

Biology Reinvigorated

Life/Society, Nature/Culture, Evolution/History

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In fact, analogy is a legitimate form of comparison, and comparison is the only practical means we have for the understanding of things. The fault of the biological sociologists was not that they used it but that they used it wrongly. Instead of trying to control their studies of society by their knowledge of biology, they tried to infer the laws of the first from the laws of the second.

Émile Durkheim, 1898¹

In order to establish itself as a discipline, it was not enough for sociology to disassociate itself from philosophy and psychology. The lines appearing in the above epigraph, published exactly one hundred years ago, remind us of what is often not considered in the difficulty of establishing social sciences in the world as a whole: first – and as a question of priority – disciplines had to fight the widely prevailing influence jointly held by evolutionism, “social Darwinism,” and “biological sociology” at the end of the nineteenth century. Durkheim’s methodological warning thus contains something that elicits a “feeling of déjà vu” in us, with respect to some of the theoretical debates that the “scholarly community” puts forward today for the general public. Is this simply a superficial and somewhat coincidental similarity, or is it, rather, the rebirth of an idea?

Patrick Tort will reconsider the response that he gave to this question in a henceforth classic essay at the end of this issue. According to him, a characteristic trait of para-scientific ideologies is the existence of a structure that is stable enough to infiltrate various scientific contexts from one era to the next as soon as the opportunity presents itself, meaning, “reactive sociobiology, under the pretext and with the excuse of genetics, Spencer’s biological sociology, and up through the controversies which the lat-

ter has instigated."² We would then be in the midst of witnessing the flagrant revenge of "social Darwinism"³ in the shape of a "neo-social Darwinism," which would have succeeded in technically updating the intuitions of the former school, making them credible within the contemporary scientific framework: no one can be unaware now that surrounding sociobiology (and from sociobiology onwards), a constellation of biological determinisms has regained a worldwide strength that clearly does not have any precedent other than that of Spencerian evolutionism.

Clearly this success is not uniform. There are comparatively resistant countries: in France, the tradition of compartmentalizing university disciplines and the neuralgic nature of the separation between naturalism and anthropology has prevented the social sciences from being affected by this phenomenon. Researchers there are not even aware of the prevailing influence they exercise in their own country among specialists in animal behavior, or abroad in their own field: the reflections of sociologist Bruno Latour along with primatologist Shirley Strum are exceptions in this respect.⁴ In contrast, after consulting issues of the journal *Current Anthropology* (considered by many to be the most prestigious and most widely circulated journal of its kind in the world), one readily notices that biological determinism has been very regularly expressed in it with no misgivings for ages now.

The New Triumph of Biologism: A Summary of Previous Episodes

Let us briefly recap the stages of transformation between the old "social Darwinism" and the new version. The first model suffered a serious setback when the Lamarckian hypothesis of the heredity of acquired characteristics was rejected by biology; this led to the founding of neo-social Darwinism in the form of a Darwinism freed from Lamarckism. According to Spencer, in effect, "there was a gradual progression from altruism within the family to social altruism."⁵ This progressive development was easily explained by a hereditary accumulation of altruistic experience. However, without the means to biologically record the experi-

ences of previous generations, the supposed essence of altruism (the logic of which was supposed to penetrate all life, from that of a microbe to that of a human) was plunged back into mystery: how could evolution so often favor the regular sacrifice of an individual for his fellow man when this act of generosity reduced the chances that this individual would produce a descendant, thus reducing the likelihood that the behavior in question would be perpetuated, and, finally, of seeing the possibility "retained" by natural selection?

This obstacle did not stop the spread of "Spencerian evolutionism" in Western societies, but little by little it eroded the scientific credibility of an actual biological sociology curriculum: by depriving it of the Ariadne thread of analogical induction denounced in Durkheim's above quote.

Neo-Darwinism required a restructuring of zoology's disciplinary landscape, one that focused firstly on genetics and ecology. During this phase of construction, the enigma of altruistic behavior was put to the side, but it returned to haunt the overall outlook at the moment that the great British geneticist John B.S. Haldane⁶ introduced the first flickerings of a "synthetic theory of evolution." However, ethology was still up in the air, and moreover, its blossoming under the direction of Konrad Lorenz and Nikolaas Tinbergen would distance Darwinism's crucial problems (except in entomology where the study of insects had rapidly stimulated an interdisciplinarity more intense than elsewhere).

