lifetimes. This also gives an illusion of group function. As suggested earlier, much of this effect is also recognizable as a result of the striving of individuals to provide for relatives and descendants as far as possible into the future.

Third, as a part of group-living we are constantly forming and dissolving coalitions, or subgroups, of individuals who temporarily have common interests. Although this too gives an illusion of group function, the mere fact that coalitions are temporary and shifting indicates otherwise.

Fourth, we have turned our group-living and group-competitive behaviors to the development of nations within which reciprocal behaviors apparently derived historically from nepotistic interactions within clans and tribes form the social cement,¹⁴ and within which extensive nepotism is both downplayed by law and thwarted by geographic mobility of individuals and families. This effect creates circumstances in which the altruism of nepotism is "misdirected," and others in which altruism with the function of maintaining acceptability in the group, or gaining status, and, hence, access to resources, again creates the illusion of group function.

Fifth, the accelerating rate of cultural innovation has caused massive novelty in our environment,¹⁵ thwarting analyses based on function, except as interpreted in terms of past environments. Although this situation may lead to frequent errors, analyses of human behavior are still most likely to be accurate if they are developed from an understanding of the effects of a history of natural selection, with allowances for the particular kinds of environmental changes known or suspected to have occurred.¹⁶

Sixth, it has proved exceedingly difficult to trace the pathways

between gene expressions and complex behaviors, so that the relationship of the latter to genetic reproduction, and therefore the importance of interactions among relatives, is still viewed with scepticism. The significance of this scepticism is much reduced by the knowledge that (a) the complete ontogeny is known for no behavior in any organism and (b) complex and accurate predictions about behavior have been made on a wide scale from a knowledge of selection alone.

Seventh, proximate mechanisms have not previously been hypothesized whereby altruistic nepotism and the altruism of reciprocity could be directed appropriately so as to maximize inclusive fitness (the reproduction of one's genes) and yet be commensurate with what is known about the plasticity of human behavior and theories of learning. At least in terms of reasonable theory I believe that this problem has been solved.¹⁷

Eighth, humans have found it difficult to evaluate with disinterest the suggestion that their evolutionary background has primed them to behave as the reproductive machinery of the genes. This fact seems to result in part from a tendency to self-deception which has its advantages in an extraordinary ability by humans to detect deliberate deception in others.¹⁸

Despite these difficulties, the theory that culture is no more or less than the outcome of inclusive-fitness-maximizing behavior by all of the individuals who have lived during history appears capable of surmounting the difficulties encountered by the older theories.¹⁹ Although efforts have been made to describe this view of culture as "Hobbesian" or "utilitarian" in ways rendering it out of date, or as not different from other approaches that have already been tried and discarded, it is in fact distinct from any view previously generated. Although efforts have also been made to associate it with some particular ideology, such as social Darwinism, its testing is a procedure in natural history; regardless of what may be said, by either its proponents or its opponents, it is not properly ideological in nature.²⁰

Science as a Social Enterprise

If my arguments to this point are acceptable, then science may be considered as a particular kind of activity of individuals,

sometimes operating in groups, with certain unique characteristics and consequences. Its central attribute is its unusual degree of self-correction, induced by the criterion of repeatability of results. This aspect of scientific method, theoretically, at least, forces the practitioners of science to explain fully the methods by which they make their discoveries and reach their conclusion. The resulting tendency for scientific findings continually to approach correctness in explanation gives an illusion that scientists are devoted to a search for truth, hence, are somehow unusually humble and altruistic. Instead, the system of investigation called science, however it may have begun, *forces* its practitioners to report their methods as well as their results, or risk being exposed as unscientific and drummed out of their profession.²¹ Scientists compete by striving to acquire authorship for as many of the best ideas as possible. This competition includes identifying and publishing the errors of others. As nearly all scientists are aware, the slightest taint of deliberate falsification of results or plagiarism is often enough to damage a career permanently, and may be vastly more significant than mere incompetence. I speculate that science, as a method of finding out about the universe, began as a consequence of competition among the ancients to prove their ability to comprehend cause and effect and to meet the challenges of one another for preeminence in this enterprise and the prestige and leadership that went with it. The requirement of repeatability is what distinguishes science, indeed, diametrically opposes it to dominance or prestige by virtue of claims of divine revelation or knowledge conferred by deities—although the two kinds of effort may exist for exactly the same reasons.

To understand why the public tolerates and supports scientists—even, sometimes, regarding science as the most prestigious of all enterprises—we must turn to the products or results of scientific investigation. These results are represented not only by all of the products of technology but by innumerable changes of attitude toward ourselves and our environment as a result of new knowledge. In some sense, essentially all of the reasons for societal affluence, and many of the reasons for our ability to achieve a modicum of serenity in the face of the uncertainties, complexities, and competitiveness engendered by the reasons for affluence, are seen as products of science. So, I suggest, science

is supported for the same reason that copyright and patent laws are maintained to allow inventors to profit from their inventions. We evidently believe, individually and collectively (or we behave as though we believe), that the discoveries made by scientists are likely to benefit all of us sufficiently to make their support worthwhile. This view of science also contributes to the impression that scientists are humble truthseekers, in no way out to maximize personal gain. The truth, however, is something else, as is suggested by the enormous scale on which scientists are employed directly by organizations that exist for the sole purpose of making profits.

Now we can see that, so long as what scientists discover represents solutions to problems that face *all* humans, the relationship of science to any system of ethics regarded as functional and acceptable at the group level (that is, as helping everyone about equally) is clearly a harmonious one. Even a science practiced by individual scientists who are totally selfish in their reasons for doing it would tend to help the group involved, except when a discovery gave a scientist such personal power as to allow him to seek his own ends in conflict with those of everyone else or the group as a whole; or to the degree that scientists themselves form subgroups with common interests among themselves and different from those of others.

