BIOLOGY AND THE SOCIAL SCIENCES

by Edward O. Wilson

Abstract. The sciences may be conceptualized as a hierarchy ranked by level of organization (e.g., many-body physics ranks above particle physics). Each science serves as an *antidiscipline* for the science above it; that is, between each pair, tense but creative interplay is inevitable. Biology has advanced through such tension between its subdisciplines and now can serve as an antidiscipline for the social sciences—for anthropology, for example, by examining the connection between cultural and biological evolution; for psychology, by addressing the nature of learning and the structure of the unconscious; for economics, by examining economically irrational behavior and by comparing economic activity in humans and other species. Sociology, concerned mainly with advanced literate societies, is relatively remote from the genetic basis of human social behavior. However, moving between biological and social levels of organization generates richness and points to new and unexpected principles.

Keywords: antidiscipline; biological evolution; cultural evolution; economic theory; genetic determinism; learning theory; psychoanalytic theory; relationships between scientific disciplines.

> The opposite of a correct statement is a false statement. But the opposite of a profound truth may well be another profound truth.
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> —Niels Bohr

For every discipline in its early stages of development there exists an antidiscipline. For many-body physics, particle physics; for chemistry, many-body physics; for molecular biology, chemistry; for

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[*Zygon,* vol. 25, no. 3 (September 1990).]
 ISSN 0591-2385

cellular biology, molecular biology; and so forth. With the word *antidiscipline* I wish to emphasize the special adversary relation that exists initially between the studies of adjacent levels of organization. This relationship is also creative, and with the passage of a great deal of time it becomes fully complementary.

In this article I will argue that biology has now moved close enough to the social sciences to become their antidiscipline. Hitherto biology has affected the social sciences largely through technological manifestations such as the benefits of medicine, the mixed blessings of genetics, and the specter of population growth. Although of great practical importance, these matters are wholly trivial with reference to the conceptual foundation of the social sciences. The conventional academic treatments of "social biology" and "social issues of biology" present some formidable intellectual challenges, but they are not concerned with the core of social theory. Many scholars judge this core to be the deep structure of human nature, an essentially biological phenomenon that is also the primary focus of the humanities.

If it is true that biology is the antidiscipline of the social sciences, the past failure of the social sciences to develop a common body of theory is understandable. The reason is that the relevant branches of biology—neurobiology and sociobiology—are only now becoming mature enough to attain a juncture with the social sciences. Although it would be premature to say that biology can revolutionize the social sciences, at least to the extent that chemistry has revolutionized biology, it would be equally premature to say that it cannot.

This proposition is well worth examining. To that end I will first describe precedents from the history of biology, then turn to some developments within the individual social sciences that suggest a growing susceptibility to biological explanation. Finally, acknowledging my obligation as a student of the antidiscipline, I will attempt to define the obviously strong limitations of biological reductionism.

DISCIPLINES AND ANTIDISCIPLINES

In general, the practitioners of a given discipline in its early, natural-history phase are concerned with the discovery and classification of phenomena. They stress novelty and particularity. In terms of the classic thematic dualities of science (Holton 1973) their explanations are characteristically holistic, emphasizing pattern and form over units of construction. In the early phase, specialists are also likely to be dualistic in philosophy, questioning whether their phenomena are directly subject to the laws of the remainder of science.[1] And in later stages, having been converted, they are still more concerned with what Victor Weisskopf has called extensive as opposed to intensive research, the use of existing theory to explain the widest possible range of phenomena as opposed to the search for fundamental laws (Weisskopf 1967, 23–26).

Members of the antidiscipline are likely to be monistic with reference to the discipline and dualistic with reference to their own subject. Having chosen as their primary subject the units of the lower of the paired levels of organization, they believe that the next discipline above can be reformulated by their laws. Their interest is relatively narrow, abstract, and exploitative, lacking the totemic attachment to phenomenology displayed by the most devoted students of the discipline above. Thus P. A. M. Dirac, speaking of the theory of the hydrogen atom, could say that its consequences would unfold as mere chemistry, whereas the biochemist Franz Hofmeister responded to the recent great advances in cell structure by recommending (in 1901) that biologists pay more attention to enzymes.