The true revival of sociobiology was achieved in 1955, in an article in which Haldane suddenly found a way to compensate for the absence of Lamarckian heredity by imagining a mechanism that allowed for the natural selection of altruism at the level of genes.⁷ It was the well-known story in which a man comes to the aid of child who is drowning in a river, and has only a 90 percent chance of saving his own life while carrying the child to the riverbank: assuming that he carried a gene that motivated him to save the victim in spite of the risk, under what conditions would evolution select this gene and the behavior associated with it? Haldane responded that it was sufficient that the likelihood of genetic gain surpass that of its loss: according to Mendel's laws, if the rescuer is the father or brother of the survivor, the benefit ratio is 5 to 1; if

it is the grandfather or uncle, the benefit ratio falls to 2.5 to 1; beyond first cousins, it is best to let the kid float down the river. The trick of this calculation consists in a shift at the level of the decision: the pertinence of the choice is no longer located at the level of the organism. It belongs to the gene, that persistent speculator. The life of the individual embodies more than a means of production, the unique structural gap with a capitalist enterprise remained for the time being that the elements of the genome are simultaneously workers and members of the board of directors.

An ingenious solution that did not require any concessions from the Spencerian viewpoint: it even created a determinism that was even more imperative than that of the former "social Darwinism," since the reference to acquired characteristics left the interpretation more maneuverable. Once the gap was detected, everything moved quickly: from 1964 onwards, the British geneticists John Maynard-Smith and William D. Hamilton reinforced this opening by publishing two separate analyses in which they respectively devised the interdependent concepts of kin selection and inclusive fitness.⁸ The two ideas worked to confirm that the gene was at the decision-making level in the natural selection of behaviors and was in direct contact with the population level. Some time later, Robert Trivers added the finishing touches when he created the potential of the development of a "reciprocal altruism" beyond the ties of consanguinity.⁹

The movement was launched, but it required development and, above all, organization. Here again, the process was embarked upon without delay: a symposium entitled "Man and Beast" was held in 1969 under the auspices of the Smithsonian Institution, putting zoologists and anthropologists, who would become the major references of sociobiology, into contact.¹⁰ The revival of reductionism, though relatively disorganized, was already evident in the body of Anglo-Saxon social sciences: in the United States, "cultural ecology" became more and more clearly integrated with the functionalist tradition and strengthened its ties with primatology, which was anxious to interpret social evolution from monkey to man. This reductionist anthropology, eager for models that could provide a "resistant" biology, then accepted without question: the emergence of a third source of intervention the interdisciplinary analysis systematically formed by the study of insects, and

in particular the study of social insects, proved exportable in that it relied on the universal “will” of genes.

Finally in 1975, Edward O. Wilson, a professor at Harvard, and a renowned entomologist and biogeographer, created a well-known sensation when he published *Sociobiology: The New Synthesis*, an essay of monumental proportions that included an extraordinary bibliographic review.¹¹ The work began by depicting growth “confronted with” ethology, and since 1950 with two disciplines that did not exist in and of themselves before they were talked about: sociobiology (the word indicating until then a subset of ethology) and behavioral ecology (a field apparently named for the occasion). The latter served above all to emphasize that, from now on, behavioral research would set up camp within the framework of neo-Darwinism. The tremendous controversy that ensued not only failed to even slightly hinder the formation of an intellectual community in support of Wilson’s theses, but it acted like a rallying cry. Thus, some months later, a journal was created, its title dedicated to the new structure of discourse: *Behavioral Ecology and Sociobiology*. Several others would follow.

A little later on, in collaboration with Charles J. Lumsden, Wilson added a final important item to the record when he published *Genes, Mind, and Culture*, a reflection based on an *a priori*, non-provocative concept: “gene/culture coevolution.”¹²

The discovery that human genotypes and cultural systems had evolved independently came as a surprise, and even an ideological adversary convinced of biological determinism had to admit that there was nothing unusual about taking the time to examine their interaction. Distrust actually subsided, since the study presented a poll of sorts conducted by American researchers: out of 151 specialists in cultural anthropology who responded to the question, 49 percent praised the promising and useful nature of this idea as opposed to 26 percent who were against it.¹³ The opinions would likely have been less positive if they had been aware of the fact that, according to Lumsden and Wilson, the definition of a geneculture coevolution required the creation of a cultural counterpart of the gene: the *culturgen*, a base unit of culture, analogous to the famous *meme* “invented” by Richard Dawkins in 1976.¹⁴ An indispensable tool for measuring the greatly hoped-for

“gene/culture aptitude”! Britta Rupp-Eisenreich will comment on the history of this repetition fantasy in this issue.

