
From Altruism to Sociobiology: Historical and Epistemological Summary

1.1. Introduction

As a whole, biology, which seems to oscillate between superbly “hard” sciences (mathematics, physics, etc.) and sadly “soft” rationalizations (psychology, sociology, etc.), represents a minor, and even marginal, source of inspiration for the formal schools of epistemology. The fact that Darwinism does not comply with the famous criterion of refutability laid down by Karl Popper as a dividing line separating accomplished scientificity from less rigorous approaches probably increased the temptation for a certain avoidance. On the other hand, after the mid-20th Century, the prominent culture of naturalists attaches little importance to a regular exercise of epistemological discussion. However, especially in comparison with their colleagues attached to others fields, researchers committed to the study of living things tend to demonstrate a strange recklessness before the history of their own knowledge. Apart from a small handful of clan ancestors, such as Linné, Darwin and Mendel, common references quickly disappear, and a blatant absence of curiosity is apparent in respect of the former masters of the discipline next to it.

Yet, sociobiology represents, at first sight, an exception. It is true that the legitimacy of its problem was immediately strongly disputed from a necessarily “meta-scientific” point of view, but, even before these attacks, it had unusually taken care of justifying its program by incorporating it to the logics of a slow progression of ideas. Nevertheless, the reader will soon find that undeclared tactical calculations presided over these “pedagogic”

operations, blurring here and there an influence, or a major participation, and masking, as a result, embarrassing questions.

A commented historical summary should then allow the reader to understand, as the dates unraveled, the series of events that led to the powerful comeback of a secular conviction, as well as the decisive links between these episodes. A long technical restructuring was obstructed in order to reinforce the image of an unexpected discovery and hide what we must call the persistence of an ideological obsession. The assimilation of this sequence will greatly facilitate the reading of the following chapters.

1.2. The “social Darwinism” of the 19th Century

Our journey starts with the appearance of a category brutally adding itself to the one of naturalists: biologists, who rely on an additional skill. If an observation method is sufficient to describe things of nature, it is no longer the case from the moment we pretend to deal with living things: a theory is required explicitly or not.

– 1802: Lamarck and the invention of biology

The word biology was independently coined several times, in Germany and France, but the initiative of Jean-Baptiste de Lamarck in *Research on the Organization of Living Bodies* was the most retained, probably because of the prestige he earned through his book. Except that in this case, posterity was not duped: Lamarck insisted straight away on the double mission of the field supposed to contain botany and zoology, that is to say the complementary quest for unity and diversity of living things. Connecting both aspirations came to inscribe in science a reflection up to now reserved to theology: God no longer monopolized the responsibility of constants, and biology would work to identify them outside the initial laws of creation. The result was that “biology” implied an evolution that was likely to link unity and diversity.

Behind this attitude, which was beyond reproach in approach, the risk of a manipulation playing on ambiguity incidentally crept in: pretending that a phenomenon *was* biological came to put it under the authority of a unifying base, whereas the field of investigation of biology referred in priority to diversity. Thus, practical projects preferably relied on heterogeneity, while theoretical ambitions claimed to have a common nature, and the discreet reciprocation between both levels was open to numerous tactical tricks. Sociobiology used trickery even better than other schools of thought before

they did, by using diversity as a shield to justify its efforts, although the proclamation of its “unifying” law clearly preceded control by comparing the validity of its statement. We should remember this point when we focus on the “thunderbolt” of 1975: the voluminous “synthesis” published by Edward O. Wilson.

– **1809: Lamarck, evolution and inheritance**

With his *Zoological Philosophy*, which brilliantly finalized his work, Lamarck imposed the first full theory of evolution: marked, among other things, by the idea of a general direction going from simple to complex. One of the “laws” included in it primarily concerned here is *the inheritance of acquired characteristics*, according to which the action of “circumstances” can develop, or reduce, a trait from generation to generation. To some, even very reduced, extent, parents could then transmit something from the experience they lived to their descendants. Lamarck, according to some commentators, developed a widespread intuition among his contemporaries, who sensed the existence of evolution. In addition, the originality of his contribution was certainly not limited to this speculation. This door left ajar in the face of an inheritance bequeathed by ascendants through the fortunes and misfortunes experienced during their life would, nonetheless, represent the core of “social Darwinism” during the second half of the century: simply through an extrapolation of behavioral dispositions.

– **1830 (?¹): Comte and “altruism”**

Auguste Comte, the inventor of “sociology”, also introduced the idea of *altruism*, outside an evolutionary perspective. It was still unclear when it came about: probably coined in the 1930s, the word was however certified in 1852, within the “positivist catechism”. Although it was found that some representatives of “social Darwinism”, starting with Spencer, whom we will talk about in the following, read Comte, we ignore in which way and at what speed this notion spread.

– **1855: Spencer and evolutionism**

According to Spencer [SPE 87], his understanding of a general doctrine of evolution was essentially built in 1855, when he was working on *The Principles of Psychology*: built and unfinished, because he worked on it until his death. The significance of this landmark in time came from the fact that

1 No one is sure of the exact date. In fact, it seems that Comte spoke of altruism in 1830, but in oral lessons.

we would then remember evolutionism² (or, from the outside and pejoratively, “*social Darwinism*”) as clearly preceding the hatching of Darwinian science. The parasitic influence that it will have on it, will then prove even more swift and efficient as, far from being reduced to a subservient extrapolation of the bulk, the metaphysical theory infiltrated in it an interpretation previously instructed: we are dealing with an impregnation here, not an addition.

The double conviction of an underlying progression from simple to complex, and from homogeneous to heterogeneous, highlighted the hierarchical orientation of this thought. We must add to it *organicism*, that is to say the double assimilation from society to organism, and from organism to society: in the 20th Century, even though it was devalued everywhere else, this inspiration would remain prominent among entomologists, who would identify hives, anthills and termite mounds as “superorganisms”, with individuals instead of cells.

– **1859: Darwin and natural selection**

The nearly simultaneous and, nevertheless, independent revelations of natural selection by Alfred Russell Wallace and Charles Darwin made quite an impression, and the publication of *On the Origin of Species* [DAR 59] had an immediate impact, which spread throughout Western Europe. Two types of debate lied ahead. The first one dominated the following decades, and concerned the unavoidable challenge of the creationist dogma of the Christian religion (and then, of the whole monotheism). The second one fell beyond this outlet: in a perspective excluding religious authority, all the thinkers understood that, from now on, a human theory claimed to be in continuity with a nature theory, and vice versa. Spencer was not the only one to consider things from this point of view: less than a month after the publication of his book, Friedrich Engels, in a letter to Karl Marx, showed his enthusiasm about the “historical development in nature” highlighted by the English naturalist [MAR 73, p. 19].

The main theory of Darwin relied on three main concepts. Each gave rise to numerous deformations: *natural selection* performed a synthesis of the *struggle for existence*, which a given species within a given natural environment faced (so identified in space and time), and it resulted from a

2 As an ideological extrapolation modifying the scientific theory of transformism. However, nowadays, numerous biologists abandon the second label and assert their scientificity as evolutionists.

place in nature. In contemporary vocabulary, this last notion is perfectly reflected by the notion of “ecological niche”, in which the struggle for existence represents “dimensions” [HUT 65]. The innovative power of Darwinism resided, in fact, in its capacity to reflect processes of evolution through ecology (which it creates) and ecological situations through the solidarity of evolutions (see [HAE 66, HAE 84], further in the book). On the other hand, “social Darwinism” would play on two levels:

– Darwin did not deny the possibility of a Lamarckian inheritance, which allows us to think that his contribution can be added to without being harmed.

– The struggle for existence that was formed between organisms and all the environmental factors to which they were exposed (climate, predators, parasites, etc.) was tacitly curtailed: reduced to the sole competitions between individuals of the same species. It only expressed social competitions and economic competitions. Consequently, nature suggested a metaphor of the capitalist market: an image, which was particularly obsessive in North America in the 20th Century.

– **1865: Mendel and genes**

Everybody knows that Gregor Mendel’s experiments went unheeded for 35 years. Apart from the relatively confidential support for their publication, a probable pattern of indifference arose from the confidence granted to the Lamarckian inheritance, which was attractive and suitable to mask the challenge of the discoveries made by the botanist.

– **1866 and 1868: Haeckel and oecology**

If Spencer is the undeniable master of “social Darwinism” in philosophy, Ernst Haeckel can claim the title for being the prominent figure among biologists. As he quickly understood that the quite original method presiding over the conception of natural selection requested a reorganization of life sciences, he deduced the need for a new discipline, responsible for studying the relationships between organisms and all the components (biotic or non-biotic) of their environment: *oecology*, simplified later as ecology. The two definitions he gave for it in 1866 and 1868 are remarkable as, despite hundreds of subsequent attempts from every horizon, they maintain the highest authority [HAE 66, HAE 84].