It is easy to see why each discipline is also an antidiscipline. A tense creative interplay is inevitable because the devotees of adjacent levels of organization are committed to different methodologies when they focus on the upper level. By today's standards a broad scholar can be defined as one who is a student of three subjects: his discipline, the lower antidiscipline, and the subject to which his specialty stands as antidiscipline. A well-rounded cellular neurophysiologist, for example, is deeply involved in the microstructure and behavior of single cells, but he also understands the molecular basis of electrical and chemical transmission, and he hopes to explain enough of neuron systems to help account for the more elementary patterns of animal behavior.[2]

Cell Biology and Biochemistry

In the late 1800s cell biology and biochemistry grew at an accelerating pace. Their relationship during this period was very complicated, but it can be broadly characterized as fitting the schema just described. The cytologists were excited by the mounting evidences of an intricate cell architecture. They had also deduced the mysterious choreography of the chromosomes during cell division, setting the stage for the emergence of modern genetics and experimental developmental biology. Many biochemists, on the other hand, remained skeptical of the idea that so much structure exists. They emphasized the possibility of artifact production by the chemical reactions used in cytological preparations and stood apart from the debate then raging over whether protoplasm is homogeneous, reticular, granular, or foamlike. Their interest lay in the more "fundamental" issues of the chemical nature of protoplasm, especially the newly formulated enzyme theory of life.

In general, biochemists judged the cytologists too ignorant of chemistry to grasp the basic processes, whereas the cytologists considered the methods of the chemists inadequate to characterize the structures that diagnose the living cell. The renewal of Mendelian genetics and subsequent progress in chromosome mapping did little at first to effect a synthesis. Biochemists, seeing no immediate way to encompass classical genetics, by and large ignored it.

Both sides were essentially correct. Biochemistry has now explained so much of the cellular machinery by its own terms as to justify its most extravagant early claims. But in achieving this feat (mostly during the past thirty years) it was partially transformed into the new discipline of molecular biology—biochemistry that entails particular spatial arrangements and movements of large molecules. Cytology forced the development of a special kind of chemistry and the employment of a wide array of powerful new methods, including electrophoresis, chromatography, density-gradient centrifugation, and X-ray crystallography. At the same time cytology metamorphosed into modern cellular biology. Aided by electron microscopy, it converged in language and outlook toward molecular biology. Finally, classical genetics, by switching from *Drosophila* to the ultrafast-breeding and far more simply constructed bacteria and viruses, has incorporated much of biochemistry to become molecular genetics.

Progress over a large part of biology was fueled by competition among the various attitudes and themata derived from biology and chemistry—the discipline and its antidiscipline. Joseph Fruton (1976), a biochemist who has paid close attention to this Hegelian interplay, has suggested that inevitably "such competition is attended by tensions among the participants. I venture to suggest that this competition and these tensions are the principal source of the vitality of biochemistry and are likely to lead to unexpected and exciting novelties in the future, as they have in the past."[3]

ECOLOGY AND POPULATION BIOLOGY

Modern ecology has had a troubled history. As recently as ten years ago it was painfully unfashionable in many American universities. An anecdote will illustrate how serious the situation had become. The Department of Biology at Harvard University was at that time increasingly dominated by molecular and cellular biology, and there appeared to be little chance of adding new faculty members specializing on populations or ecosystems. One afternoon I proposed departmental membership for a distinguished ecologist who had been appointed by another school within the university. One of the molecular geneticists, a new Nobel laureate, said, "Are they out of their minds?" When I asked what he meant, he responded that anyone who hired an ecologist must be out of his mind. After the meeting one of my senior colleagues in evolutionary biology suggested that I not refer explicitly to ecology in the future, because it had become a "dirty word." The ecologist was invited much later, after the molecular and cellular biologists had formed independent administrative units.

Ecology is almost embarrassingly fashionable now, and I should add at once that the several ecologists subsequently added to the Department of Biology are on very cordial terms with all their colleagues. But the lesson learned at Harvard and at other universities with similar experiences was, I believe, that few scientists are willing to share resources with those whose research is more than one level of organization removed. Populations and communities, the central concern of ecology, are separated from molecules and cells by an entire level, that of the organism. In an environment ruled by competitive research, profit can be extracted only from the discipline and antidiscipline.