The gene/culture coevolution narrowly escaped controversy. It also escaped debate, and this view of cultural atoms, though contrary to all the beliefs that had been accepted by social anthropology since Marcel Mauss’s complete social event and Alfred R. Radcliffe-Brown’s social structures, was established as an uncontested concept in scientific literature. In retrospect, sociobiology won its most stunning victory with this, but the confusion that ensued among contemporary anthropologists brought with it an equal responsibility toward the presumed arrogance of zoologists. Be that as it may, from that point on, research on “group selection” of behavioral traits in our species settled back into the human sciences, and the explicit reference to sociobiology was no longer requisite.

Let us mention, among the most recent of them, the publication of an “empirical test” in New Guinea, which was warmly received by diverse points of view: the authors concluded that group selection of a simple beneficial trait for the community (such as an efficient means of resolving internal conflict) would require at least five centuries.¹⁵ Many Anglo-Saxons appear to have approved of the article’s careful and moderated argumentation. Nevertheless, if it had been presented in a French journal on biological anthropology, one does not dare imagine the reception it would have been given: cutting up a social organization into small or barely isolable functional traits would be a tremendous pitfall, and no one would accept (at the present moment) that an analysis authorized itself to overlook this. Certainly, outside of postmodern intellectual circles (whose tributes are admittedly not without reservation), the French human sciences no longer enjoy great prestige in the United States. If one wants, despite everything, to grant them an important role in the history of anthropology, it would be in our best interest to ask ourselves what such a sudden and profound imbalance indicates about the sturdiness of our “community”: can a science withstand such scissions over the long term? And are the French the only ones at fault?

The last point to note in this brief summary: in the 1980s, sociobiology was thought to be denying its theoretical soul in order to

pose as a discipline that was open to multiple trends of thought. An effective strategy that allowed numerous European ethologists (in fact, almost all of them) who had at first resolutely condemned the movement to then rejoin it by claiming that the only rational stance consisted from now on in remedying sociobiology “from the inside,” since its existence was now undeniable. This turnaround, a long way from the previously witnessed impassioned declarations of principle, was achieved very discreetly. This would only be a means of hiding the sophism that brought the turnaround about: it was not sufficient for a field of analyses to tolerate several contradictory intuitions so that it could be legitimately called a disciplinary field. The existence of a discipline implies a known subject matter and the free participation of all of the scientific problematics that it describes, without any prior restrictions. Sociobiology would be a true discipline if it intended to deal with the relationship between biological and social facts without instantly yielding to every conjecture offered by genetics and population dynamics.

No one would dream of denying that life is present in society, that nature has not stopped (nor will it ever stop) influencing culture, that human evolution is concerned with history. But that origins contain the seeds of their consequences and that the power of causes persist in their effects is a completely different matter. Over time, the atom created the gene that created society, but the original impulse that instigated the development of the structure did not govern it indefinitely: it could only succeed in this if this structure was an illusion. Reductionism tends to invent “disciplines” in which it is necessary to confirm an uncertain result at the outset. Sociobiology does not, as it claims, begin with the affirmation that the goal of life is the proliferation of life: it implies that society’s goal is the proliferation of life, which is not deducible from the assertion it begins with.

A mean-spirited exaggeration? A caricature? Let us read the significant opening sentence (which, as usual, shows little interest in concise reasoning) that the anthropologist Laura Betzig presents to us in the preface of a collection of outstanding articles on human sociobiology: [“We did it. We have finally discovered where we came from, why we are here, and who we are.”]¹⁶ Betzig explains that, to begin with, it is thanks to Darwin, and at the end

of the day it is thanks to the sociobiologists: they taught us that the root of the problem is “gene fertility.” For the provenance still fades away, but is salvaging the two other questions not similar to skidding out of control around a turn that has never lacked road signs? Contrary to serious arguments, whims still make sense.