Yet, in the second occurrence, after having perfectly well defined the field of ecology because of a lucid reading of Darwin, Haeckel allowed himself to

inaugurate the travesty by including, a few pages further, a particular case: humankind. The comparisons he outlined brutally left the ecological field to restrict themselves to the behavioral register: this characteristic narrowing would even lead the biologists from the beginning of the 20th Century to wonder about his real objective: ecology or ethology? In reality, far from being confused, he was modifying the aim in order to deduct a “*place for humankind in nature*”, under the flag of Darwinian ecology, but with the tools of racial, or even openly racist, psychology, since it was true that the influence of Haeckel on biological anthropology in Germany was significant. After 1975, this reminiscence triggered hasty or basic suspicions, when sociobiology found itself in the hot seat. Racist convictions can indeed draw a certain benefit from such a revival: this does not instantly mean that sociobiology defines them by itself.

– 1871 a: *Darwin and the Descent of Man*

Twelve years after *On the Origin of Species...*, Darwin published *The Descent of Man* [DAR 71], an event that was lastingly experienced by the public as the expected crossing of a threshold: the admission of the human branch within the whole of the beings likely to “experience” evolution. It seemed only appropriate, but, within the turmoil of the debates, protagonists expected from “the inventor” an official commitment and the open selection of a party. Unfortunately, this point eclipsed the content of the book in other respects, except for a large part dedicated to sexual selection (“social Darwinism” made haste to sever this aspect from natural selection, artificially giving it an unseemly autonomy).

However, the worst was that the finally stated theory did not attract any interest in any way, and the author was allocated with a grossly reductionist, and even “racist”, mindset, which was eager to turn humankind into a beast and humiliate its originality in the history of nature. In addition, this lasted for more than a century; since it is in 1983 that the epistemologist Patrick Tort, relying on the argumentative logics of the text, unearthed the unnoticed conclusion, which is now concentrated in one phrase: “*the reverse effect of evolution*” [TOR 83]. In short, Tort clarified that natural selection, which is the guiding principle of evolution implying the elimination of the less able individuals in the struggle for existence, selects within humankind a form of social life whose progress toward civilization tends to exclude more and more qualifying behaviors through the joint use of ethics and institutions.³

³ Tort commented and detailed this theory in the preface of his publication of the *Descent of Man* [TOR 99] and in numerous other books.

Thus, natural selection selects civilization, which *is against* natural selection, which results in protecting the weak instead of eliminating them. When a disability or crisis, which “should” put an end to a life that has become incapable of self-protection on its own, assistance, care and help are regularly provided by the members of the group, who compensate for the infirmity.

– **1871 b: Tylor and culture**

The anthropologic idea of culture appeared the same year with the definition given by Edward B. Tylor in the very first sentence of *Primitive Culture*: “culture or civilization understood in its broad sense is a complex set composed of knowledge, belief, art, morals, law, customs and all other skills and habits acquired by a human as a member of a society” [TYL 71]. In fact, Tylor used the proposition of a German author.

During the following decades, “culture” and “civilization” were more or less dissociated according to countries and sciences; the first finally revolving around the differences between ways of life, and the second aiming at connoting a level of cultural complexity. This contributed to marginalize the remote perspective defended by Darwin from “social instincts”.

– **1879: Spencer and the ethics of altruism**

The publication of *The Data of Ethics* by Spencer showed that, now, evolutionism had a perfectly clear vision of the image to be presented regarding the duo egoism/altruism. A few sections are enough to show why sociobiology would have trouble to deny this inheritance:

– “the efforts to the benefit of the race really represent a part of the substance of parents” [SPE 80, p. 175];

– “there is a gradual progress from altruism in the family to social altruism”⁴ [SPE 80, p. 176];

– “since the origin of life, egoism has depended on altruism, as altruism has depended on egoism, and in the course of evolution, the reciprocal services of one and the other increased”³ [SPE 80, p. 185].

For his part, Haeckel wrote, during the following years, equivalent convictions, praising the exact equilibrium between egoism and altruism.

4 Authors’ translations from the French edition.

– **1883: Weismann and neo-Darwinism**

Suddenly, August Weismann published a text about the continuity of the germ plasm, where he categorically denied the conjecture on the inheritance of acquired characteristics. Natural selection, freed from any competition, irresistibly monopolized the interpretation of an irreversible transformation of organisms. Neo-Darwinism is not an extended or complemented Darwinism: it is, on the contrary, a Darwinism from which Lamarckism was *removed*. The new situation tends to require a restructuring of life sciences, which would take several decades.

Although, in practice, ecology was barely starting, it theoretically had to endorse the status of an *essential* field of research, since the environment constantly sorted, with each generation, the “acceptable” and “unacceptable” individual variations. Lamarckism had implied that environmental variables acted on an ad hoc basis. Neo-Darwinism would have to break away from this intuition, and develop an external structure within which natural selection produced its syntheses. Another German scientist, Karl Möbius, had started to prepare this reflection by proposing, as early as 1877, the notion of *biocoenosis*.

As for “social Darwinism”, it obviously received a crippling blow there: the authority of biology no longer guaranteed that the development of altruism, or of other social instincts, benefited from the relay of an inheritance.

– **From 1890 to WWI: reorganizations**

Spencer and Haeckel, among many others, resisted this adversity, questioning inch by inch the validity of the limit indicated by Weismann. In vain: confirmations prevailed and caused various restructuring of the intellectual scene. Of course, in various Western societies, starting with the United States, the denial was ignored and the ideological conviction kept spreading a homology between the natural struggle for existence and the fierce competition in a market economy. This representation still remains nowadays widely prominent, under hardly watered down forms, and seeks to legitimize economic liberalism on a global scale.

However, at a university level, the protection of this scheme proved to be harder. Thus, in France, the eminent organicist and evolutionist power of “biological sociology” crumbled under the attacks of Gabriel de Tarde, and then Émile Durkheim. The latter created a French influential sociology, which admitted the autonomy of the social fact in the face of both the

biological fact and the psychological fact. Other oppositions arose, with less elaborated academic strategies: for example, that of Franz Boas, a German geographer, who, after staying with Eskimos, turned against the environmental determinism of his teachers, and created a cultural anthropology, which would become in the United States the main accuser against the divagations of biologism.

As for life sciences, naturally, Weismann's verdict influenced research more directly. Protected from the speculative hubbub on inneism and the origins of humankind, botanists methodologically took advantage of their flexibility to significantly get ahead and prepare the reorganization of a biology compliant with neo-Darwinian times: Mendel's laws were exhumed, when the independent works of three of them faltered at the dawn of the 20th Century. At the same time, they opened the field of a clearly imperial phytoecology, which would show for a long time some kind of condescension toward other districts, which were outrageously infected by the disorderliness of animals.

1.3. Reconquest of the quest: 20th Century social neo-Darwinism

The first third of the 20th Century was globally dedicated to these sectorial restructurings, up to the development, during the decade prior to WWII, of a “*synthetic theory of evolution*”, at the instigation of a dozen researchers attached to genetics, population dynamics and systematics. Around it, a mental map of zoology was reformed, where three thematic disciplines were constantly intercrossing: ethology, which is concerned with reintegrating the issue of behavior in natural environments, was later added to genetics and ecology. Yet, on closer examination, this triptych reproduced in the animal that which now dominates human sciences: genetics overlaps biological anthropology, ethology takes over the role of psychology, and ecology, which deals with a patchwork of interactions, ultimately takes the place of sociology. In fact, the ideological tensions that would disrupt one of the two triangles would loyally have an impact on the other [GUI 94], starting with the famous quarrel over what is innate and what is acquired.

– 1932: *Haldane and altruism. The return of aporia*

Among the founders of the synthetic theory, the charismatic British figure John B. S. Haldane stood firm on his rather “strong” positions, which were marked by his endless trust in the intrinsic power of genes. Symptomatically, from 1932, in *The Causes of Evolution*, he again expressed doubt about

altruism: how could the kingdom of competition governed by natural selection have promoted attitudes who benefit counterparts? According to him, the selective advantages of behaviors falling in this category would prove commendable if a significant portion of the tribe members had the disposition. As Ernst Mayr (another eminent contributor of the synthetic theory) tactfully indicated, the question remains as to how an altruist trait could raise at the level of expected frequency [MAY 74].

Up to now, the suggestion probably did not contain anything revolutionary, but the precocity of the reflection in itself, in an article by one of the movement leaders, brilliantly illustrated the untouched desire to bring back the logic of welfare under the umbrella of the biological cause. The dread of altruism remained because it contained the ultimate challenge: overcoming this obstacle meant defeating the greatest difficulty, thus taking over the challenge as a whole. In short, as the old proverb says: he who can do more, can do less.

– *1955 a: Haldane and altruism. The real discovery*

Twenty years later, Haldane came back to the subject with a vision that was a complete game changer:

“Let us suppose that you carry a rare gene which affects your behaviour so that you jump into a river and save a child, but you have one chance in ten of being drowned, while I do not possess the gene, and stand on the bank and watch the child drown. If the child is your own child or your brother or sister, there is an even chance that the child will also have the gene, so five such genes will be saved in children for one lost in an adult. If you save a grandchild or nephew, the advantage is only two and half to one. If you only save a first cousin, the effect is very slight. If you try to save your first cousin once removed the population is more likely to lose this valuable gene than to gain it” [HAL 55, p. 44].

The author continued by referring to two types of conditions that would be favorable to this basic scenario: small populations, such as the ones of Paleolithic humans, where children are nearly all close relatives of the one helping them, but also hives and anthills, whose members are literally brothers and sisters. William D. Hamilton, a few years later, would loyally follow this second “lead”.