The problem with ecology, however, was more than lack of sympathy from molecular and cellular biologists. The difficulty came ultimately from ecology's focus on the highest level of biological organization, the community, and the weakness of its connections to fundamental population biology. Without quite realizing it, ecology was an orphan discipline. By the 1950s theoretical population genetics, one of the logical antidisciplines, had grown into a technically formidable, even arcane subject. Its models were derived from the distinctive chromosomal mechanics of Mendelian heredity, and it dealt almost exclusively with the interplay of the Darwinian operators—selection, mutation pressure, gene flow, genetic drift, and meiotic drive. Ecology was reduced to the single parameter of the selection coefficient. By a remarkable piece of bad luck, experiments and field studies were concentrated on *Drosophila*. Fruit flies are superb insects for rapid genetic and demographic analysis in the laboratory, but they are singularly hard to find and to study in their natural habitats. Much of what is known of their ecology was deduced from their appearance at food traps. Partly as a result of this historical accident, population genetics remained apart from ecology.[4]

Similarly, with the single exception of pure demography, the fundamental theory of population ecology had not advanced much beyond the principles advanced by Alfred Lotka, Vito Volterra, and R.A. Fisher in the 1920s and early 1930s. Ecology, consisting largely of analysis at the level of entire communities, developed as an ever more elaborate descriptive science. In some of its branches, such as biogeography and phytosociology, the systems of classification and quantitative description reached phantasmagoric extremes.

The time was obviously ripe for a new effort that would bring population genetics and population ecology together and cast them in their proper role as antidisciplines to community ecology. In the late 1950s and early 1960s a small group of younger population biologists (I confess to being a member) set about self-consciously to make such an effort. Several meetings were held to discuss the matter and to some extent to divide the labor. The undoubted leader was the late Robert H. MacArthur, whose work on complex community phenomena was exceptional in its originality and discrimination of important problems (see Wilson 1973, 11–12). MacArthur's seminal contribution was his 1957 analysis of the population-abundance frequency curve of a community of bird species (MacArthur 1957, 293–95). Descriptive ecologists had accumulated a large amount of information on this subject but had done little more than fit empirical curves to it. MacArthur used the data to test a series of competing hypotheses based on models of various forms of interaction among species populations, a process which up to that time had been very difficult to study directly in nature. Although his method has had only limited success when applied to other biological communities, it validated postulational-deductive theory and demonstrated that leaps of the imagination can lay open the most complex ecological processes and give them new meaning.

The response of other ecologists to such renewed model building at the population level was predictably mixed. Many of those devoted to painstaking descriptive work refused to believe that general laws could be so cheaply bought. The patterns they discerned seemed too elaborate, the variables too numerous, and the mark of history too deep and idiosyncratic to make general models anything more than a clever illusion. A community of organisms is a tangled bank, to use Darwin's famous phrase in the closing paragraph of the *Origin of Species*, or a uniquely woven tapestry, in the metaphor of one of the recent critics. Prominent ecological schools still exist, comprised of ecosystems analysts, theoretical population ecologists, physiological ecologists, and so forth. It is cytology versus biochemistry all over again. But the history of this subject, if I have interpreted it correctly, is now entering the middle phase of the classic ontogeny. Broad areas of agreement are already apparent, and model building at the population level has become routine (see, for example, May 1976).

THE SOCIAL SCIENCES AND BIOLOGY

Let us now return to the original proposition that biology is the antidiscipline of the social sciences. This assessment is not congenial to the prevailing view in the social sciences and humanities that human social life is the nearly exclusive product of cultural determinism, constrained only by the most elementary and unstructured biological drives. There is a strong tendency to think of our own species as entirely plastic and hence all but equipotent in the design of its social institutions. However, this conception will not stand close scrutiny. A comparison of the literally tens of thousands of other highly social species on Earth, from colonial coelenterates through the social insects to the most social of the birds and mammals, reveals that the sum of all the varieties of human social behavior occupies only a small envelope in the space of realized social arrangements (Wilson 1975).