Scientists employed by subgroups, such as corporations, seeking their own profit rather than that either of the group (nation?) as a whole or of others in the society, are somewhat removed from the continual scrutiny and approval of the collective of individuals called the public. Given the view of science I have just presented, such scientists may be expected to develop and pursue lines of investigation that do not represent the interests of the group as a whole, or even of the majority of individuals within it. Technological and other products of science which create serious problems for society, I suggest, may frequently be expected to come from these kinds of scientific enterprises. Accordingly, in this particular realm, many problems in the relationship of science and ethics may be expected to occur. For example, what is the net value to society as a whole of new herbicides, insecticides, patent medicines, cosmetics, and particular trends in automobiles, farm, and industrial machinery, com-

puters, appliances, office equipment, and so on? Trends in such products may frequently proceed in directions catering to individual needs, desires, whims, and weaknesses, such as susceptibility to novelty, desires to prolong the phenotype at whatever cost (even, in the eyes of relatives, using all of the resources one has saved during a lifetime), or desires to reserve, at great cost, the opportunity to reproduce far into the future (for example, through sperm banks). Given such propensities, and the readiness of people to accept placebos, some of the directions taken by corporation-dominated science are bound to be detrimental, not merely to most of the populace but to all users, while nevertheless profitable to their creators and manufacturers, and to the stockholders.

These assertions, of course, do not speak to the question of what proportion of the scientific discoveries useful to all members of the group are also likely to come from scientists employed by profit-seeking subgroups because of the profit incentive. Also, although government scientists, who may create weaponry raising the most serious of all ethical questions, may seem to be excluded, in the sense involved here they may also be regarded as employed by subgroups, since such weaponry is presumably developed explicitly for employment against the members of other similar groups (nations) when the interests of the different groups are sufficiently in conflict in the eyes of their leaders.

The above view of science is entirely compatible with the general theory of culture and sociality described earlier. It does not appear to me to be counterintuitive, though it is surely not the most widely held view of science.²² I believe that it tends to resolve certain paradoxes in generally held views of science.

The next question is: What does the new view of human sociality mean for our understanding of ethics, and, in turn, what does the view of ethics so generated mean for the relationship of science and ethics?

The Biological Basis of Ethics

Consistent with the above arguments I hypothesize that ethical questions, and the study of morality or concepts of justice and right and wrong, derive solely from the existence of conflicts of

interest. In social terms there are three categories of such conflict: (a) individual versus individual, (b) group versus group, and (c) individual versus group. In biological terms two kinds of returns are involved in judging conflicts of interest: (a) those coming to Ego's phenotype and (b) those coming to Ego's genotype, through the success of various kinds of relatives including offspring, and representing reproductive success. In evolutionary terms, all returns are of the second kind, and, as theories of senescence and reproductive effort indicate,²³ our efforts to garner the first kind of returns are expected to be shaped so as to maximize the second kind; there is no other reason for lifetimes having evolved to be finite.

The recent exacerbation of ethical questions has been caused by an accelerating tendency for discoveries from science to cause new kinds of conflict and to cause conflict in new contexts. This situation has caused us to reexamine the basis for ethical norms, seeking generalizations which may assist us in extrapolating to solve the new problems. The effort is actually urgent, since the difference between the processes of organic and cultural evolution are such that the latter continues to accelerate in relation to the former, so that we may be assured that new ethical questions will be generated at ever-increasing rates in the future.²⁴

The two major contributions that evolutionary biology may be able to make to this problem are, first, to justify and promote the conscious realization that it is conflicts of interest concentrated at the individual level which lead to ethical questions, and, second, to help identify the nature and intensity of the conflicts of interest involved in specific cases. Undoubtedly the most dramatic and unnerving aspect of these contributions is the argument, or realization, that all conflicts of interest among individuals, in historical (evolutionary) terms, resolve to conflicts over the differential reproduction of genetic units, hence, that conflicts of interest exist solely because of genetic differences among individuals, or histories of genetic differences among individuals interacting in particular fashions. I emphasize that the major barrier to acceptance of this argument—absence of theories about proximate (physiological and ontogenetic) mechanisms acceptable in light of learning theory and the modifiability of human behavior—has been at least partly eliminated.²⁵

The above arguments indicate that analyses of ethics, either from a descriptive approach or as an interpretation of the sources of normative ethics in the past, must be phrased from the individual's viewpoint and must bear on the problem of how the individual is most likely to maximize its inclusive fitness. This is true even if most concepts of right and wrong, most laws, norms, traditions, and reasons for courses of action, were established in generations past and are resistant to change. The inertia of culture does not remove the individual's historical reasons and tendencies to strive, it only restricts or alters the manner of striving and the degree to which the ends involved are likely to be achieved.