A hole was open in the rampart raised by Weismann in the face of a selection of altruism. Here, we undoubtedly leave the social Lamarckism of Spencer and Haeckel to enter into an authentic *social Neo-Darwinism*. We will see by concluding this chapter that the geneticist discreetly made an unjustifiable choice within the core of his reasoning, under the form of a “deadlock”, and that his followers have absolutely never questioned this initial amputation of the reflection. Nevertheless, it has to be granted that this subtle and brilliant conjecture is enough to ensure the hoped reopening of the field: it makes the ungratefulness of those who will take the path thus revealed, even stranger. As, although that key moment has not completely fallen into oblivion at the end of the 20th Century, and that today we rather regularly quote his contribution, the geneticist owes it to the critics of sociobiology and not in any way to its followers⁵: what biologists call in unison the “Hamilton’s law” and should, by rights, be called the “Haldane’s conjecture”.

Of course, it was pointed out among the creators of the synthetic theory that this researcher embodied a strong determinist tendency, to which was wrongly or rightly opposed the wiser and more cautious position of Theodosius Dobzhansky. Furthermore, without dwelling on this point, Haldane was characterized as both a British geneticist and a communist, which, at this time more than nowadays, was a lot for a single man (especially because Stalinism rather disliked Darwinism and Mendel’s discipline). Thus, Richard Dawkins mentioned to any who would listen this contribution, indicating that it “preceded Hamilton” [DAW 76], whose merit would later be enhanced by a second exclusion, because Wilson’s rash declarations – “genes hold culture on a leash”, for example – succeeded to undermine the reputation of scientific neutrality, which was eagerly desired by sociobiology. Haldane was not a precursor: he did not anticipate the solution, which was to be reviewed; he invented it.

– 1955 b: *Julian H. Steward and cultural ecology*

Far from this event, in anthropology, one of the overseas masters of anthropology, Julian H. Steward, published an essay entitled *Theory of Culture Change* [STE 55], in which he encouraged the creation a new field: *cultural ecology*. The desire to do so was not his as such, but its expression grewed significantly and spread, and part of ethnology joined him in order to

⁵ In France, the ethologist Paul Hopkins insisted, as early as 1977, on this source in a widely distributed article, which made the avoidance of local sociobiologists even more sensitive about this subject [HOP 77a].

maintain interdisciplinary discussions with primatology, prehistory, paleontology and ecology. In 1966, the symposium, *Man the Hunter*, raised optimism to its climax by dismissing numerous prejudices about the primitive hunters [LEE 68].

Two reasons require us to insert this offset in our summary. The first one withered away in a short time: Steward defended the theory of a multilinear evolution, by branching out social organization forms on the model of classical Darwinian trees. The label of “*neo-evolutionism*” highlighted the perspective. The second one concerned practical consequences: the initial unanimity was disintegrated when finding contradictions: holism (or structuralism) versus functionalism, to name but one. Some saw the register of initial causes in ecology; others refused to *a priori* prioritize the diversity of facts to be put together. Under these conditions, sociobiology charmed a significant part of the cultural ecology supporters, including Irven DeVore.

– **1962: Vero C. Wynne-Edwards and group selection**

Animal dispersion in relation to social behaviour [WYN 62], written by the naturalist V. C. Wynne-Edwards, gave a warning shot, with a theory uniting the social behaviors of a species with a control of its environment tenure. Even though it was noticed and greatly discussed, the book did not convince anyone: the author argued in it the existence of a *group selection* (i.e. directly operating on communities), which did not have unanimous support, far from it, among geneticists or even among future sociobiologists. Especially since the mechanisms assessed in the book seemed sometimes unnecessary (because individual selection could lead to similar results) and sometimes insufficient. A minority of researchers would however continue in this direction, in numbers inversely proportional to the power of the “Selfish Gene” among biologists.

In fact, some kind of underlying competition crept within biological determinism between group selection and the leading role of altruism, for which it was difficult to find reasons: when confronted about the social organization target, one of the two concepts inevitably managed to plunge the other one to a lower level by absorbing it as a secondary aspect. From this point of view, it is not absurd to wonder – provided the answer is not established in advance – whether the success of sociobiology does not maintain a discrete relationship with the fact that the lack of knowledge in demography would be enough for a non-biologist to challenge a theory on

social relationships relying on group selection, while this lack would straight away inhibit it in the face of the problem revealed by Haldane.

– 1964: William D. Hamilton, John Maynard Smith and parental selection

It is precisely a criticism of Wynne-Edwards that led John Maynard Smith (a student of Haldane) to invent, contrary to the notion of group selection, the concept that stirred up a hornet's nest at short notice: kin selection, that is to say parental selection, where the chances of survival of an individual are reviewed with those of his/her parents, thus a direct follow-up of the conjecture issued in 1955 by his master [MAY 64]. The same year, an article in two parts by William D. Hamilton offered, for his part, the concept of global selective value (inclusive fitness), which expressed the same problem from a different angle [HAM 64]. Posterity will praise Hamilton's genius, while preferring to adopt Maynard Smith's denomination, who was not offended by it.

Why did Hamilton's contribution play the role of a beacon? No one remembers for certain that, in the end, he discussed the processing of "abnormalities" and the habits of emperor penguins. In addition, numerous other illustrations were similarly forgotten. Such as the introduction of a sort of "malevolent" action, on the same level as egoism and altruism, which was attractive at first and then gradually fell into abeyance. However, the echo of the excerpt on bees never declined: Haldane had suggested, at the end of his reflection, to look closer at social hymenoptera, and Hamilton followed his advice by remembering a detail characterizing their reproduction mode. In fact, hives and anthills operate under the mixed regime of *haplodiploidy*: males are born from unfertilized eggs and are said to be haploid, since they only have half as many chromosomes as diploid females (resulting from fertilization).

Hamilton then calculated the genetic proximity of the members of the "society": a female generally equally shares her genes between her father and mother, but haplodiploidy results in her having three-quarters of her genes in common with her sisters "compared to" only one-quarter with her brothers. To increase the amount of elementary components in the next generation, it is then in her best "interest" to cooperate to the best of her ability with her sisters, even more than with her daughters (with whom the degree of relatedness "remains" 1/2). A correlation emerges, which, for some minds duly prepared by a unique scientific culture, suddenly falls within a brilliant and exciting, even obsessive, obvious fact: haplodiploidy would encourage

altruism, which would itself facilitate sociality. In the previous sentence, we can replace the verbs “encouraging” and “facilitating” with another less modest one: “determining”. Anyhow, this time, the door was wide open for a biological evolution, where altruism would assume the role of a drive belt toward society: there certainly were many enthusiasts to rush into the hoped-for opening.

– *1966: John H. Crook and socioecology*

During this summary, we focus on the genesis of a renewed sociobiology. However, it is important here to insert an external scientific contribution: epistemologically essential, even though it was powerfully diminished during the end of the 20th Century, it is the competing school, which was ruined by the dazzling notoriety of social Neo-Darwinism. I would point out that this radiance reflected by both critics and admirers, in so far as bitter critics generally omitted to call upon this contrary competence, for reasons that will be revealed further.

Theories receive birth certificates more regularly than scientific programs, whose birth is sometimes noticed afterward. Let us say that it was in 1966 that John H. Crook and J. Stephen Gartlan published the article “Evolution of Primate Societies” in the journal *Nature* [CRO 66], which led zoologists to realize the ongoing set-up of a new field of studies, which anthropology, for its part, started to classify under the denomination of ethno-ecology: a global confrontation of social structuring and environment organization. It indeed concerned a subject of research defined outside a previous interpretation and, consequently, a disciplinary field as a means of reconciling diverging conjectures. Logically, sociobiology should then have been presented as a socioecological theory, but it deliberately refused this situation by presenting itself as a new “discipline”: although rough, the stratagem succeeded and guaranteed social Neo-Darwinism a significant advantage in the face of challenges, which were accused of wanting to repel a science instead of a theory. Few French ethologists, Jean-Yves Gautier in the lead [GAU 82], advocated a defense of socioecology against the abusive claims of sociobiology, but they did not resist the disintegration of the reference in English-speaking universities long.

– *1971: Sociobiology ready for battle*

Four years before the unrest that would trigger controversy, several events showed the growth of a thought impulse ready to greet it. The

publication of a symposium, held 2 years earlier under the leadership of the prestigious Smithsonian Institution, where Hamilton and Wilson met. Its title, *Man and Beast: Comparative Social Behavior* [EIS 71] indicated quite well the preparation of a future debate. Moreover, Wilson published at the time a splendid book, *The Insects Societies*, whose conclusion made the wish for a “united sociobiology” [WIL 71].

In addition, a young American biologist, Robert L. Trivers, complemented the series of explanations about altruism with a modeling of “reciprocal altruism” [TRI 71], which was warmly greeted, even though it seemed to rely more on group selection than on Haldane’s conjecture. We will see that it is regularly used as an “assisting force” or “spare wheel”.

Finally, anthropologists joined the movement, with Lionel Tiger and Robin Fox who published *The Imperial Animal* [TIG 71]. Four years later, Fox published the symposium *Biosocial Anthropology*, with the noteworthy participation of William Hamilton.