Anthropocentrism is a disabling vice of the intellect. I am reminded of the clever way Robert Nozick deflated our sense of superiority to other animal species in order to make his principal argument for vegetarianism. If visitors from another planet happened to be far more intelligent and sensitive than ourselves, and applied our own criteria of relativity, they could proceed in good conscience to eat us (Nozick 1974). By the same token, and to our considerable chagrin, scientists among them might find us uninteresting as a social species—just another cultural-linguistic variant on the basic mammalian theme—and instead turn to study the more theoretically challenging societies of ants and termites.

It is this quality of specificity and restriction that biologists have in mind when they speak of genetic determinism. In order to define a genetic trait precisely it is necessary to compare two or more states of the same character. To say that blue eyes are inherited, without further qualification, is not meaningful, because everyone knows that blue eyes are the product of the interaction of genes and the largely physiological environment that brought final coloration to the irides. But to say that the difference between blue and brown eyes is based partly or wholly upon differences in genes is a meaningful statement by virtue of being testable and translatable into the laws of genetics.

Human social behavior can be evaluated in the same way, first by comparison with the systems of other species and then, with far greater difficulty and ambiguity, by studies of variation within the species. For example, certain general traits are shared with most other Old World primates, including size of intimate social groups on the order of 10–100; males larger than females, probably in relation to polygyny; a long period of socialization in the young, shifting in focus from the mother to age- and sex-peer groups; and social play strongly developed, with emphasis on role practice, mock aggression, and exploration. It is virtually inconceivable that primates, including human beings, could be socialized into the radically different repertories of insects, fish, birds, or antelopes; or that the reverse could be accomplished. Human beings, by conscious design, might well *imitate* such arrangements; but it would be a fiction played out on a stage, running counter to deep emotional responses and with no chance of persistence through as much as a single generation.

Homo sapiens is distinct from other primate species in ways that can be explained only as the result of a unique human genotype. Universal or near-universal traits include the facial expressions that denote basic emotions, and some other forms of paralinguistic communication; elaborate kinship rules that include incest avoidance; a semantic, symbolical language that develops in the young through a relatively strict timetable; close sexual and parent-offspring bonding; and others. Again, to socialize a human being out of such species-specific traits would be very difficult if not impossible, and almost certainly destructive to mental development. People might imitate the distinctive social arrangements of a whitehanded gibbon or hamadryas baboon, but it seems extremely unlikely that human social systems could be stably reconstructed by such an effort.

It is significant that not only do human beings develop a species-characteristic set of social behaviors, but that these behaviors are generally mammalian, and most specifically Old World primate in character. Furthermore, even the species-specific traits are logically derivable in some cases from the inferred ancestral modes still displayed by a few related species. For example, the facial expressions and some nonlinguistic vocalizations can be plausibly derived in phylogenetic reconstructions (van Hooff 1972, 209–41). This is precisely the pattern to be expected if the human species was derived from Old World primate ancestors (a fact) and still retains genetic constraints in the development of social behavior (a hypothesis).

An important quality of a genetic determinism is that it seldom entails the control of a single phenotype by a single gene. Polygenic inheritance is the rule, and the entity determined is not one trait but rather a range of possible phenotypes. For example, diabetes and schizophrenia possess moderate genetic components. The multiple genes underlying them produce a stronger tendency to develop the traits; they also prescribe the range of possible manifestations that are probable under specified environmental conditions. In a parallel way basic human social behaviors—including those as structured as male-group bonding, territoriality, and kinship rules—emerge as outermost phenotypes following behavioral development, the range and scope of which is constrained by the interaction of polygenes with the environment. With reference to this interaction, there is no reason to regard most forms of human social behavior as qualitatively different from physiological and nonsocial psychological traits.

Whatever the present social arrangements of our species, the biological foundation of human nature arose in populations that adapted to special environments. The prevailing hypothesis, which holds that the basic qualities were fashioned as an adaptation to a more predatory existence in open habitats, may or may not be correct in detail. The important point is that the emotional controls and the developmental pathways are considered to be structured in idiosyncratic ways that can be wholly understood only by retracing the ecological history of the species. That such a relationship exists in other social species can be readily demonstrated (Wilson 1975a). This is why paleoanthropology, by reconstructing the Pleistocene African environment, has an important role to play in behavioral biology.