In the individual's terms, then, a statement by a biologically knowledgeable investigator about the normative ethics of *yesterday*, applicable in any cultural situation, might come out as follows²⁶: I "should" treat others so as to maximize my inclusive fitness. My treatment of relatives "should" be more altruistic than my treatment of nonrelatives (that is, altruism to kin should be more likely than altruism to nonkin in situations in which phenotypic returns are unlikely). My treatment of both relatives and nonrelatives "should" be developed in terms of (a) effects of my actions on the reproduction of relatives (including offspring), hence, the reproduction of my genes; (b) effects of my actions on how I will be treated by those directly affected by my actions (how will interactants treat me subsequent to my actions toward them?); (c) effects of my actions on how my relatives will be treated by those affected by my actions; and (d) effects of my actions on how I will be treated by those only observing my actions, and either (i) likely to be interacting with me subsequently or (ii) likely to be affected by the success or failure of my actions because of the observation, and, hence, acceptance or rejection of them by still others. It is particularly perplexing that we must investigate the extent to which our behavior supports this hypothesis under the realization that, if such goals do guide our behavior, they are nevertheless not consciously perceived, and, if the hypothesis is correct, this means, paradoxically, that we are evolved to reject these goals whenever we are asked to evaluate them consciously. This does not mean that we *must* reject them, but that individuals not aware of all this are expected to behave as if these were their goals even if denying it is so,

and that to convince them of self-deception may be difficult, and will be most difficult for the precise activities about which they deceive themselves, for the same reason that they do so. The question is testable: Do we or do we not behave as predicted, whether we think so or not, when we are not yet aware of the predictions? It is the same kind of question anthropologists always must ask when they undertake to analyze the structure of a culture alien to their own.

By these arguments the complexity of ethical issues derives not from their general basis but from the diversity and complexity of sources of conflict, and of the means by which they are altered.

We are led to a division of normative ethics into those of the past—before development of the realization that genetic interests underlie conflicts of interest—and those of the future, following conscious understanding of such arguments. It is crucial that this distinction be recognized; otherwise, what I have said above will be interpreted erroneously as naively deterministic, with new knowledge of the significance of history not acknowledged as having effects on the future of human sociality or the determination of ethical procedures.²⁷ I appreciate the way Albert Rosenfeld put this particular point:

... the individual who militantly seeks to have the quest for knowledge brought to a halt is often the same individual who is outraged by the sociobiological suggestion that we are more controlled by our genes than we realize. *We are* more controlled by our genes than we have realized [This is a reasonable assertion, since not too many years ago we hadn't even heard about their existence]; therefore, the more we discover about the mechanisms of genetic control, the better equipped we will be to escape these controls, through our enhanced awareness, to transcend them so that we may, for the first time in our history, work for ourselves, instead of for our genes, exercise truly free will and free choice, give free reign to our minds and spirits, attain something close to our full humanhood.²⁸

Why should biologists, social scientists, philosophers, and historians find it so difficult, or distasteful, to accept what Rosenfeld has grasped so well? I am inclined to suggest that what is involved are the reasons for cultural inertia and the nature of science, already mentioned here. Leaving aside the obvious virtue of some conservatism about novelty, the emotionality of re-

sponses to this issue suggests to me that those of us who make our living in this subcultural arena are reluctant to accept new paradigms, which, if they succeed, represent someone else changing the rules in the middle of our game; we have learned how to use the system—in our own subarenas of science and humanism—to meet our own ends, and we resent the suggestion that we must in any sense start all over again.

Perhaps as well it has not for a long time been profitable for social scientists to entertain truly novel theories, partly because of the supposed relationship between new ways of viewing human activities and the potential for misusing them. Thus, someone has said that a natural scientist is remembered for his best ideas, a social scientist for his worst. Perhaps the new paradigm in evolutionary biology will be first absorbed into fields like economics, and by laymen, who are curious but lack the vested interests and other inhibitory baggage of much of academia.²⁹

Justice, Happiness, and Keeping Up with the Joneses

Rawls developed the idea that justice correlates with happiness, and that happiness may be identified as follows: "A person is happy when he is in the way of a successful execution (more or less) of a rational plan of life drawn up under (more or less) favorable conditions, and he is reasonably confident that his intentions can be carried through . . . adding the rider that if he is mistaken or deluded, then by contingency and coincidence nothing transpires to disabuse him of his misconceptions."³⁰

But Rawls fails to consider fully how individuals decide upon particular courses of action, thus, why there is any likelihood at all of selecting a plan of life that is *not* likely to be carried through, particularly in an affluent society where scarcely anyone is actually in danger of starving, freezing, or otherwise dying prematurely because of inability to secure necessary resources. In others words, he has failed to explain why people strive, and what he has left out seems to be the crux of the problem, and the source of the conflicts of interest that lead to ethical questions. I think we can be certain that, even in affluent societies—and, I would venture, *especially* in some such societies—there will be much evidence of unhappiness. Why should this be so?

It should be so because, again in historical terms, success is only measurable in relative terms. We set our goals and determine our plans of life in terms of what we observe others about us achieving; such goals are irrational, or likely to be inaccessible and thus to lead to unhappiness, when different individuals (a) strive from different resource bases, and fail to take this into account; (b) fail to consider the different sorts of obstacles placed in their ways (because of race, sex, physical or mental handicaps, or other bases for discrimination); (c) fail to consider trends in society that may eliminate possibilities open to others; or (d) fail to consider the extent to which achievements of others have required use of excessive power, influence, chicanery, or injustice against others (and the attendant risks). I think we can predict that unhappiness as a consequence of unlikely or irrational personal goals is likely to be most prevalent in societies that are hierarchically structured, so that lofty goals may be developed from observations of the success of others, and yet so constituted as to generate severe inequalities of opportunity so that the perceived goals are inaccessible for what are logically interpreted as unjust reasons.

In natural selection the likelihood of a genetic element persisting depends entirely on its rate of change in frequency *in relation* to its alternative; changes in absolute numbers are irrelevant. Among the attributes of living creatures, whatever can be shown to have resulted from the action of natural selection may be expected to bear this same relationship to its alternatives. This means that we should not be surprised to discover that the behavioral striving of individual humans during history has been explicitly formed in terms of relative success in reproductive competition. As I have noted elsewhere³¹ this is the reason why justice is necessarily incomplete, why happiness is not a commodity easy to make universal, and why ethical questions continue to plague us, and can even become more severe when everything else seems to be going well.