– 1972: Nobel Prize for Konrad Lorenz, Nikolaas Tinbergen and Karl Von Frisch

Then, a coincidence occurred, which was too striking not to be mentioned here. It was at this precise moment that classical ethology received a significant recognition with the award of the Nobel Prize of medicine to three German scientists. The first two, Lorenz and Tinbergen, created the discipline by breaking away from laboratory psychology, which involved experiments on animals in extremely impoverished contexts, therefore devoid of any meaning in terms of the lifestyle of the studied species. The reciprocation between observations and experiments restored the priority of environment. With Von Frisch, who discovered “the bee dance”, the reward acknowledged the major role of entomology in behavior analysis, while using the non-trivial support of social insects. This acknowledgement, which was greatly discussed by the mainstream press and audiovisual media, brutally attracted the attention of a broad audience, who ignored everything about this field: suddenly, ethology became trendy.

–1974: Sarah Blaffer Hrdy and infanticidal monkeys

Following the example of Trivers and Hamilton in their decisive moments, the primatologist Sarah Blaffer Hrdy was 28 years old when she

made the contribution, which assumed an essential part in the rise of the theory in the making. In India, she had observed infanticides committed by male temple monkeys (*Presbytis entellus*, also called grey Langur) after the conquest of the dominant position in a group –and the exclusion of the former leader –she explained this violence by the winner’s desire to exclude the genes of the banished former leader in order to replace them with its own [HRD 74]. Some saw in it the first stunning “confirmation” of “Hamilton’s law” among primates.⁶ We will come back to it in Chapter 3.

– 1975: *Edward O. Wilson and the thunderclap of the “new synthesis”*

Wilson published *Sociobiology: The New Synthesis* at Harvard [WIL 75], the leading university in the United States, which he apparently never left after defending his PhD: the place of issuance, the breeding ground of the nation’s elite, did not go unnoticed. The massive size of the book also attracted attention: 697 large pages, each with two columns, which would correspond to approximately 2,000 pages in conventional format. In addition, the bibliography with several thousand titles, which were often recent ones, concretely reinforced the claim of a “synthesis”. Furthermore, behind a wise-looking tripartition (“social evolution”, “social mechanisms”, “social species”), a talent for provocation crept in the titles of the first (“The Morality of the Gene”) and final chapters (“Man: from Sociobiology to Sociology”): a good invitation to ring alarm bells at a time when social sciences still easily frowned at the issue regarding their independence. Moreover, the first paragraph set the tone with a phrase destined to become famous:

“In a Darwinist sense the organism does not live for itself. Its primary function is not even to reproduce other organisms; it reproduces genes, and it serves as their temporary carrier” [WIL 75, p. 3].⁷

Far from the sharp-teethed juniors we reviewed before, this professor had not only already enjoyed a very prestigious reputation in ethology, but also at the junction between biogeography and ecology. Strangely, we rarely mention this second point, as if it concerned another world or another life.

6 The following year, Wilson did not mention this first article published by the zoologist, as he acquired another one “in press”.

7 Strangely, we often allocate the authorship of the words to Richard Dawkins in his book published a year later.

However, 12 years earlier, accompanied by Robert H. MacArthur, he started [MAC 63] and then refined [MAC 67] an innovative construction on long-term reproduction strategies, which still hovers nowadays over ecology as a main source of inspiration: the “dynamic equilibrium theory”. We can add that the authority of the zoologist in terms of social insects could not give rise to any challenge. In short, sociobiology could now line up behind an American General, with all the desired accreditations. It is clear that the devices of this founding book will require decrypting later, under different perspectives.

– 1976: *Dawkins and the Selfish Gene*

After the General, the Prophet: another figurehead emerged the following year with Richard Dawkins, whose image spectacularly improved over the years. Biologists first saw *The Selfish Gene* as a book aimed at the layperson, since it was true that Wilson already discovered the core of the matter by deploying a documentation that this junior could not compete with [DAW 89]. Progressively, Dawkins’ speculations and supputations, which were collected in a series of other essays, were greeted with more and more enthusiasm by sociobiologists, and the label of informed populizer was replaced by one of thinker and theoretician. Through this oscillation between scientific speech and philosophical rhetoric, but also through the regular connection between the defense of sociobiology and the attack against creationism, he was akin to Haeckel (minus racism, I repeat).

Incidentally, the fight against the religious enemies of evolution encouraged, during the last decade of the 20th Century, the introduction of a ridiculous alternative, but which was incredibly beneficial to reductionism, because it was endorsed with great complacency by the press and popularization: everyone was either a sociobiologist, or a creationist. Nevertheless, exceptions had here a comfortable majority. Let us move on.

The intervention of Dawkins, through the title itself of his first book, increased an underlying confusion, which was then maintained by various sociobiologists, between the duo egoism/altruism, allocated to some carrier genes and an intrinsic egoism of the gene, which would, in a way, be part of its very essence. On the one hand, genes diversely stimulate an organism behavior, which includes them, while, on the other hand, we broaden the vision up to the point of considering that gene reproduction prevails over the one of living beings. Both visions, unlike what Dawkins admitted *a priori*, do not reciprocally imply each other, even though they do not contradict each other either: the fact that the reinforcement of these speculations mobilizes

different mathematical devices is enough to confirm it. It followed that the tacit agreement of universities to them came from a coalition located upstream of the argumentations.

– **1981: Wilson, gene and culture**

In collaboration with a young physician colleague, Charles Lumsden, Wilson published an essay entitled *Genes, Mind, and Culture: The Coevolutionary Process* [LUM 81], which introduced the notion of *culturgen*, competing with Dawkins' *memes*.⁸ Although the interest of the general public waned, the idea of coevolution immediately spread among the supporters of biological determinism: the vagueness that filled it made it in fact very ... adaptable, in the sense that very different things could be installed in it, according to the guessed strength of the link that made evolutions interrelated and parallel. Coevolution went from simple parallelism to interdependence, according to the speaker's mood.

Like Dawkins, Lorenz or even Haeckel on that point, Wilson enjoyed personally taking charge of the popularization of his convictions. After *Sociobiology*, he published *On Human Nature* [WIL 78]. Lumsden and he offered, once again, an essay intended for the "cultivated public", *Promethean Fire* [LUM 83], complementing the austere "scientific" book. However, this raucous strategy progressively embarrassed ethologists' microcosm, especially since it was not consistent with the subtlety of the concepts: Hamilton, overall, was a more presentable founder.

1.4. Expansion, disintegration signs and recent defections

Then, the construction hardly moved for 15–20 years: sociobiology stuck to bold promises, colonized investigation fields with sophisticated intuitions, got implanted in Anglo-Saxon anthropology, by also promising there the Earth for tomorrow without fail, but the phase of the "findings" seemed to have run out. At most, it tried to specify here and there some assertions. The controversy quickly took a spectacular turn – both multidisciplinary and international – which was used to mask, and even excuse, the sudden immobility of ideas.

The battle raged for 12 years, before being replaced by a kind of "cold war", which would itself turn into a mutual ignorance of the protagonists: we

⁸ The book developed an article from these two authors, published in 1980.

explained in the introduction that the seriousness of this neglect represents the first motivation of this essay. Before analyzing the weaknesses of the debate, let us briefly outline the practical movements and the evolution of the balance of power.

In *ethology*, which is the main issuing discipline, the windfall was impressive and the only significant resistance occurred in France, where several laboratories had been opposed for a long time to the innatism imbuing Konrad Lorenz's school. A substantial and not surprising exception was the great majority of researchers attached to social insects who demonstrated their immediate approbation. Concerning others' disapproval, it unraveled at the beginning of the 1990s, before the symptomatic fear of being "cut off from the international community"⁹: to make amends, numerous articles quoted in their introduction "Hamilton's law", even when the topic was not consistent at all with the reference. The journal *Behavioral Ecology and Sociobiology* was born by the end of 1976 and was immediately assured a desirable strategic place.

In *ecology*, silence, in both Europe and the United States, was complete. The community seemed to have unanimously considered that this matter did not concern them. This indifference is partly explained by the fact that theoretical ecology was then shaken up by another fever: political ecologism progressively eroded its responsibilities and did not want to commit itself for or against this movement, up to the point of refraining from uttering the technical criticisms that it should have assumed. Thus, in 1986, when Wilson (who was always reluctant to differentiate between biogeography and ecology) ensured the promotion of a new notion, *biodiversity*¹⁰, and thus became again "likeable" to the eyes of the mainstream press, ecology kept its head down: it resigned itself to use the term in front of a large public, despite the damage caused to the essential concept of *ecological niche*, which then took refuge in the communication register between specialists [GUI 14]. Paradoxically, ecology, whose duty was to produce syntheses in biology, turned out to be the most cautious of all life sciences, and was incapable of giving any reply to the striking appetites of ethology, even when the latter claimed to invade its field. Let us leave aside for the moment a potential counterexample, which was already threatened: the recent and blurred *Niche Construction Theory* that we will discuss in the last chapter.