The Failed Debate

After several decades of relative neglect, the strong return of a problematic is not in and of itself enough to prove the truth of the ideological content of the rediscovered thesis. Otherwise a simple historical comparison of the abundant thematic correspondence between Spencer’s time and ours would have been enough to enlighten us. After all, science is within its rights when it rejects a proposal style because of apparently immovable technical obstacles, and later restores it to favor because a development in information has contradicted the reality or the impermeability of these barriers. The possibility that the error of “social Darwinism” was that it worked on a justifiable intuition with deplorable tools must be willingly confronted, and it will surely be regrettable if current sociobiology, or one of its satellites, is condemned before ensuring that its own tools are capable of tackling the task at hand.

Likewise, one of the worst uses of the history of sciences was without a doubt the one that intended to disqualify epigones from sociological evolutionism by the sole reminder of the depravities of which the former followers were guilty (such as the frequent complicities with eugenics and collusions with racism). Patrick Tort tells us later in this issue that if an ideology cannot originate from a science, it can, on the other hand, contribute to the creation of the science that originally split off from it. As far as a reflex is concerned, the rejection of new biologisms, instead and on behalf of outdated biologisms, is potentially as absurd as it would be for astronomers to do so on account of their original collaboration with astrologers: historians who surrendered themselves to the analogy for one hundred or one thousand years are no less condemnable than the naturalists were when they blindly put their faith in it for millions of years. Worse yet, confining curiosity

about the past in a search for intrinsically instructive “precedents” amounts to depriving oneself of an education while renouncing the contributions of a thorough comparison.

For the most part, opponents of sociobiology have committed this error, and their oversight has largely contributed to the success of those they wanted to oppose: instead of drawing theoretical homologies between “social Darwinism” and its reconstruction, or making an inventory of the active dissimilarities in their respective scientific contexts, they accumulated superficial analogies, thus falling into the same tendency that doomed their opponents. The sociobiologists were unimaginatively compared to Konrad Lorenz and his innatist ethology without considering the fact that, beyond their very similar ambitions, the sociobiologists quickly demonstrated a superior efficiency by drawing an audience in the social sciences. We should start then by asking ourselves where this unusual ability of persuasion came from and why sociologists never felt the need to respond to Lorenz even though they rushed to respond to Wilson. Against all likelihood, they also wanted to denounce the use of genetics dating back to the 1930s (the famous “bean-bag genetics,” which more or less computed a function for each gene and a gene for each function): a convenient way to avoid questioning oneself for the present time about the excessive confidence in population dynamics. The evocation of the *culturgen* noted earlier sufficiently points out that one would have been better advised to find that the geneticists in particular tended to impose the image of cultural facts “in bean bags,” of modes of behavior “in bean bags,” or of ecological niches “in bean bags.”

And then, one wondered melodramatically if biologists would “seize power”;¹⁷ there was talk of prohibiting the continuation of the project (notably in the wake of the collective *Science for the People*); the accused were called racist and sexist, and their condemnation was based on evidence or intuition, etc. In short, in this first hazy controversy, the accusers engaged in a balance of power that went beyond the initial debate, as if their deep-rooted fear was that sociobiology was “true”: ill-fated politically, but scientifically competent. Supporters of the theory under attack had it easy: they stood on their injured pride, and their inflexibility in the face of the ideological corruption of which they were the victims allowed them to avoid

responding to embarrassing critiques, pretending to be unable to distinguish them from the stream of invectives. The bucket of water that was overturned on the head of one of their principal mentors by a hysterical group in 1978 has become the symbol of their martyrdom and the permanent defense for their objections.

Suddenly the protestors found themselves once again in the role of suspects. Except that the lampoonists and tribunes who called on the people to bear witness had left the battlefield at the first signs of public boredom (in order to join with others, more amply supplied with war correspondents). The irreducibles of anti-reductionism that persisted in this devastated field were no less controversial nor less worrying than their rowdy precursors: they only thought that a problem is not resolved by hastily throwing together a definition. Unfortunately, the economic crisis seemed to erode the ethical prudence of the world of popular science, impelling it to share the impulses of the tabloids, and today the public has a right to believe that as far as evolution is concerned, reality comes down to two alternatives: sociobiology or mysticism.