Right and Wrong

Interpreting the concepts of right and wrong in terms of conflicts of interest is a difficult task. First, there is an implication of

absoluteness about right and wrong which gives an illusion of group function to their invocation. This flavor is promoted by legislative bodies and law; by authority in the form of parents, organized religion, and other sources of power, influence, and leadership; by persistence of meanings across generations; and even by our use of the terms right and wrong in the context of correctness and incorrectness about decisions or answers, or understanding of factual matters (e.g., the *right* or *wrong* distance, direction, number, or answer; a *right* line is a straight line; the *right* hand is the correct one; *right* now means precisely at this time; and so on).

Yet all of the arguments I have presented so far suggest that this implication of absoluteness and group function has some significance other than actual unanimity of opinion or equality of return to all individuals. What is this significance?

Parents begin instilling the ideas of right and wrong in their children, and this is probably the normal origin of the concepts for most individuals. Initially, at least, right and wrong are for children whatever their parents say is right and wrong. What, though, are usual concepts of right and wrong in parents' views of their children's behavior? One might suppose that children are simply taught by their parents never to deceive, always to tell the truth, the whole truth, and nothing but the truth; therefore, that children are taught always to be altruistic toward others, to be certain that justice is afforded all those with whom they interact, and that their own interests are secondary to those of others or of the members of the group to which they belong.

Alas, it cannot be true. As we all know very well, children so taught, who also obeyed their parents' teaching faithfully, could not be successful, at least in this society; whatever they gained personally would immediately be lost. They would be the *rubes* of society, of whom advantage would be taken at every turn.³²

I suggest something so different that it may at first sound pernicious: that parents actually teach their children how to "cheat" without getting caught. That is, that parents teach their children what is "right" and "wrong" behavior in the eyes of others, and what truth-telling and forthright behavior actually are, so that from this base of understanding children will know how to function successfully in a world in which some deceptions are sometimes profitable, some unforgivable, and hence expensive,

and some are difficult to detect, others easy. I suggest that parents are more likely to punish children for (a) cheating close relatives, (b) cheating friends with much to offer the family in a continuing reciprocal interaction, or (c) cheating in an obvious, bungling fashion, sure to be detected, than they are to punish them for simply cheating (I am using the word "cheating" here in a very general way, referring to any kind of social deception or taking of advantage.) In other words, I suggest that the concepts of right and wrong are instilled into children in such fashion as to guide them toward inclusive-fitness-maximizing behavior in the particular societies and sub-societies within which they are growing up and are likely to live out their lives; that they are taught by parents accustomed to living by these rules; and that the courts and prisons are filled with individuals whose teachers failed, for one reason or another, to impart just these concepts of right and wrong.³³

The reasons that the concepts of right and wrong assume an appearance of absoluteness and group-level uniformity of application, then, are that (a) on some issues there actually is virtual unanimity of opinion, especially when dire external threats exist, as during wartime, and (b) it is a major social strategy to assemble as a coalition those who agree, or who can be persuaded to behave as though they agree, and then promote the apparent agreement of the subgroup as gospel. On these accounts relatively few ethical questions actually *seem* to involve disagreements between *individuals*: In one fashion or another one or both individuals are likely to have made their grievance appear to be that of a group. This is relatively easy to accomplish if the presumed offender constitutes a potential threat to others not directly involved. We subscribe to laws against acts like murder, rape, robbery, and usury not so much because strangers are victims as because we have assessed, consciously or unconsciously, the probability that we or those on whom we depend, from whom we expect to receive assistance or resources, or through whom we expect to achieve reproductive success, may sometimes be in a position similar to that of the victim.

In this light one may ask about the source of the apparent recent rise of attention to issues like child abuse, rape, and the rights of minorities, women, and the mentally and physically handicapped. I suggest that, as individuals, we regard ourselves

as more vulnerable in the modern, urban, technological, socially impersonal environment, in which we are increasingly surrounded by strangers, and in which bureaucracy, weaponry, and medical knowledge of new gadgetry and substances affecting the functioning of the human body and mind seem to place us increasingly at the mercy of others. I speculate that the recent rise of interest in the rights of even nonhuman organisms represents an extension of the same trend—an effort to preserve our own rights, before they are directly threatened, by singling out others whose rights are directly threatened and using their situation to develop the social machinery to protect ourselves.³⁴

A Concluding Remark

I have been asked by the editor to discuss briefly the limitations of the approach I have attempted here. First, I would reiterate my opinion that evolutionary understanding (therefore, science) has little to contribute to the *identification* of goals in systems of ethics and morality.³⁵ Second, in regard to the analysis of human sociality—the “natural history” of activities like science and the formation and maintenance of systems of human behavior—I am willing to risk seeming unduly optimistic in supposing that evolutionary understanding represents *the central key*. Beyond this, I am impressed with the degree to which the conclusions of authors totally outside evolution seem to converge on those derived from modern evolutionary approaches. Thus, I agree with Friedmann that “The only general conclusion to be drawn is that, in any society that preserves a modicum of individual responsibility, there is a tension between individual ethics and social morality on the one part, and social morality and the legal order on the other part. How much these three spheres of normative order influence and modify each other is a question that cannot be answered in absolute terms.”³⁶

I believe that an evolutionary approach leads us to the same conclusion, but I also believe that it tells us, better than any other approach, why Friedmann’s conclusion is reasonable and what are the likely degrees and patterns of expression of the interactions he discusses.