An unavoidable question is the extent to which social behavior varies genetically *within* the human species. This is a subject entirely removed from the distinctive properties of human behavior vis-à-vis that of other species. The evidence is strong that almost but probably not quite all differences among cultures are based on learning and socialization rather than on genes.[5] At the same time variation within populations is evidently great enough to create the potential for further human social evolution by population-wide genetic change. Studies comparing monozygotic twins with dizygotic same-sex twins suggest a genetic component in the variation of a large array of traits having an influence on the development of social behavior. These traits include verbal and number ability, word fluency, perceptual speed, memory, the timing of language acquisition, psychomotor skill, extroversion-introversion, homosexuality, and certain forms of neuroticism and psychosis (Ehrman and Parsons 1976; McClearn and DeFries 1973). Although not conclusive in themselves, such studies are strongly suggestive. Behavioral heritability is also enhanced by the undoubted existence of single, identifiable point mutations and chromosome aberrations, such as those causing the Lesch–Nyhan and Turner's syndromes, that alter various components of behavior differentially. Some geneticists have gone so far as to suggest that once the conditions make their appearance in spite of medical precautions, their study can permit the indirect genetic dissection of behavioral traits, in analogy to the technique used for nematodes and fruit flies.

My overall impression of the available information is that *Homo sapiens* is a typically animal species with reference to the quality and magnitude of the genetic diversity affecting behavior. If the comparison is correct, the psychic unity of humankind has been reduced in status from a dogma to a testable hypothesis. This is not an easy thing to say in the present political ambience of the United States, and it is regarded as punishable heresy in some sectors of the academic community. But the idea needs to be faced squarely if the social sciences are to be entirely honest. I cannot regard it as dangerous. Quite the contrary: the political consequences of its objective examination will be determined by our value system, not the reverse. It will be better for scientists to study the subject of genetic behavioral diversity than to maintain a conspiracy of silence out of good intentions and thereby default to ideologues.

Following this elementary excursion into genetic determinism, let us now consider how the various social sciences might be influenced by biological theory. In the brief sections to follow I have little more than scraps of information and impressions to offer. It is hoped that they will nevertheless suffice to provide a view of the reverse side of the social sciences as it has been glimpsed by a biologist approaching in that direction.

ANTHROPOLOGY

The central question of biological anthropology is the nature and strength of the coupling between cultural and biological evolution. Cultural evolution is Lamarckist and usually very fast; biological evolution is Darwinist and slower by at least an order of magnitude. Because the most rapid cultural changes track environmental fluctuations too brief in duration to influence directional genetic selection, cultural fitness can be expected to diverge frequently from genetic fitness (Richerson and Boyd 1978). But the divergence must be limited in degree and duration, because ultimately the newly created social environment will be tracked and the gap narrowed by natural selection. If this modification of basic sociobiological theory is correct, there should be two detectable consequences. First, it will be learning rules rather than specific cultural forms that are inherited.[6] And where the rules are most directly concerned with survival and reproduction, as in the case of sexual and parental bonding, incest avoidance, and xenophobia, they are likely to be the most rigid and structured. Marshall Sahlins has recently argued that the lack of clear correspondence between human kinship rules and the details of genetic kin selection theory disproves basic sociobiological theory as far as human beings are concerned (Sahlins 1976). But this goes much too far. Kinship rules are central to social organization in most societies, and in aggregate they appear to enhance inclusive genetic fitness. Richerson and Boyd have suggested that four competing hypotheses can be posed for the explanation of kinship rules: detailed genetic control, rational strategizing, complete cultural determinism, and coupled cultural and genetic control (Richerson and Boyd 1978). They conclude that the ethnographic facts are consistent only with the model of coupled cultural and genetic control.

The second expected consequence of coupling is that the genetic fitness conferred by particular cultural traits should be strongest in the oldest, culturally most stable societies. Thus it appears correct to focus attention on hunter-gatherer and persistent, preliterate herding and agricultural societies. This circumstance explains why anthropology has already become the social science closest to sociobiology. Explicit tests of sociobiological theory are being made in studies on polygyny, status, societal fissioning, territory, and warfare (see, e.g., Chagnon 1976, 14–18, and Durham 1976, 385–415).