9 Expression heard several times by Georges Guille-Escuret when he asked his colleagues about the causes for the shift.

10 If he is not the author of the word himself, he determined the problem surrounding it.

In *genetics* and *molecular biology*, a small handful of masters diligently reacted: among them was Richard Lewontin, another figurehead of Harvard, except that, in most cases, his replies were in the areas of epistemology, ethics, philosophy and politics: therefore, outside the field of expertise of genetics. First, Haldane, Maynard Smith and Hamilton appeared as modelers and mathematicians: the experimental sector –“technoscience” in short – which explores the reality of genes, did not really feel the need to directly expound on sociobiology. Second, increasing collusion did however proliferate: implicitly.

We can easily see that geneticists’ voice was restrained, without excessively imputing motives: replying head-on to Haldane’s conjecture would have forced them to publicly assess the extent of their expertise, i.e. to nearly concede that their discipline was unable to get involved in such speculations. At the time of the trigger of the controversy, they had certainly assimilated the existence of *polygenes* (several genes for a single trait) and *pleiotropism* (a single gene for several traits), but they very approximately measured the extent of these phenomena. By way of illustration, the study of their “fetish” animal, the drosophila, concluded that one-third of the genes acted on eye configuration, and it was difficult to imagine that the other genes were alone responsible for all the “rest”. Molecular biology increased its authority because of the power conferred upon it by the increasing detail of its experiments, on the one hand, but also by carefully hiding the reciprocation between the exercise of modeling and practical results: a protective esotericism resulted from it, which was maintained all the way to its relationship with popularization.

From this point of view, the reply to sociobiologists fitted completely in the last lines of the book that Lewontin had published before Wilson’s “new synthesis”, with the draw back that continuing with these words no longer only concerned genetics, but a multidisciplinary instance that would have challenged the sovereign position. Foreseeing the success of the theory of the “gene as a unit of selection”, the professor entitled the last chapter “The Genome as the Unit of Selection” and ended it as follows:

“The fitness at a single locus ripped from its interactive context is about as relevant to real problems of evolutionary genetics as the study of the psychology of individuals isolated from their social context is to an understanding of man’s sociopolitical evolution. In both cases, context and interaction are not simply second-order effects to be superimposed on a

primary monadic analysis. Context and interaction are of the essence” [LEW 74, p. 318].

On this basis, human ecology would have had no reason to fear the intervention of genetics. Unfortunately, another path prevailed: by declaring the gene as a unit of selection and by *a priori* refusing to maintain the potential competition of the genome, sociobiology set social sciences and ecology in a passive waiting position in relation to the teaching provided by sociobiology.

In *social anthropology*, splits were more clearly asserted, by taking, at times, the form of divisions. To the astonishment of the Europeans (at least the continental ones), names known and deemed as “representative” from North America succumbed to the sirens of sociobiology, and surprising divisions were created between universities. In France, however, there was a complete refusal with some ambiguous changes on the fringes: some advocated a cautious “biosociology”, which wanted to distance itself from the Anglo-American abuses without desisting from exploring potential biological to social continuities. However, beyond national traditions, the increasing influence of “postmodernism” and several other relativisms through the entire western world of university confused social sciences and generated a kind of interference. The possibility itself of the controversy seemed obsolete, governed by the arrogance of scientificity, which was now incongruous with unexpected consequences: in the United States, an anthropologist could not ignore the triumph of sociobiological inspiration in all the laboratories studying “behavior”, while most of their French counterparts had no idea that it was the same for them. This is why we talked about a phase of mutual ignorance. In the end, a central contradiction was exposed: anthropology answered sociobiology through its greatest figures and thus admitted the existence of a real danger, which it pretends not to see anymore today, as if the problem was resolved. Except that, the threat quietly enjoys a prominent position in the buildings next door. How can we dissociate avoidance from evasion?

So, sociobiology extended its influence and the resistance against it decreased: the wealth of accusations authorized it to act as if it was misunderstood and cover itself with an offended dignity in the face of any questioning, even if it was purely methodological. The entire ethology turned any criticism into a condemnation and thus refrained from having to answer, by attributing the impossibility of the debate to a global malevolence from “other sciences”, which it brought together into an infernal magma. This quick fix still maintains its efficiency in ethology nowadays, apart from the

field of anthropology where the sterility of altruism's, subtleties caused an increasing morosity. In the United States, the journal *Current Anthropology* succeeded in more or less maintaining, in its issues, a coexistence between reductionists and supporters of the cultural autonomy (even though the latter bitterly felt the massive space consumed by a shameless biologism). Judging by this extremely rare "barometer", the return of the attraction exerted by group selection at odds with the Selfish Gene took place around 1995, which was a divergence creeping between the intuitions of primatologists and those of ethnologists: we concede the subjective part of this date, which relies on the warm welcome received by a daring modeling, which was however congratulated for its rigorous caution [SOL 95]¹¹.

On the other hand, *the repudiation of "Hamilton's law"* by Wilson gave an objective signal, which caused a sensation among specialists. The first blow was struck in 2005 in an issue of *Social Research* on the theme of big scientific errors: the myrmecologist cast doubt on kin selection, because of some "paradoxes" that were hard to overcome. Immediately, three European authors answered through an article with a clear title: "*Kin selection is the key to altruism*" [FOS 06]. The second denial occurred in the famous journal *Nature* in 2010: accompanied by two mathematicians, Martin A. Nowak and Corina E. Tarnita, he admitted that the trick, which was once applauded, did not confirm the assumed "heuristic" qualities, and that it was better to come back to a "standard" conception [NOW 10]. Two years later, he added insult to injury in *The Social Conquest of Earth* [WIL 12], which did not deter him in any way from continuing to impart a nature philosophy, as he obviously no longer needed a model as a mentor, except for a rather vague pleading to come back to group selection.

This time, Dawkins was angered and replied with a strongly worded article in an electronic magazine [DAW 12], and he collected in a hurry nearly 140 signatures of biologists in favor of the humiliated theory: it was a fantastic petition, which submitted truth to a "democratic" vote! In addition, it *a posteriori* officialized the attention of sociobiologists to power relations in the practice of science! Annoyed, Wilson looked down on things and sighed in his answer that his accuser wallowed in an archaic version of global fitness. Yet, his naivety on that point compared favorably with the one of his critic: did they not talk enough about "Hamilton's law" as a principle? A key

11 See final chapter.

core of this kind did not progress by itself, it did not correct or improve itself: one either validates it or not.

Beside this slip attributable to an outburst of susceptibility, Wilson did not however abandon ship on a whim. The zoologist declared that he was struck by doubt caused by some counterexamples, which we will review in Chapter 1, but threats also appeared from other horizons: although “Hamilton’s law” did not progress, experimental genetics, for its part, had made great strides since 1975. It can even now be caught heavily putting into perspective the relevance and reliability of the “elementary” concept of gene, at the risk of ruining the chances of this subject to reign over nature as a fundamental unit of selection.

However, for the moment, our attention must remain on another issue: under these conditions, should we expect, if not a complete collapse of sociobiology and what results from it, at least a deep crisis in contemporary ethology, with bitter discussions and feverish debates where – why not? – past adversaries, who were demonized and vilified as subservient agents of intolerant thought, would rise again. However, no jolt troubled the ambient tranquility: the reign, intellectual comfort and prerogatives of sociobiology (or behavioral ecology) hardly suffered from this trivial incident. Carefree spirit, in truth, but after all, Hamilton is dead, which spares the fear of a change of heart from his part. In *theory*, the “new synthesis” is dying, but sociobiology is *institutionally* doing great: recklessness, carelessness or cynicism? We will come back to that point.

1.5. Missing or extra squares on the chessboard

In hindsight, the expansion of this theory in the scientific world, despite its numerous constitutive incongruities, leaves us flabbergasted. If a conspiracy had directed the operations, it would be rightly qualified as “Machiavellian”. Among others, the philosopher Pierre Thuillier pretended to ask the question in these terms in the title of an essay – *Les biologistes vont-ils prendre le pouvoir? (Will biologists seize power?)* [THU 81] – thus revealing the background of a pervading concern: the one of an organized, conniving and, maybe, secretive intention. Feeling “innocent”, the said biologists turned back the accusation in the direction of their adversaries, who were suspected to knowingly advocate ignorance. In fact, an ideology is even more effective when its supporters do not see it as such, because they shelter the source of their convictions in a strong “common sense”: this is what a devious conspiracy could not manage to premeditate without

stumbling in many places; mental automatism can induce it with complete peace of mind.

1.5.1. Wilson's initial epistemological traffic: sociobiology and behavioral ecology

Nevertheless, there was a successful operation, and we find it difficult to imagine that it would have innocently obeyed this kind of reflex. Wilson started his critical book by painting an epistemological picture of his environment, in which he detailed the prominent interdisciplinary relationships. Yet, this invaluable lesson, issued in peace and quiet, hid a devious trick when it was installed in a diachronic perspective. The zoologist presented, in fact, a “subjective design” of the condensation of ideas springing around behavioral biology through a succession of equivalent schemes assigned to three dates: 1950, 1975 and a projection for 2000.