Mankind's self-interpretation, its conception of itself, its essence, and its destiny, is not without influence on what it then is.³⁷

NOTES

1. See R. D. Alexander, "Natural Selection and the Analysis of Human Sociality," in *The Changing Scenes in the Natural Sciences, 1776-1976*, ed. C. E. Goulden, Philadelphia Academy of Natural Sciences Special Publication 12 (1977):283-337. In this paper I made a special effort to trace the sequence of changes in thinking responsible for the current model, because it seemed to me that much of the existing confusion about "sociobiology" stems from a failure by the authors of books in this area to identify and trace what has actually happened since 1957. For example, E. O. Wilson, in his massive and influential 1975 volume, *Sociobiology: The New Synthesis* (Cambridge, Mass.: Harvard University Press, 1975) defines sociobiology as "the systematic study of the biological basis of all social behavior." But this is not a new kind of study in biology. Moreover, the adjective "biological" when applied to behavior by social scientists all too often means "genetic," and it often is used explicitly to mean "other than social" in efforts to account for the ontogeny of behavior. Further, although Wilson says that "the organism is only DNA's way of making more DNA" and gives credit to W. D. Hamilton's (1964) theory of inclusive-fitness-maximizing (kin selection) (i.e., that *genetic reproduction can be enhanced by helping nondescendant as well as descendant relatives*) in explaining altruism, in my opinion he muddles the question of group selection which is crucial to understanding altruism. To make matters worse he refers to the seminal arguments of George C. Williams in *Adaptation and Natural Selection* (Princeton, N.J.: Princeton University Press, 1966) that *selection is highly unlikely to be effective above the level of the parent and its offspring* (regarded by many as responsible for the entire revolution) as Williams' "fallacy"! In effect, Wilson reintroduced genes into the formula, Genes plus Environment Yield Phenotype (including behavior), without clearly telling the reader why this can now be done satisfactorily; he persists in using the phrase "genetically determined" when referring to human behavior (even, sometimes, without specifying that he is referring to *differences* in behavior); and he gives the impression that the main change is simply a massive accumulation of very relevant data from field studies (later, in "Animal and Human Sociobiology," in *The Changing Scenes in the Natural Sciences 1776-1976*, pp. 273-81, he actually says this). But it is

not true: A massive *refinement of theory* reoriented the study of behavior. It may be difficult for outsiders to understand from accounts like Wilson's what is really new in evolutionary biology, and why it is important. The revolution was caused by the arguments of Williams and Hamilton, italicized above.

2. Gunther Stent, in a critical review of Richard Dawkin's *The Selfish Gene* (Oxford: Oxford University Press, 1976) recently published in the Hastings Center Report, has missed the point, in his distinction between deliberate and nondeliberate altruism, that "intent" is a proximate mechanism; a paradoxical aspect of its molding to contribute to ultimate function is that not all goals are conscious. This is not to suggest that "intent" is a trivial aspect of behavior or that it is not important to distinguish intentional and unintentional altruism and selfishness or kindness and cruelty. After all, intent is a central aspect of the definition of such terms, demonstrating its importance. It is crucial to ask *why* intent is so important to us, when it would seem that *consequences* are what count. The reason, I believe, is that intent has consequences outside the immediate circumstances. I think we *use* intent to enable us to predict about events additional to the ones in which we are immediately involved, just as we use information about whether associates follow the rules or play fair in trivial circumstances, or in games, to determine whether we should interact with them in more serious matters. We actually believe that he who is cruel or kind to others—or to animals, children, and other vulnerable beings—is likely to be cruel or kind to us as well. We are positive toward someone who *intends* to be altruistic for the same reason that we are negative toward someone who *intends* to be cruel: He may do it to us.

Stent also fails to grasp the all-important distinction, in evolutionary arguments, between incidental effects and evolved functions (well explained by Williams in *Adaptation and Natural Selection*). Stent's contention that evolutionary theory is not predictive is serious, not because it is true, but because he echoes a misconception prevalent among those accustomed to determining the nature of scientific predictiveness from theories dealing with nonliving phenomena. Stent, like some others, regards "the concept of 'fitness' [as] the Achilles' heel of Darwinism, for which a substitute has to be found if natural selection is to be upgraded from the status of a retrodictive historical theory to that of a predictive scientific theory." He acknowledges that "fully predictive evolutionary analyses are available" for "bounded situations in which the context can be completely specified," such as "the development of a drug-resistant bacterial strain from a drug-sensitive strain in a culture medium containing that drug." But he does not regard such predictions as adequate to give evolution "full standing as a theory in the natural

sciences.” He believes that what is needed is “some concept formally equivalent to fitness, but descriptive of an intrinsic quality.” He remarks that “Dawkins evidently hit upon selfishness as a substitute for fitness.” Maybe he did. But I would recommend to anyone interested in these questions (including both Dawkins and Stent) that they begin with Darwin, not Dawkins. The following is only one of his several grand challenges to falsification (C. Darwin, *On the Origin of Species. A Facsimile of the First Edition with an Introduction by Ernst Mayr*. (Cambridge, Mass.: Harvard University Press, 1967), p. 201, 1st ed., 1859.

If it could be proved that any part of the structure of any one species has been formed for the exclusive good of another species, it would annihilate my theory, for such could not have been produced through natural selection.