It is probable that population biology will be simultaneously altered to accommodate the special problems of anthropology. In addition to the solution of the dual inheritance problem, there is a need for advances in the theory of group and kin selection to distinguish unilateral, "hard-core" altruism from transactional, "soft-core" altruism. Also, the complexity of human population structures presents unique challenges to biology. Population boundaries are seldom sharp, often being confused by discordant linguistic, cultural, and historical-political patterns. Groups also shift rapidly in their loyalties, forming alliances in one year and dividing into quarreling factions the next. The present theory of population genetics and ecology is entirely inadequate to handle such complications.

Psychology and Psychoanalytic Theory

Just as anthropology has been burdened in the past by the doctrine of complete cultural determinism, conventional psychology has been burdened by general process learning theory. It was natural for Thorndike, Watson, and other pioneering psychologists to choose large animals, that is, birds and mammals, for their study objects, rather than insects and opisthobranch mollusks. Partly as a result of this choice, learning came to be accepted as the central process of behavior. Moreover, the learning process was viewed as being essentially equipotential: the same laws were theorized to apply to whatever learning process and organism are chosen. Thus Skinner could say in (1938), "The general topography of operant behavior is not important, because most if not all specific operants are conditioned. I suggest that the dynamic properties of operant behavior may be studied with a single reflex."[7] It was believed that by placing animals in simplified laboratory environments, where stimulation can be rigidly controlled, the most general laws would emerge.

This is a powerful idea, with seductive precedents in the physical sciences, and it has resulted in substantial advances in the study of animal and human behavior. Nevertheless, general process learning theory has started to crumble. In its place is appearing the description of a mélange of specialized learning phenomena that conform to no general law except, perhaps, evolution by natural selection.[8] The full range of learning potential of each species appears to be separately programmed. According to the species, each animal is "prepared" to learn certain stimuli, counterprepared to learn others, and unprepared (neutral) with respect to still others. For example, adult herring gulls quickly learn to distinguish their newly hatched chicks but never their own eggs, which are nevertheless as visually diversified. Indigo buntings are prepared to learn the circumpolar constellations by which they orient their nocturnal migrations but are counterprepared to learn other constellations. When chicks are shocked at their beaks while drinking water and simultaneously given a visual stimulus, they thereafter avoid the visual stimulus, but they do not learn to avoid an auditory stimulus presented the same way. The reverse is true when the shock is administered to the feet; that is, the chicks are prepared to learn sound but counterprepared to learn visual cues (Shettleworth 1972, 228–36). The timing of preparedness in the life cycle is also programmed and species-specific in ways that are readily interpreted as adaptations to the particular environments experienced by the species during their recent evolutionary pasts.

The hypothesis of learning rules as idiosyncratic evolutionary adaptations has seldom been examined with reference to the human species. It seems significant that phobias, which share some properties with imprinting in lower animals, are readily acquired against snakes, spiders, rats, and other potentially dangerous objects in mankind's ancient natural environment, but only rarely against such dangerous artifacts as knives and electrical outlets (Seligman 1972, 451–62). Language is acquired by small children through a progression of closely timed steps, involving distinctive vocalizations and phrase forms later replaced by adult language (Brown 1973). It is difficult to believe that the rules of this most human of all learning events have not been shaped by natural selection.

Psychoanalytic theory appears to be exceptionally compatible with sociobiological theory, a fact already appreciated by some of the psychoanalysts themselves (e.g., Lifton 1976). If the essence of the Freudian revolution was that it gave structure to the unconscious, the logical role of sociobiology is to reconstruct the evolutionary history of that structure. When Freud speculated in *Totem and Taboo* on the primal father, primal horde, and the origins of the incest taboo, he created a sociobiological hypothesis, but a poor one. The same is true of his insights into the conflict of self and society presented in *Civilization and Its Discontents*. Whether population biology and evolutionary theory can be used to restructure and objectively test some of psychoanalytic theory remains to be seen.[9]

ECONOMICS

Classic economic theory restricted itself to the goods and services that can be measured by money and market pricing. In recent years new, less easily quantified parameters have entered the equations, including time, human capital, and environmental quality (Samuelson 1976). A closer scrutiny is also being made of what Leibenstein has termed the X-efficiency factors, which include motivation, *esprit*, effort, persistence, and other psychological variables made mensurable (Leibenstein 1976). In a word, microeconomics has begun to incorporate social psychology. It is now widely appreciated that human beings do not behave as rationalizing economic machines. Macroeconomic predictions of the future will almost certainly be based on the wiser perception of irrational elements in human nature.