On the left side of the image, we witnessed the continuous growth of a space named “integrative neurophysiology”, fed in the background by cellular biology. In the center, both units of ethology and comparative psychology, which were distinct in 1950, were then combined in 1975, even though lessened, and in 2000 only built a relative narrow bridge linking the sets, which swelled on both sides. Finally, on the right-hand side, a small spot containing sociobiology and behavioral ecology hardly grew between 1950 and 1975, but it was significantly similar to a biology of underlying populations, which fed it with information. In the year 2000, sociobiology and behavioral ecology flourished into a unit as massive as neurophysiology and, behind, the area of population dynamics had also tripled.

Wilson declared: unlike popular belief, which would make of it unifying scientific fields, ethology and comparative psychology were “doomed to be cannibalized by neurophysiology and sensorial physiology, on the one hand, and by sociobiology and behavioral ecology, on the other hand” [WIL 75, p. 6]. In a falsely sheepish tone, he added that he hoped that his colleagues would not be too numerous to feel offended by his prophecy. No worry: the absorption turned out to be clearly more enticing for the prey than digestible for the predator!

A persistent vagueness enveloped this intuitive understanding of the avatars experienced by disciplines outside the mind of the researchers who animate them. Integrative neurophysiology and cellular biology were not well-established enough to cause defensive reactions: no complaint was to be

expected on this side, even if nothing indicates that specialists would support the perception. In the midst of images, things became more blurred, as Konrad Lorenz defined ethology as a comparative psychology. In addition, some of his students resolved to create a human ethology. Then, what did the division into two disjoined research areas around 1950, followed by their fusion observed in 1975, mean?

These hesitations would seem insignificant if they did not help to blur a less naive maneuver on the other side. “Sociobiology” was not indeed a new notion in 1975. In France, for example, the term was part of the title of a master’s degree that ignored Haldane or Hamilton, and was issued by the Sorbonne in a curriculum on social sciences: the word flatly referred to the ethology branch centered on animal societies. On the other hand, it was not the same for *behavioral ecology* that we would track down in vain in the indexes of ecology books published before this date. Even in ethology, we did not find any mention of it.

Why was *eco-ethology* not mentioned when it was used in a small number of rather important publications? Because eco-ethology admitted that it was defined as an ethology taking into account environmental variables. By inventing from scratch – as all evidence suggests – a behavioral ecology to which he offered *ex nihilo* a very comfortable position, Wilson shifted the center of gravity: it would concern an ecology scrutinizing behaviors instead of an ethology identifying the influence of environment.

Sociobiology thus made up a siamese twin, which was draped in an eminent legitimacy in ecology, and so in the breeding ground of Darwinian syntheses. This is why we insisted on the launch of the journal *Behavioral Ecology and Sociobiology*, a year after Wilson’s book: reductive ethology immediately understood how to take advantage of this imaginary *alter ego* and quickly endorsed it. The embryo appeared after his birth certificate, which certified the activity of the said discipline in 1950. The fascination with the principle of genetic theory obliterated the subterfuge, and no one was worried about the consequences. Wilson simply performed a scientific takeover bid that ethology deemed “friendly”. As for ecologists, they did not have any worries about it: today, ecology manuals do not mention behavioral ecology any more than the manuals of the said “discipline” are interested in

biocenoses and ecological niches: here too, it is enough to look at thematic indexes to verify it¹².

1.5.2. Communicating evasiveness: behavior, altruism, society, culture

Usually, or “normally”, a scientific theory analyzes, by means of an explicit method, a corpus of observations established under carefully identified (experimental or non-experimental) circumstances, which leads it to correct the concepts in place, and even, if necessary, to create new ones. We recognize that mishaps sometimes complicate the process and modify this ideal scenario. In the case in question, we are not simply dealing with a discrepancy: sociobiology is built around an elementary key, “Hamilton’s law”, which it applies to the entire animal kingdom by pasting it on inconsistent concepts, which are literally “one-size-fits-all”. It shamelessly adapts them (with no explanation) to its desire. Here, *theory is involved in method*; it gets comfortable and protects the illusion of the “future” rise of an elucidation.

More concretely, the core of the *psychology of gene interest* designed by Haldane–Hamilton generated the “new synthesis” advocated by Wilson using *evasive and inconsistent concepts as drive belts* going from one end to the other of the machine. Among the most dreadful concepts in this regard, we will remember “behavior”, “altruism”, “society” and “culture” and, more recently, “gene” since molecular biology now challenges its constant reliability as a life atom: we will come back to it at the end of this chapter regarding future perspectives. For the moment, let us just put the problem raised by the other four notions in context. They all have a common source: their sense always varies between the echoes of “common sense” that adopted them, on the one hand, and a processing declared scientific, which would give them more rigor, on the other. Yet, prescientific definitions remain the unique landmarks available in multidisciplinary discussions, because the added “rigor” systematically comes from specific disciplines, which intend to immediately take over the concept and deprive the other sciences of it.

12 This might be changing: a famous ecologist recently admitted, out of the blue, a connivance with behavioral ecology. It is however too early to say whether the reclaiming is anecdotal.

1.5.2.1. Behavior

Let us consider “behavior”, or its counterpart *ethos*. The idea revolves around a reaction when confronted with a certain situation. The recognition of the answer and of the situation progress, by mutually becoming clearer until all the parameters capable of stimulating or inhibiting the questioned activity, are identified and analyzed. If we stopped at this point, would everything work out for the best? Yes, in the eyes of the specialists. In reality, the worm is already in the fruit, but let us retain for a moment the internal view of the ethologist, who is satisfied with a base he believes to be neutral. According to him, theoretical diversity with the contradictions it generates, later emerges when making comparison, which results in the recognition of dissimilarities, the elaboration of categories and the weighting of variables. There are innate or acquired, individual or collective, sexual or feeding, etc. behaviors: with arguable boundaries, heterogeneities and disturbing overlap between classes.

Feverish or non-feverish dissent is part of the functioning of sciences, at least to prevent it from falling asleep: provided, however, that it is not organized so as to become unsolvable and to drag out clan rivalry. These immobilizations often occur when antagonist parties agree to support an assumption, which remains silent. For example, in the famous “quarrel about what is innate and what is acquired”, the protagonists protected themselves against an (institutional and non-intellectual) risk of agreeing, or shifting the debate, by questioning the legitimacy of the behavioral concept.

Where did the deficiency creep in? While considering the “basic” definition, we used the words “action”, “reaction” and “answer”, which, at first sight, are not exposed to any objection. Nevertheless, beyond their numerous differences, ethologists and psychologists share the conviction that behaviors make up societies as irresistibly as small streams make up big rivers. The reaction of an insect to light, or the one of a fish to the color of another fish from the same gender and species, is behavior. The building of a nest by a weaver, or the making of a termite mound by thousands of workers, is also behavior. A dominant chimpanzee being groomed by a subordinate, or a courtship dance by such a bird, is also behavior. In addition, in the end, nobody gets offended by listening to someone discussing the behavior of a society.

Elementary actions, “chain” reactions and global responses: behavior goes from reflex to structure, passing through all the scales of *behavioral sequence*, a low-level concept that ethologists seek to keep discreet. Neither the length of sequences, nor their addition to an entity located at a higher

level, is accompanied by explicit limitations, and therefore, behavior comes to a track transporting the ethologist and the psychologist from gene to society, or from hormone to culture.

Action, reaction, answer: sometimes atoms, sometimes sets, sometimes set of sets and so on. Many disciplines are confronted by this difficulty and its presence does not intrinsically condemn a “behavioral science”. Ecosystems go from fruit to biosphere, societies from bunch to civilization, and what about technology operating systems, or the linkage of language signs?

The criticism to be brought up focuses on the potential lack of attention from a research field to dangers of recklessness regarding this point. The segmentation of sequences and correlation with levels of reality give rise to little curiosity at the methodologist level. On the other hand, let us think about the place that ecologists give to the processing of “scales”.

From the very beginning, ethology has extended the secular desire of psychologists to annex sociology by gifting it strong “foundations”. When Wilson declared to the press that the genes held culture on a leash, his metaphor operated on two levels, because it attractively highlighted the potentials of the “telescopic track”.

1.5.2.2. *Altruism*

Auguste Comte imagined altruism as the logical complement of egoism (which broke away from “self-esteem” less than a century ago), probably in order to highlight a persistent state of mind, or even a tendency. The “social Darwinism” of Spencer and Haeckel then used the term again without modifying its meaning: as long as Lamarckian inheritance underlied the matter, the initial definition did not create any trouble.

However, Neo-Darwinism, as we saw, prohibited its development merely because of the advantages drawn from it by the previous generation. Therefore, when Haldane revisited the issue head-on, he implanted – implicitly in 1932, and then formally in 1955 – a supplement to which no one objected: altruism was no longer exclusively defined as what it brought to the beneficiary, but also and inextricably as what it took from the issuer. *Altruism would always have a price*. In other words, if the individual X makes a gesture that benefits Y , but X is not affected in any way by it, we are no longer in the presence of an altruist action! For sociobiologists, the “zero risk” in this type of behavior corresponds to an illusion, a phantasmagoria, a *typical case of “fetishism”*: because

altruism has a price; we find it a currency. In reality, it is the genetic currency that claims the price stipulation.