Fitness is a *relative* concept, and it has no significance except in the environment of the organism. There is no such thing as absolute fitness, except in some trivial formulations of population genetics. Unlike nonliving materials, living organisms actively compete, and their phenotypes, by definition, represent evolved capabilities to adjust in the face of particular kinds of competition. This does not mean that some kind of conceptual barrier to predictiveness is inherent in either an evolutionary theory based on fitness or the nature of living organisms. It only means that predictions about the evolution of life will be more difficult than predictions about nonliving phenomena, and that Stent’s notion of an intrinsic quality equivalent to fitness and independent of immediate circumstances is irrelevant. There are no surprises in this for anyone who has truly considered the relative complexities of the aspects of the living and nonliving universe so far available to us.

One invariably predicts in what Stent calls “bounded situations.” There are no theories which predict in the absence of assumptions. The only question is whether or not the predictions are useful in analyzing the phenomena under study. Stent may have developed his notion that evolution is not predictive partly from remarks by prominent evolutionists like Ernst Mayr and George G. Simpson to that effect; I have heard their statements cited to support such arguments. But Mayr and Simpson meant to refer to macroevolution, or the long-term patterning of life forms across geological time, which is essentially nonpredictive because we cannot reconstruct extinct environments in sufficient detail to understand the precise nature of adaptive change by natural selection that occurred prehistorically. This does not mean, however, that we cannot predict very extensively and with great accuracy about life from

the assumption that the traits of extant organisms are the *cumulative results* of the microevolutionary process, guided chiefly by natural selection. The philosopher who wishes to understand how this is done ought to go to the current literature of evolutionary biology and not run the risk of generalizing from what he gratuitously refers to as a "vulgar popularization" by a mere "thirty-six-year-old student of animal behavior, [who] teaches at Oxford, and . . . seems to have published only one sociobiological paper . . ."

3. The reader should beware that, from this point on, when I use the term "function" I mean it in the sense of (b) above—as *evolved* or *adaptive* function, as distinguished from either "incidental effect" (see G.C. Williams, *Adaptation and Natural Selection*) or some assumption of physiological or other function in the individual that is not at least visualized as part of, or a contribution to, the ultimate function of reproductive maximization.

4. R. C. Lewontin, "The Units of Selection," *Annual Review of Ecology and Systematics* 1 (1970): 1–18; G. C. Williams, *Adaptation and Natural Selection*; E. C. Leigh, "How Does Selection Reconcile Individual Advantage with the Good of The Group?" *Proceedings of the National Academy of Sciences* 74 (1977): 4542–4546; R. D. Alexander and G. Borgia, "Group Selection, Altruism, and the Hierarchical Organization of Life," *Annual Review of Ecology and Systematics* 9 (1978): 449–74.

5. There appears to be a feeling in some circles that a failure exists to define gene adequately for its use in discussions of behavioral evolution. The impression one gets is that if definitions were sharpened then implications of unacceptable determinism would disappear (or, alternatively, that evolutionary analyses of behavior would be shown to be inappropriate). Partly this feeling seems to derive from the error of supposing that such definition-sharpening would principally involve precision in describing gene function in terms of physiology or ontogeny—of generalizing about the connections between gene effects and behavior. But the generalization for this direction of definition, adequate for use of the concept of gene or genetic unit in evolutionary analyses, even of behavior, already exists: It is that genes always realize their effects in environments, and their effects change in different environments. I do not imply that all self-proclaimed evolutionists so use it, or use it appropriately or properly. Because the use of gene by evolutionary biologists actually refers principally to heritable or recombining units—or alternatives (and assumes the above physiological-ontogenetic-functional generalization or definition)—to refine the evolutionists' definitions (usages) would chiefly be a matter of describing the sizes and

divisibility of genetic units; this activity would not bear on the question of genetic determinism, as may be supposed. Genetic determinism, in its unacceptable forms, implies that only *some* behaviors are “genetically determined” (E. O. Wilson, “Human Decency is Animal,” *New York Times Magazine*, October 12, 1975, pp. 38–50); that there are reasons for believing that some human social behavior is not learned (E. O. Wilson, “The Social Instinct,” *Bulletin of the American Academy of Arts and Sciences* 30 (1976): 11–25); or that human behavioral variations like homosexual tendencies depend upon genetic variations which exist because of their contribution to homosexual behavior (thus, that the “capacity” for homosexuality exists only in “moderate frequencies” in the human population—E. O. Wilson, “Animal and Human Sociobiology”). In fact, either all human behavior is “genetically determined” or none of it is; unless learning is defined in a fashion dramatically more restrictive than its current usage in the social sciences there is no reasonable alternative to the hypothesis that all human social behaviors are learned; and even if some human behavioral *variations* are genetically determined (i.e., environmental variations are not involved in their expressions), there is, for example, no evidence that the capacity to behave either homosexually or heterosexually, even in rather ordinary environments, is absent in any human.

Biologists who develop general theories about behavior seem vulnerable to becoming the caricatures their adversaries initially make of them. Thus, many ethologists, originally interested in distinguishing behaviors with cryptic ontogenies from behaviors dependent upon obvious learning contingencies also were led eventually to defend them (as “innate” and “instinctive”) as if they had virtually no ontogenies at all. The same thing need not have happened in the current circumstance, and this explains why some of us resent being called sociobiologists as long as to most nonbiologists the term expressly means acceptance of particular views about the ontogeny of behavior (see *Addendum 1*, pp. 150–52). It is surprising to me that Wilson, who has spent his life working on the social insects, in which the strikingly different castes are almost invariably determined by environmental variations, should seem so determined that such a vaguely defined behavioral variation as homosexuality in humans must depend upon a genetic polymorphism. Such causes were postulated for social insect castes, but they turned out to be wrong, at least in nearly every case.