To the extent that the new parameters of human irrationality are interpreted as an evolutionary product, the methods of economics will converge toward those of biology. Already, models in ecology and sociobiology have borrowed heavily from the graphical methods of economics. Optimization and decision theory are routinely used. The utility measure of biology is genetic fitness, and the enabling devices are anatomy, physiology, and behavior. I expect that once a method is developed for assessing the coupling of genetic and cultural evolution, the utility measures of economics and evolutionary biology will come to overlap broadly.

An interspecific comparative economics is also a possibility.

During recent research on the evolution of division of labor in insect societies, George Oster and I have written a short book that resembles a textbook in microeconomics (Oster and Wilson 1978). Insect economics differs in several respects: the transactions among colony members are almost exclusively instead of merely partially instinctive (that is, "irrational"), the societies are mostly sterile and female, and (because of the haplodiploid genetic bias) unilateral altruism has far greater genetic utility than in human societies. The broad forms of the analyses are otherwise much the same. The point is that human economics is not really general economics, but rather the description of economic behavior in one mammalian species with a limited range of the biological state variables.

SOCIOLOGY

By virtue of its loftier perch in the hierarchy of subject matter, sociology should be the queen of the social sciences. Yet I personally find it the most alien and least interesting. Part of the cause is revealed in the following statement by Durkheim: "In a word, there is between psychology and sociology the same break in continuity as between biology and the physicochemical sciences. Consequently, every time that a social phenomenon is directly explained by a psychological phenomenon, we may be sure that the explanation is false" (Durkheim 1938). I suspect that this statement is as completely wrong for sociology as it proved to be for biology. Although few contemporary sociologists would uncritically accept *The Rules of Sociological Method,* Durkheim's dualism lives on by tradition. Sociological analysis seldom utilizes the known facts of social psychology to any depth, and evolutionary biology remains all but taboo. The specters of biologism and social Darwinism are still feared, entirely without justification.[10] The situation is so extreme that I suspect that progress in the near future will be measured by the connections sociology makes with its antidisciplines. To the extent that it does not make these connections, it will remain an ad hoc, descriptive science.

Yet sociology is not destined to be cannibalized by the antidisciplines, any more than cytology was absorbed by biochemistry. The reason is that sociology is truly the subject most remote from the fundamental principles of individual behavior. Advanced literate societies, the main concern of sociology, are the most removed in character from the kinds of social and economic systems in which the genetic basis of human social behavior evolved. Having been jerry-built on the Pleistocene human biogram, they are the least stable, probably have the greatest discrepancies between genetic and cultural fitness, and hence are most likely to display emergent properties not predictable from a knowledge of individual psychology alone.

THE LIMITS OF REDUCTIONISM

Karl von Frisch once made a remarkable statement about his research. He said that the honeybee is like a magic well—the more you draw from it, the more there is to draw. Other students of social insects share this sense of seemingly infinite richness in the phenomena of colonial life. They have learned that a great deal of evolutionary novelty at the social level can be generated by only a small amount of genetic change at the level of the individual. A slight modification in one parameter of allometric pupal growth, for example, can produce a new array of castes; whereas an altered response to a pheromone can create a new mode of communication.

The full phenomenology of social life cannot be predicted from a knowledge of the genetic programs of the individuals alone. An observer who shifts attention from one level of organization to the next expects to find obedience to all of the laws of the levels below. But upper levels of organization require specification of the arrangement of the lower units, which in turn generates richness and the basis of new and unexpected principles. The specification can be classified into three categories: combinatoric, spatial, and historical. Thus the ammonia molecule neutralizes its electric dipole moment and conserves the laws of nuclear physics by inverting the negatively charged nitrogen back and forth through the triangle of positively charged hydrogens at a frequency of 3×10^{10} per second. But this symmetry is broken in the case of sugar and other larger organic molecules, which are too large and complexly structured to invert themselves. They break but do not repeal the symmetry laws of physics (Anderson 1972). This specification may not be of great interest to particle physicists, but its effects redound throughout organic chemistry and biology.