What about mutual help? When Kropotkin challenged the omnipotence of competition within natural selection by asserting its main power [KRO 79], he described actions that enriched all the participants. A “reciprocal altruism”, in a way, where there would be nothing to fear, since a cheating partner would result in a social penalty far too disadvantageous. Biologism shrugged off this kind of counterexample, by assuming that a careful analysis – of the kind that traditional social sciences are unable to provide – would reveal a more subtle reality, which would be more accessible to smart speculations of the Selfish Gene.

The truth is that accepting discussion on that point would lead the altruism theory to collapse like a house of cards. All secondary school students know that one cannot divide a number by zero. In addition, an altruist behavior devoid of any danger would invalidate the calculations worked out by the *game theory*, which captivates the supporters of Hamilton: this monster would destroy the fine organization of the whole as soon as it is incorporated, by invalidating all the range of comparisons. Altruism necessarily incorporates deteriorating chances for the issuer; otherwise, biologists would have to worry about a possible precedence of “secured” altruisms in relation to genes: certainly a chore, but even worse, a resignation to see altruism escape the biological monopoly (and, eventually, the intolerable perspective of conversations between equals with social sciences).

Nothing prohibited sociobiology from promoting a theoretical tendency in the field of socioecological research. The negation that was not raised concerning “non-payable altruism” clearly assisted the irresistible desire to clearly wipe out the field of socioecological research (which would include a wide range of debates) and to only keep sociobiology (and “internal” discussions).

The embedding of competitions and solidarities in a community calls for a category of socioecological research dealing with an enormous range of targets. A satiated monkey giving food to another one does not really scare its genes. Furthermore, he will perform actions relating to a more interested or more daring altruism. Who can believe that the different ways to privilege another are independent, that evolution would only be interested in risky altruism, that it would despise “the rest” and that it would be legitimate to disregard the quest for interactions inside this heterogeneous register? Only sociobiology would, against the methodological caution of socioecology.

1.5.2.3. Society

First among the “elementary concepts of sociobiology” reviewed by Wilson, here is society:

“A group of individuals belonging to the same species and organized in a cooperative manner. The terms *society* and *social* need to be defined broadly in order to prevent the exclusion of many interesting phenomena” [WIL 75, p. 7].

Bravo: Let us certainly not diminish the phenomenon. Pity that the author made this worthy recommendation, just one sentence after cutting to the quick. Because, if society is composed of individuals from the same species, the biological link uniting them inevitably plays an eminent role in their association and cooperation. Once this cement is fixed with the blessing of common sense, the sociobiologist is not losing anything to plead for open-mindedness: now that he is certain to fall back on his feet whatever happens, he will easily direct what follows.

A science necessarily progresses by repelling “common sense” convictions, i.e. prejudices. Here, the strength of sociobiology consists of relying on an obvious fact: a society is a set of individuals who talk and interact with each other. So, individuals of the same species, and so, united by an eminent biological link. Our common sense is thus confronted with an alternative: either it maintains this position and will ultimately have to assimilate the image of the Selfish Gene, or it rebels against this vision and must question the constitutive elements of a society.

There, we discover the reason why animal societies hypnotized those who pretended to build sociology on a scientific base. If societies existed before humankind, they must contain the essence of the phenomenon: human societies then appear like animal societies extended by complications, but upholding the same original spark. They add something; they remove nothing. Even if they *transform* what is social (hypothesis which is generally evaded), they would necessarily depend on original laws. Yet, what we have always been differentiating as “animal societies” does not contain neither factory nor myth: only interacting individuals, who are more or less related, and more nor less similar.

The philosophical curiosity inspired by animal societies involves in fact two intuitions. First, the radical side of what is social is unchangeable: humankind completes it, extends it, but does not corrode it. Then, it is built

from the relationships between organisms of the same species. Symbiotes, parasites, predators and animals competing for food represent another kind of problem, even though the organization of the group widely changes according to their presence or absence. However, we must understand that *this definition results in the inclusion of the entire psychic and the exclusion of ecological relationships*. Society stands before the center like a fortress, and behavioral ecology will never be anything else than an ethology.

Let us figure out the challenge. The zoologist can consider that symbiotes living in the entrails of termites are not an integral part of their society, and common sense will spontaneously prove it right. Except that, without these protozoa, termites would not manage to digest cellulose, so they would not feed on wood, they would not build termite mounds, etc. Wilson knew it well and explained with this internal contribution the rise of the said societies, even though they do not give a foothold to Hamilton's correlations.

Logically, if protozoa are not part of termites' societies, then yams, taro and banana trees are not part of Bantu societies, which colonized the African forest area, either, despite the fact that entering the forest was made possible by their importation from Asia. Therefore, it is not appropriate to compare these groups with those living in savannah: "socially speaking", they will be the same! Likewise, a European society will be analyzed by leaving aside cows, pigs and broods, as well as stables, granaries, etc. What will matter, however, are the farmers and the agricultural workers whose genomes will be carefully studied.

Despite appearances, these words are not in any way a caricature, and merely methodically follow a rule, whose ambition is indeed to cover humankind: depending on whether or not society incorporates the criterion of "belonging to the same species", the content of the discipline will change completely.

Thus, Haldane was one of the first authors to mention a complex case of altruism: in the forest as well as in the savannah, sometimes several species exposed to attacks of the same predators not only decrypt the alarm signals emitted by individuals of their taxon, but also the warning given by other species. The whistleblower is characterized as altruist, in its shortened sense, as it takes the risk to be spotted by the approaching carnivore. However, the advantage goes widely beyond the group of its counterparts. Therefore, we are thus facing a non-social form of association, in the context of sociobiology: collaboration excludes social relationships by the sole virtue of

an axiomatic criterion, engraved in marble ahead of any observation. Wilson's definition offers society to ethology and removes it from ecology.

The first sociology thesis defended in France, in 1877, precisely concerned animal societies. The author, Espinas [ESP 78], who was an admirer of Comte¹³ and Spencer, passed for one of the masters in what we called then "biological sociology" and which dominated the university scene. However, the sworn adversaries of biological determinism, starting with Gabriel de Tarde and Émile Durkheim, refused to mistake this author for the supporters of "social Darwinism" and showed real respect toward him because he was resolved not to let himself be governed by easy analogies. Here is the beginning of the first chapter:

"The idea of society is one of a permanent competition between separated living beings for the same action. These beings can be brought by the conditions where their competition takes place to regroup in an area under a determined form, but it is in no way necessary for them to be juxtaposed in order to act together, ready to form a society. A usual reciprocity of services between more or less independent activities, this is the characteristic trait of social life, a trait that does not primarily modify contact or remoteness, the apparent disorder or the regular layout of parts in space"¹⁴ [ESP 78, pp. 157–158].

This introductory comment foresaw interactions, before the word, and protected itself against tempting partitions and easy evictions. Compared to Wilson's wording, we understand that Tarde and Durkheim, sworn enemies of biological sociology – and, moreover, rivals – did not compromise their respect toward this elder: the harsh and disconcerting debates they launched did not contain anything futile.

In the chapter dedicated to social insects, we will come back to the pros and cons of "monospecific" society.

13 A reference, which is still notorious and was the reason for the Jury's refusal to publish the long historic introduction where Comte was mentioned. It only appeared in the second edition in 1878.

14 Authors' translation.

1.5.2.4. *Culture*

You must taste euphemism to be able to qualify the concept of culture as evasive: in front of this bottomless well, brevity is a challenge. In 1953, two American anthropologists, who were not the least important, had analyzed a series of 300 overviews, including half which were complete definitions [KRO 53]. Half a century later, another essay brought another 300 more recent phrases [BAL 06], and it lacked numerous suggestions from biologists. A representative, if not exhaustive, corpus should gather at the very least a thousand proposals, and again, provided we do not leave university forums. Why this avalanche or abundance of information?

First, culture is one of these notions that duplicates from the outset: tails, an ethnic group, a society or an organized community shows us “cultures” as collectivities united by durable links, whereas on observation, we glimpse a complex phenomenon supposed to strengthen nature, or break away from it. A population lives in nature, but a culture dwells in... culture. The researcher who could securely define culture would manage to understand cultures, but the opposite induction, even though less straightforward, also works, because a method that would precisely identify cultures, would quickly find the grail: the synthetic vision.

Second, the idea of culture, as a phenomenon, is designed to incorporate what structurally distinguishes humankind. Contrary to a scientific instrument, culture therefore incarnates the ultimate treasure of anthropology pursued in a common epic by biologists, psychologists, sociologists and – may heaven have mercy! – philosophers.

The definition of culture is torn between the plan of a path to be taken and the magical appropriation of the challenge. A sword of Damocles is permanently swinging over anthropology: the sophism that changes horizon into “here” and the objective into “here it is”, with a definition that leads straight away to the author’s theory.

Sociobiologists excelled at this type of travesty, the first prize going to the inspiration of John Tyler Bonner, who so intimately linked culture and altruism that he found the embryo of culture among amoebae [BON 80], probably in a broth.