6. Stent (Hastings Center Report) confuses the issue by referring to the efforts of molecular biologists to define genes in molecular terms as if theirs were the first efforts at useful definition of genetic units, with definitions functional in evolutionary analyses only coming along later

to “denature” the “meaningful and well-established central concept of genetics into a fuzzy and heuristically useless notion.” This is nonsense. The gene concept was functional as a recombining unit, and highly useful as such, a half century before knowledge of DNA as its molecular basis; it has not ceased to be such a concept in evolutionary genetics, population genetics, and Mendelian genetics, despite Stent’s assertion that for “all working geneticists” the concept is restricted to the unit of genetic material in which the amino acid sequence of a particular protein is encoded. Stent says that genes were “previously conceptualizable by classical genetics only in terms of differences or alleles.” True enough, and they are still so conceptualized in studies outside molecular biology. The reason is that this is a very useful concept. We are back to the fact that fitness is only a matter of better versus worse in the immediate environment (Williams, *Adaptation and Natural Selection*). The important thing about genes is not what they are but what they do, and the most important thing they do is work together to produce organisms; we know very little yet about how they do that, and except for very few simple cases involving simple organisms what we know about it was not learned by studying either DNA or amino acid sequences. To behave as though all such things have to wait until we work up from the molecular level is to fail to comprehend that the secret of life is not DNA after all, but natural selection; the structure and integrity of the DNA molecule, as well as its relationship to the identity of the recombining units, are all products of natural selection. Satisfactory understanding of genetic units ultimately will involve connecting molecular-level structure and function with complex phenotypic effects, like behavior, the genetic basis of which will continue to be studied chiefly through recombination; such understanding is unlikely to be accomplished by either of these approaches alone.

7. G. C. Williams, “Pleiotropy, Natural Selection, and the Evolution of Senescence,” *Evolution* 11 (1957): 398–411; W. D. Hamilton, “The Moulding of Senescence by Natural Selection,” *Journal of Theoretical Biology*, 12 (1966): 12–45.

8. E. Walster and G. W. Walster, “Equity and Social Justice,” *Journal of Social Issues* 31 (1975): 21–43.

9. A particularly good example is Jeremy Boissevain’s approach in *Friends of Friends* (New York: St. Martin’s Press, 1974). Another is B. F. Skinner, *Beyond Freedom and Dignity* (New York: Alfred A. Knopf, 1971), in which the author discusses positive and negative reinforcement in terms of *individuals* but skips to the group or species level to discuss cultural change (even though, curiously, moving back to the individual level to discuss *objections* to deliberate designing of culture through

conscious control of behavior). Never does Skinner hit upon the obvious: that *individuals* are evolved to *reproduce*: and this flaw, it seems to me, causes his entire theme (of behavioral control, design of culture, or search for "an optimal state of equilibrium in which everyone is maximally reinforced") to collapse.

10. For discussion of how the consideration of nepotism alters analyses of networks and systems of social exchange, see R. D. Alexander, "Natural Selection and Social Exchange," in *Social Exchange in Developing Relationships*, ed. R. L. Burgess and T. L. Huston (New York: Academic Press, in press); "The Search for a General Theory of Behavior," *Behavioral Science* 20 (1975): 77-100; "Natural Selection and the Analysis of Human Sociality."

11. W. D. Hamilton, "The genetical evolution of social behaviour, I, II," *Journal of Theoretical Biology* 7 (1964): 1-52.

12. I am not suggesting that culture has no significance or value, but hypothesizing that the only *singular* thing about its significance, in historical terms, is that it derives incidentally from inclusive-fitness-maximizing behavior by individuals acting separately and in common-interest groups, and that its value—say, in terms of the present and future—will probably also be interpreted by individuals and common-interest groups on the basis of its ability to contribute to inclusive-fitness-maximizing and the surrogates of inclusive-fitness-maximizing in modern environments. This hypothesis, of course, remains to be tested.

13. To identify kin individually is to specify them as avenues of potential inclusive-fitness-maximizing by individuals. See R. D. Alexander, "Natural Selection and the Analysis of Human Sociality."

14. R. D. Alexander, "Natural Selection and Societal Laws," in *The Foundations of Ethics and Its Relationship to Science* vol. 3: *Morals, Science and Sociality* ed. H. Tristram Engelhardt and Daniel Callahan (Hastings-on-Hudson, New York: The Hastings Center, 1978).

15. For a discussion of the reasons why cultural evolution continues to accelerate in relation to organic evolution, and for other references, see R. D. Alexander, "Evolution and Culture," in *Evolutionary Biology and Human Social Behavior: An Anthropological Perspective*, ed. N. A. Chagnon and W. G. Irons (North Scituate, Mass: Duxbury Press 1979).

16. Thus, tendencies to become deleteriously obese or to seek "excessively" immediate pleasures, such as overconsuming sugar when it is abundant, are most likely to be understood by considering the kind of environment in which these propensities evolved.

17. See note 15 and references therein; also see R. D. Alexander, "Evolution, Human Behavior, and Determinism," *Proceedings of the Biennial Meeting of the Philosophy of Science Association* 2 (1976):

3–21; R. D. Alexander and G. Borgia, “Group selection, Altruism, and Levels of Organization of Life.” *Annual Review of Ecology and Systematics* 9 (1978):449–74.

18. See R. D. Alexander, “The Search for a General Theory of Behavior,” *Behavioral Science* 20 (1975):77–100; R. D. Alexander, “Evolution Human Behavior and Determinism,”; also R. D. Alexander and K. M. Noonan, “Concealed Ovulation and the Evolution of Human Sociality,” In: *Evolutionary Biology and Human Social Behavior*.