The usurer gene or divine finality. Insofar as the "end of ideologies" had been irrefutably certified, proclaimed, and ratified, something still engendered a certain uneasiness. Besides, the revival of this age-old antagonism benefitted from a favorable union: not only was reductionism no longer steadily confronted by criticism, but its scientific self-satisfaction lived comfortably with a disheveled relativism, announcing to all who would listen that when all was said and done, the sciences were only beliefs, no better or worse than those held by sorcerers. Under these conditions, what was the use in getting angry? In retrospect, it is ironic to think that Paul Feyerabend's famous *Against Method*¹⁸ appeared in the same year as Wilson's *New Synthesis*. Yesterday's radical scientists have vanished and their successors are clearly more refined: the failed discussion gave way to a marked overall contempt for the debate. For several years, human sociobiology thus carried on with its venture in isolation, not having to respond to anyone outside its own community.

From this angle, the sensitive spot finally shows up in the current atmosphere of unease: the inherent danger in sociobiology is incomprehensible to anyone who disregards the weakness of its

detractors. *The harmfulness of reductionism and the fragility of the social sciences make an indissociable couple.* The eagerness to denounce the depravity of Wilson and his colleagues came from an equal unspoken inability to contradict it. The infrequent argumentations that rise to the challenge without pretense take refuge in the uncommon nature of humankind or in the mysterious energy of its “symbolic capacity,”¹⁹ thus admitting that they cannot construct an objection from the social event itself: the ultimate shield has always been the psyche, which is somewhat ambiguous in the framework of a claim turned toward the insurmountability of the social event. Is it not true that many anthropologists have profited from the supremacy of Durkheimian theses by sparing themselves the effort of interdisciplinary knowledge? Have they not neglected the puzzle of the formation of the human social event over the course of evolution because they were more attracted by other themes? If so, they themselves have let the fissure in which “social Darwinism” has reappeared widen.

Similarly, in 1994, *The Bell Curve* by Charles Murray and Richard Herrnstein provoked an all-consuming though brief scandal: the racist content was immediately exposed and deactivated by a list of evidence showing the authors’ incompetence. In this case there was no need to intermix the refutation and the moral accusation. A scholar does not need to use military might in order to inform a citizen. Refraining from morally condemning the sociobiologists did not mean justifying their thoughts: ideological obsession cannot be considered on par with individual delirium since it is a social phenomenon. A scientific community commits a serious error in dismissing an ideological movement and a reactionary fad in the same way. An almost comical paradox was created: those who wanted to define the autonomy of the culture and the invincibility of the social have spontaneously treated a socio-cultural resurgence like a psychological anomaly! What ensued were personalized confrontations, duels of illustrations, and a history of the sciences revolving around anecdotal memory.

As a consequence, sociobiology concealed its worst hypocrisy with complete peace of mind: it presented the inconsistencies of its opponents as an indication of its own straightforwardness and its ideological purity, when the relationship between these two

aspects was not compulsory; and at no time did it feel the need to disassociate itself from the openly moralistic discourse that was being conducted under its aegis. Would it not have been necessary to return to this theory in order to discredit each of the attempts to ideologically rehabilitate “social Darwinism,” which claimed its scientific backing, since sociobiology itself maintained its complete independence in this respect? On the behalf of honest and honorable scholars, shouldn’t one expect that, with the risk of their theses being taken over by a school of thought that history has proved to be too harmful, they would vigorously oppose? As far as this is concerned, at the very most one notes that biological fatalism takes care to announce its intention with the introductory term “politically correct” (which is, most often, meaningless).²⁰

The roots of anxiety extend beyond a single school: thus, although genetics today is wary of praising the merits of eugenism, not long ago its biggest names wished to, and certain current theoretical views are still imbued with it.²¹ At a time when “historians” tend to uphold the view that there are wise and honest eugenicists, not to be confused with the unhealthy versions, biologism’s lack of effort to disassociate itself from the past adds to the uneasy atmosphere.

Method in Dialogue and Dialogue from Method

There remains a considerable number of researchers who do not intend to revel in the pitiful decay of the peaceful coexistence between scientism and relativism, between innatism and culturalism, and between determinism and possibilism. For them, a real question remains, in light of the re-ascent of biologism: is there a means to distinguish between that which belongs to the scientific reformulation and that which concerns ideological obsessive fear?

Fortunately there is. Durkheim’s saying indicated this at the outset: when a theory involves the chronic repetition of a basic error in logic, it is essential to ask for an explanation of the subject. The frantic use of analogy and its discrete substitution for comparison are traditionally causes for the diversion of sociological reasoning. Assuming that it might sometimes be essential to devote

oneself temporarily to the use of an invigorating conjecture, it is advisable to take care that the expedient not be disguised as a legitimate tool of production. At this point, a lapse in vigilance signals a state of scientific crisis which, Durkheim repeats, greatly exceeds the responsibility of the offenders.