Primitive wasps, comprising early members of the order Hymenoptera, evolved the sex determination mechanism of haplodiploidy, whereby unfertilized eggs yield males and fertilized eggs yield females. This mechanism may have been a specific adaptation that permits females to choose the sex of the offspring according to the nature of the separate prey items they are able to subdue. But whatever the initial cause, haplodiploidy represented a historical accident that predisposed these insects to develop advanced forms of sociality. The reason is that it causes sisters to be more closely related genetically than mothers are to daughters, and so they find genetic profit in becoming a sterile caste specialized for the rearing of sisters. As an apparent result, social life among insects is almost limited to the phylogenetically advanced hymenopterans, namely the social wasps, social bees, and ants. Furthermore, most cases of insect social life can be classified either as matriarchies, in which queens control colonies of daughters, or as sisterhoods, in which sterile daughters control the egg-laying mothers. Many other strange effects flow from this genetic asymmetry. In addition, the hymenopterous societies have proved so successful that they dominate and alter much of the terrestrial ecosystems of the Earth (Wilson 1971). Who could have guessed all of this from a knowledge of haplodiploidy?

The urge to be a reductionist is an understandable human trait. Ernst Mach captured it in the following definition: "Science may be regarded as a minimal problem consisting of the completest presentment of facts with the least possible expenditure of thought" (Mach 1942). This is the sentiment of a member of the antidiscipline, impatient to set aside complexity and get on with the search for more fundamental ideas. The laws of his subject are necessary to the discipline above, because they challenge and force a mentally more efficient restructuring; but they are not sufficient for its purposes. Biology is the key to human nature, and social scientists cannot afford to ignore its emerging principles. But the social sciences are potentially far richer in content. Eventually they will absorb the relevant ideas of biology and go on to beggar them by comparison.

NOTES

1. A useful ontogeny of disciplines, starting with the natural history phase, has been outlined by F. S. C. Northrop (1947).
2. An admirable example of this approach to neurobiology is provided by Kandel (1976).
3. Another recent product of this interplay is molecular immunobiology. G. M. Edelman (1974), one of the leading contributors, has noted that "the idea of specificity and molecular recognition was prior to, and was necessary but not sufficient for, the idea of clonal selection which operated at the level of cells. In turn, the particular picture of variability and constancy which emerged at the level of the molecular analyis of antibodies could be applied to refine the idea of clonal selection at those points where it seemed most dubious.
4. This imbalance in experimental population genetics is well illustrated in the work of T. Dobzhansky and his associates (see Dobzhansky 1970). They made brilliant progress in elucidating the genetic processes of natural selection and speciation but remained largely unaware of the central issues of ecology. In recent years some *Drosophila* geneticists have concentrated more deliberately on natural history, and information more useful for ecological analysis is now accumulating rapidly.
5. D. G. Freedman (1974) has provided evidence of average differences in motor activity and excitability in neonates between Navajo, Japanese, and Caucasian populations. Such differences could lead to at least slight variations in the ways infants are handled and carried.

6. See M. W. Feldman and L. L. Cavalli-Sforza (1976). These authors provide an explicit model for the selection of genes that alter the learning rules of cultural transmission.

7. Skinner has since broadened his view to incorporate genetically programmed behavior patterns as the analogs of learned behavior in lower animals (see Skinner 1966, 1205–13). He differs from most biologists in his view, not of the basic mechanisms of learning and instinct, but in the amount of structuring that occurs in the human brain. Matters of degree are generally the most easily resolvable in science.

8. Seligman and Hager, eds. (1972), and R. A. Hinde and J. Stevenson-Hinde, eds. (1973). A contrary view has been expressed by M. E. Bitterman (1975, 699–709), who concludes that underlying principles can still be drawn, at least for the vertebrates.

9. The same is true of Marxism, which is to a considerable degree sociobiology without biology.

10. The last remnants of social Darwinism died with the advent of sociobiology, which delineated the roles of cooperation and altruism in societies and rendered them consistent with population genetics. (See, for example, Dawkins 1976.) A frankly biological critique of sociology has been provided by P. L. van den Berghe (1975).

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