19. See R. D. Alexander, *Darwinism and Human Affairs* (Seattle; University of Washington Press [in press]).

20. An evolutionary model does not deny that events contrary to inclusive-fitness-maximizing occur, only that when present they are most likely to be interpretable in terms of the history of environments in which they and their ontogenetic-physiological backgrounds were selected.

21. This does not exclude the possibility that some or even many scientists are, in fact, at least to the best of their ability to describe their motivations, devoted to a search for the truth. Repeated and sufficient positive social reinforcement for approaching this condition, and negative reinforcement for diverging from it, can surely bring it about.

22. Neither is it new. P. W. Bridgman, for example, expressed essentially this idea in *Reflections of a Physicist* (New York: Philosophical Library, p. 227), in these words: “. . . in scientific activity the necessity for continual checking against the inexorable facts of experience is so insistent, and the penalties for allowing the slightest element of rationalizing to creep in are so immediate, that it is obvious to the dullest that a high degree of intellectual honesty is the price of even a mediocre degree of success.”

23. G. C. Williams, “Pleiotropy, Natural Selection, and the Evolution of Senescence: *Evolution* 11 (1957): 398–411; W. D. Hamilton, “The Moulding of Senescence by Natural Selection,” *Journal of Theoretical Biology*, 12 (1966), 12–45.

24. R. D. Alexander, *Darwinism and Human Affairs*.

25. R. D. Alexander, *Darwinism and Human Affairs*.

26. Exactly the same set of statements could be developed into a set of *predictions* about the behavior of individuals in any extant society in which knowledge of the predictions does not exist.

27. Anyone incredulous about my acceptance of Rosenfeld’s interpretation, or who fails to appreciate its extent (perhaps because of his own inability to visualize a compatibility between natural selection as a causal agent in human behavior and the kind of freedom of decision or will implied by Rosenfeld), will regard the inevitable paradox of more

and more profound self-analysis as something other than a problem for all analysts and observers; he may even see it as a special problem for the evolutionist. To the contrary, the problem will lie in the particular form of the bogey man of determinism seen in the mind's eye of such a critic, and will only disappear when his biological sophistication has exceeded the level indicated by his incredulity. A commentator on this paper, for example, suggested that I am guilty of the fallacy of self-referential inconsistency. An evolutionary view, however, may instead resolve this philosophical paradox. Thus, to say that humans have *evolved* to be nothing but inclusive-fitness-maximizing systems is not to say that in all environments they can *only* be such. Who can say what humans so evolved may do in an environment of both self-reference *and* knowledge of their evolutionary background?

28. Albert Rosenfeld, *Saturday Review*, December 10, 1977, pp. 19–20.

29. I am not arguing here that all cultural inertia has such causes, or that all cultural inertia is retrogressive; rather, only the obvious point that part of cultural inertia results from individuals and groups acting in their own personal interests, and that these interests may be realized by conserving essentially any aspect of culture, including demonstrably false ideas and interpretations.

30. John Rawls, *A Theory of Justice* (Cambridge, Mass.: Harvard University Press, 1971), pp. 548–49.

31. R. D. Alexander, "Natural Selection and Societal Laws."

32. It is worth considering in what kind of society this would not be so. I suggest that the criteria are not complex. A certain minimum contribution of each member to the common good must be specified. All material benefits and reproductive outlets (or their surrogates) above this minimum must be equalized among societal members, with graded rewards existing only in the form of differing degrees of social approval (indicated by entirely symbolic awards such as nontransferable and otherwise valueless medals, or by titles such as various orders of heroism). It would be a necessary concomitant that societal members not meeting the minimum contribution and otherwise accepting these criteria either be exiled to a less desirable circumstance or otherwise eliminated from society.

33. I allow for the essential certainty that in some circumstances, and perhaps for certain offspring more than others, parents actually manipulate offspring to maximize the parent's inclusive fitness rather than the offspring's own (See R. D. Alexander, "Evolution of Social Behavior," *Annual Review of Ecology and Systematics* 5 (1974):325–83. J. E. Blick, "Selection for Traits which Lower Individual Reproduc-

tion," *Journal of Theoretical Biology* 67 (1977):597–601, has noted that one part of my 1974 argument was wrong; this does not detract from the general asymmetry of the parent-offspring interaction, resulting from the phenotypic power difference and the facts that offspring depend on parents and parental care evolves to maximize the parent's reproductive success.

34. The advent of socialized medicine, at least in a society like our own, may actually exacerbate this problem in some respects, because it has the interesting consequence of causing medical care to become a burden on *society as a whole* which may sometimes lead to its validity or feasibility being judged in cost-benefit terms less directly relating to the welfare of the individual patients involved. Since none of us is likely to favor classes of discrimination likely to affect ourselves detrimentally, one might expect that common afflictions will sometimes be compensated when rare ones are not, or that medical compensation could become excessive in circumstances in which all in society feel threatened by the system.

35. See also R. D. Alexander, "Natural Selection and Societal Laws."

36. W. Friedmann, *Legal Theory*, 5th Edition, (London: Stevens and Sons, 1967), p. 47.

37. Michael Landman, *Philosophical Anthropology* (Philadelphia: The Westminster Press 1974), p. 22.

THE ROOTS OF ETHICS

SCIENCE, RELIGION, AND VALUES

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PLENUM PRESS · NEW YORK AND LONDON

ISBN-13: 978-94-009-5731-2 e-ISBN-13: 978-94-009-5729-9
DOI: 10.1007/978-94-009-5729-9

© 1976, 1977, 1978, 1980, 1981 The Hastings Center
Softcover reprint of the hardcover 1st edition 1981

Institute of Society, Ethics, and the Life Sciences
360 Broadway, Hastings-on-Hudson, New York 10706

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