Now, sociobiology does not content itself with amassing analogies while pretending to believe that their accumulation will end up resembling a proof. It renews the same analogies and the same type of induction whose aberration Durkheim has exposed. And all that without responding to the critics who have discredited them in the past. To think that there was a time limit given on refutations. On these grounds, most certainly, the history of sciences is apt to contribute a good deal more to scientific practice than a collection of anecdotes will. The way in which sociobiologists have toned down their rhetoric is proof enough.

In a rather curious epistemological flight, the American anthropologist Robin Fox²² unearthed for his compatriots (after Wilson, however) the great figure of Alfred Espinas, who in 1877 created the first sociological thesis recognized as such by the French University: *Des sociétés animales*.²³ In spite of what Fox wrote, Durkheim was never Espinas's student. After this, the confrontation between these two thinkers necessarily implicated the presence of a third intermediary: Gabriel de Tarde who, opposed to a union between sociology and the series of natural sciences (Espinas), or an independent will (Durkheim), spoke out for a connection with psychology. Now, Tarde and his "interpsychology" was in the midst of bringing out, each in turn, forgotten elements through the "cognitive sciences." Finally, one would have liked to know the reason why determined opponents of "biological sociology," which Tarde and Durkheim were (and also Célestin Bouglé), systematically positioned Espinas outside of this movement despite his open admiration for Spencer: his research on animal societies wholeheartedly made use of the analogical shift, but with this methodological peculiarity that he held to be genuinely instructive at the same time that the observation prohibited his proceedings. Espinas was not looking for a law to carry him effortlessly from the amoeba up to man: he was lying in wait for the times when nature manifested radical transformations.

By shelling the culture in order to put it into a bean bag, by transforming the conditions (suicidal societies do not last) into ultimate causes (the society is founded upon a concern for genes' fertility), by multiplying statistical correlations so that a display of figures masks the guess work, biologism does the opposite; on the subject of man, on the subject of monkeys (Bernard Thierry later mentions the exemplary case of gray langurs), but also in the field in which they are presumed unassailable, that of social insects.

Let us sum up the problems that their symbolic success brings up: the analysis of hymenopteran societies according to the fact that the males, born from nonfertilized eggs, have half as many chromosomes as the females (this predisposition is called *haplodiploidy*). The degree of resemblance is not identical for sisters, a brother and his sister, or a sister and her brother. Hamilton then drew a close correlation between the variation of altruistic behaviors and those of the grades of kinship between the actor and the beneficiary. Wilson elaborated on this discovery: complex societies appear about twelve times among the hymenopterans, whereas they only appear a single time among the other insect orders (termites). Their mode of sex determination is then an eminently favorable factor for the birth of the social, which confirms the idea of a regulated genetic altruism. The thesis looks good, apart from a couple of details:

- 1) The possibility of some dozen emergences of social behavior among the hymenopterans clearly does not come from paleontological knowledge: as far as this is concerned, it comes solely from the heterogeneousness of bees and wasps (a series of social species side by side with a series of solitary species). In other words, the unproven hypothesis is that evolution can drive a solitary species to become social, but not the inverse: Hamilton's explanation must already be accepted in order to believe this without reservation.
- 2) Wilson considered the existence of termite societies while calling on a radically different kind of occurrence (the protozoa who live in symbiosis with them).
- 3) In spite of their differences (for example, termites have a progressive development, hymenopterans go through a meta-

morphosis), the societies present in the two orders have disturbing homologies: differentiation into three “castes,” in which one caste consists of soldiers, etc.

- 4) Now, there is a biological criterion that termites share with hymenopterans and which plays an obviously important role in their social organization: grinding mandibles which facilitate building activities.²⁴ All of these species build collective nests, whose complexity, we know, grows with that of the society. Wilson rather absent-mindedly mentions it, and does not stop there.

In other words, sociobiologists simply missed out on at least one means of interpretation that *logically had priority*;²⁵ after which all discoveries have drifted across this shaky problematic in order to “confirm” the relevance of Hamilton’s rules. Mole rat colonies corroborate this perfectly: incidentally, these little mammals live in collectively hollowed out underground tunnels. And the revelation of a “kind of” soldier caste among certain aphids was also an occasion to celebrate, although these animals do not form societies (and their buccal parts render them incapable of building).

The gap between scientific activity and ideological work can be clearly viewed here. Provided that it is not construed as proof, Hamilton’s hypothesis is scientifically admissible for anthills and swarms: no argument can deny a certain intervention of haplodiploidy in these social organizations. At what level remains to be seen. Then, when this conjecture gains a prominent position, escaping the potential competition of other research programs, a momentary error can be imagined. On the other hand, ideological parasitism must be suspected when one sees that the remaining option offers itself to an analogical voyage, and that the “forgotten” option (more or less meaning the building ability) resists all kinds of applications. This suspicion is greatly reinforced by the subsequent infatuation of a great number of universities that goes beyond the issuing discipline. And the homology between the system thus produces and the former system adds the piece that changes presumption into certainty: it is not a scientific reformulation, it is an obsessive fear.

Beginning with the Controversy So As to Be Able to Escape It

In this issue, it was crucial that the introductory text not avoid this controversy so that the articles that follow might be spared the risk of being imprisoned by it. Beginning with a preamble of this sort responds above all to the necessity of emphasizing an abandoned hope and a neglected fear. The hope is that the social sciences finally turn the warning shot that the revival of biologisms represents to good account, in order to organize their legitimate ambitions: they must calmly examine their overdiversification, their silences, and their deficiencies over the course of this century in order to respond as a group to the periodic attempts that aim to “refound” them from an external point of view. They will also have to question themselves about their extraordinary tendency to create gaps between national traditions, at the risk of seeing their dialogues break off. The fact that all of the contributions collected in the following pages are from French researchers, or from researchers who keep regular company with them (such as the prehistorian Randall White, to whom we, along with the Anglo-Saxons, are indebted for an introduction to the long-awaited arrival of the English translation of André Leroi-Gourham’s book, *Le Geste et la Parole*²⁶) stems in part from the recent widening of these gaps. But it is also destined to remedy it. Just this one time does no harm. The French scholars’ debate is clearly motivated by an international breakdown in communication in which no one can be presumed innocent. It is as if the regional pledges of interdisciplinarity intensify the geographical compartmentalization of theoretical practice.

As for the more pressing fear that brought the authors together, it dominates this issue. The spread of reductionism, from socio-biology up to the “cognitive sciences” (which readily brought the nature/nurture debate back into style), contains an underestimated harmfulness: it weakens, truncates, and puts a stop to promising projects. It diverts attention from less spectacular but more authentic new ideas. It demeans the task of popularization by accustoming it to sensational intoxication. Ever since sociobiology came into being, there has been no more talk of socioecology:

it is sufficient, however, to read publications from the 1960s and 1970s to see that socioecology held a much wider and more stimulating field of interest for the social sciences. Since the arrival of behavioral ecology, ecoethology has dissolved: curiosity has abated. It is difficult to get rid of the feeling that our sciences have heretofore wanted ... to turn the new into the old.

The objective is not then to attack contemporary reductionists by drawing up a list of their errors, nor to hold them individually responsible for a general decline in sociological and anthropological thought. More modestly, we should content ourselves with approaching a response strategy that consists of a defense of the right to quote research themes that the public is not prepared to expect or desire. By effecting a few "soundings" in febrile sectors: those who are military recruits and those who are relieved from their duties by the debates. The juxtaposition of the well-founded questioning of genetics, ethology, prehistory, anthropology, history of sciences, and epistemology already appears unusual and promising. Pierre Darlu's biological anthropology and the animal sociology of Bernard Thierry are not focusing on the same horizon, and this is why it is important that they be placed side by side here: we should distrust any biology containing a "fundamental" message to free us and, for that, we should listen more closely to the real diversity in their lessons. As for the prehistory of techniques and the prehistory of art, they are threatened by a fracture that could drive each one to talk about the nature/nurture relationship without worrying about what the other one says: the contributions of Frédéric Joulain and Randall White incur the subsequent risk (a submission to ethnology's oldest prejudices) and provide the kind of original contributions that one hopes for in their field. It is thus symptomatic that the prehistory of symbols, still relatively spared by the sirens of biology, has once again started singing the refrains of "art for art's sake" and functional art. One other comparison, brought forth in a text by Britta Rupp-Eisenreich analyzing other "repetitive music" in the field of pseudo-Darwinism, concludes our roundup: that of an anthropology that is aware of its epistemological content (Jean-Luc Jamard) and of an epistemological carrier of anthropology (Patrick Tort).

The social sciences and anthropology constantly endure a reproach prompted by technocracy: they are sciences paid to do nothing. In theory, this is false. In practice, this could prove to be accurate if we do not react to the multiplication of prefabricated “disciplines,” most certainly more sympathetic to technocracy, which closely resemble sciences that are funded so that nothing gets done.

Translated from the French by Ann La Vigne

Notes

1. Émile Durkheim, *Sociology and Philosophy*, trans. D.F. Pocock (Glencoe, IL, 1953), p. 1.
2. Patrick Tort, *La Pensée hiérarchique et l'Évolution* (Paris, 1983), p. 539.
3. Inasmuch as the expression “social Darwinism” is paradoxical, as Patrick Tort noted, in that Darwinism only becomes social when it is no longer Darwinism, it is best to keep the quotation marks in each occurrence.
4. For example, Shirley Strum and Bruno Latour, “Redefining the Social Link: From Baboons to Humans,” *Social Science Information* 26 (1987): 783-802.
5. Herbert Spencer, *Les Bases de la Morale Évolutionniste* (Paris, 1880), p. 176.
6. John B.S. Haldane, *The Causes of Evolution* (New York, 1932).
7. John B.S. Haldane, “Population Genetics,” *New Biology* 18 (1955): 34-51.
8. John Maynard-Smith, “Group Selection and Kin Selection,” *Nature* 201 (1964): 1145-1147; William D. Hamilton, “The Genetical Evolution of Social Behavior,” *Journal of Theoretical Biology* 7 (1964): 1-52.
9. Robert L. Trivers, “The Evolution of Reciprocal Altruism,” *Quarterly Review of Biology* 46 (1971): 35-57.
10. John F. Eisenberg and Wilton S. Dillon (eds.), *Man and Beast: Comparative Social Behavior* (Washington, D.C., 1971).
11. Edward O. Wilson, *Sociobiology: The New Synthesis* (Cambridge, MA, 1975).
12. Charles J. Lumsden and Edward O. Wilson, *Genes, Mind, and Culture: The Co-evolutionary Process* (Cambridge, MA, 1981).
13. Leonard Lieberman, “A Discipline Divided: Acceptance of Human Sociobiological Concepts in Anthropology,” *Current Anthropology* 30 (1989): 676-682.
14. Richard Dawkins, *The Selfish Gene* (Oxford, 1976).
15. Joseph Soltis, Robert Boyd, and Peter J. Richerson, “Can Group-Functional Behaviors Evolve by Cultural Group Selection? An Empirical Test” with comments, *Current Anthropology* 36 (1995): 473-494.
16. Laura Betzig (ed.), *Human Nature: A Critical Reader* (Oxford, 1997), p. xi.

17. Pierre Thuillier, *Les biologistes vont-ils prendre le pouvoir? La sociobiologie en question* (Bruxelles, 1981).
18. Paul Feyerabend, *Against Method* (London, 1975).
19. Obviously one thinks here of Marshall Sahlins, *The Use and Abuse of Biology: An Anthropological Critique of Sociobiology* (London, 1977).
20. See, among recent publications, Del Thiessen, *Bitter-Sweet Destiny: The Stormy Evolution of Human Behavior* (New Brunswick, 1996).
21. Daniel Dreuil, "Entre science et eugénisme: le fardeau génétique," in P. Tort (ed.), *Darwinisme et société* (Paris, 1992), pp. 471-487.
22. Robin Fox, *The Search for Society: Quest for a Biosocial Science and Morality* (New Brunswick, 1989).
23. Alfred Espinas, *Des sociétés animales: étude de psychologie comparée* (Paris, 1924). The first edition in 1877 did not include the in-depth historical introduction since the thesis committee wanted the reference to Auguste Comte to be taken out.
24. See Michael H. Hansell, "Les nids des insectes sociaux," *La Recherche* 209 (1989): 14-22.
25. We have certainly intended to reduce this critique to its simplest expression. Other factors include both termites and hymenopterans in close association with building ability (group effects, stigmergie). On this point, as well as on the debate between Durkheim, Tarde, and Espinas, see Georges Guille-Escuret, *Le Décalage humain: le fait social dans l'évolution* (Paris, 1994).
26. Randall White, "Introduction," in André Leroi-Gourhan, *Gesture and Speech* (Cambridge, MA, 1993), pp. 1-21.