It is useless at that stage to look further: sociobiology falls there within an old competition to capture the content culture, a competition that goes widely beyond the framework of its establishment. At best, the snares of zoologists

in this respect found it harder to be imperceptible than those of psychologists. However, it happens that, reversely, a researcher feels obliged to define a culture that does not represent in his/her eyes a disturbing variable, and the result then reveals the authentic limits of expertise of biology at the outset of its argumentations. This is why we will not grow tired of quoting a pearl found in the glossary of a physical anthology paper which offers a fine internal scuttling of the abusive pretensions of biological determinism:

“Culture. Set of behaviors and information communicated through another path than DNA”¹⁵ [HIE 80, p. 394].

1.5.3. *Cut-short dispute and perverted controversy*

If the set-up of the theoretical device was done in the comfortable peace and quiet provided by a circle of specialists, Wilson’s book caused an immediate outcry, which acknowledged *ipso facto* the sharp perception of an unprecedented danger, with a background of ironic and disdainful frowns. *First mistake*: either one does not condescend to answer, or if one “resolves” against it, one does not pretend to despise the adversary against which one requires a mobilization. When an anthropologist as renowned as Marshall Sahlins hurries to write and publish a pamphlet in order to slay the new version of reductionism, and that, furthermore, he chooses the medium of a book rather than the one of an article in a magazine, it means that he rings the alarm bell: *The Use and Abuse of Biology* was published a year after Wilson’s “new synthesis” [SAH 77].

The charge was brilliant, but it included the *second mistake*, which would globally undermine the criticism of sociobiology: philosophic and political accusations strove to fill in the blatant gaps of the scientific contradiction. Sahlins hastily gathered some scattered data in biology, but he completely ignored the theoretical continuity that we reproduced in the first part of this chapter. The most invigorating part of the text is found in a flurry of ethnographic counterexamples where cultures greet with great pleasure the biological contributions of their neighbors and have an utter disregard for the desire of their genes to reinforce their representation with the next generation.

A flurry of spectacular counterexamples are not sufficient to refute a theory: they materialize and intensify doubt, each of them transforming it into a challenge that the reflection called upon will have to take, but they will only

15 Authors’ translation.

really invalidate it at the expense of a complete analysis of the influences attributable to all the variables of the context. Their accumulation reinforces the probability of a fault, nothing more. In other words, a counterexample indicates a promising impact point in order to methodically develop a denial. That said, Sahlins didn't have to make a great effort in order to transform his counterexamples into a strict denial.

The anthropologic knowledge of this ethnologist did not find it difficult therefore to report the double intemperance of sociobiology: breach of trust toward the audience and breach of confidence in itself. Nevertheless, once again, the demonstration is missing. On the one hand, he did not have all the tools necessary for its construction. On the other, reductionists are perfectly aware of this risk and their comments are always prepared to disable it: by asserting that genes only explained 10% of culture, a scientific ineptitude that refers to a more or less temporary arbitration of "common sense"; by also inventing conceptual dumpsites designed to discard emergencies into annex or accessory research to be undertaken when the main issue will be clarified. The prize for logical pirouette goes to the ecological determinism of Rappaport with his "maladaptation" [RAP 84].

The ethics of a scientific rhetoric cannot naturally be judged through the frequency of the truths delivered. Who would preside over the jury? The way it attacks a contrary rhetoric speech, or defends itself against it, gives us much more information. When Lumsden and Wilson published *Genes, Mind, and Culture* [LUM 81], they did not once mention Sahlins! However, they relied on Clifford Geertz to point out their disagreement, as it concerned texts that did not refer to them, but whose position was close to Sahlins'. In addition, they quoted Marvin Harris on eight pages, who was the pope of "cultural materialism" and a great enemy of Sahlins! For his part, Richard Dawkins granted him a note in the second edition of *Selfish Gene*, ridiculing an inappropriate technical objection that could have been avoided by consulting a zoologist colleague. We could easily continue: most sociobiologists avoided Sahlins, and the few exceptions mocked peripheral remarks by carefully bypassing the processing of counterexamples. This deceitfulness definitely does not agree with what we would expect from scientific honesty. Wilson's supporters bitterly complained about the imputing motives aimed at them, but they used it as a curtain to hide the observations they did not want to face: a dismal image on their behalf, which was also more widely distressing regarding the ability of the university world to manage its conflicts far from prevarication, which politicians plague us with during election period.

What of Sahlins himself? Is he beyond blame? Dawkins denounced his despising tone, which was a funny complaint made by an expert in this field. To tell you the truth, this admonition does not matter, either way. Denials of expertise whether they involve cumbersome ironies, condescending remarks, obvious disdains, even personal denigration, always prove to be double-edge swords: it is appropriate to use them with caution and to take responsibility for them, but not to banish them. Grievances against the anthropologist would then seem better based from his own side: his virulence and his taunts would not have been a problem if they remained in his discipline, but he wanted to attack his adversaries “in their own domain”. He is the only one who devalued the remarkable tiredness of sociobiology promised by his ethnographic adversaries, when he added to it a dilettante philosophy and malicious gossip, since he relied – in his turn! – on “simple common sense” in the face of specialists who were all too happy to correct his ignorance in order to forget the rest.

A few years later, in a more restrained style, another great figure fell for similar flaws: Lévi-Strauss [LEV 83], who summarized the history of sociobiology in relation to Hamilton’s contribution, before suffering from the hypnotism of a few shocking provocations from Wilson. The structuralist was bogged down in a discussion condemned to powerlessness in the absence of appropriate knowledge. Unable to identify the core of the reported problem, he expressed thoughts whose main perversion was to always go too far with his answers to questions, which were interesting nonetheless, but that he however left unclear.

However, the great majority of anthropologists who were reticent toward the Selfish Gene deemed the accumulation of both objections completely satisfying (i.e. sufficient). The opinion can only be understood in relation to a deplorable mitigation of the challenge: the defense of the disciplinary territory because of an unbridgeable gulf. By proclaiming that “Culture is biology plus the symbolic faculty” [SAH 77, p. 65], Sahlins fell back into the philosophical habit stipulating that humankind would be “an animal *which...*”. The factor mentioned just after gave the content of an irreversible fracture, to which the appearance of a mysterious suddenness was attached. Lévi-Strauss, himself, did not get bogged down in this rut and professed a progressive reversal of determinism, with the rise of rules of conduct established outside conscience. Rhetoric deviation, rather than rhetoric divergence, probably.

What should get our attention reveals the *third mistake*, which infected the controversy: while maintaining courtesy, the two masters botched

criticism by rushing toward a “call to order”, taking the form of a restoration of the correct path. Thus, they mistook their challenge – that which threatened their reflection – for the global challenge, i.e. all the issues destabilized by the arrival of the new theory. Yet, this priority granted to the protection of their positions generated a lasting immobilization of the debate: opposite sides established the antagonism and, in the end, they would draw from it a practical advantage. In fact, each of them recognized its official adversary in the other, which incidentally led to a relegation of the other approaches on the fringes.

Sociobiology triggered a multidisciplinary disorder. First, one should have reflected on the chance it seized, located the crack in which it fell, and identified the sources of unprecedented inefficiency. During this work, the central points of a multidisciplinary response would certainly have emerged with success. Instead of that, we got corporate, partitioned and potentially contradictory responses. Wilson’s theory reinforced these absurd claims to be established as a discipline, because disciplinary recriminations were addressed to him.

Sahlins and Lévi-Strauss did not seek the opinion of the researchers exploring the issues examined by sociobiology. They would have avoided inaccuracies, and even blunders, but they preferred to assume straight away the condemnation. Consequently, no one asked ecologists their opinion. In addition, primatologists, for reasons we will see later, could not drive back the entomologists’ group on their own initiative.

Did a deplorable narcissism pervert both anthropologists? The lesser evil would then reside in this explanation. However, let us not forget that this strategy spared them the uneasiness of a much broader questioning in relation to the establishment of an embarrassing dialogue between recalcitrant sectors. Is it really a coincidence if their interventions put protagonists back to the same place that the clans challenging the innate/acquired or *nature/nurture* relationships used to occupy formerly? *They hurried to “offer” an opposite counterproposition, whereas the appropriate answer was to request a multidisciplinary research program able to restore the possibility of solving the problems raised.*

The philosopher Jacques G. Ruelland exhumed a rather shrewd article, signed by Wilson in 1977, in which the latter imagined sociobiology as an “anti-discipline” of current social sciences, thereby supposing the emulation of a rather balanced rivalry in order to force each of the two groups to question themselves [RUE 04, p. 158]. Nevertheless, the elegant masquerade

mainly confirmed the selection of a “malevolent double” designed to drag the fight on forever.

The antidiscipline of “Wilson, Dawkins and Cie” included Sahlins and Lévi-Strauss, but ignored the existence of André Leroi-Gourhan, Edward Evans-Pritchard, Edmund Leach, Jack Goody, Pierre Bourdieu, etc.: the list of disqualified leaders, without reaching the level of social register, took rather significant proportions.

The biology of the speculator gene and the anthropology governed by symbolic faculty share one conviction in secret: the historical dimension of socioecological interactions would conveniently let itself be reduced to a set of contingencies that do not participate in the constitution of structures. The interdisciplinarity that would open this case would prove to be much more uncomfortable within a short time than a traditional rival with whom we developed some kind of complicity. Can science allow itself to indefinitely repeat the same games of chess by changing from time to time the appearance of the pawns?

1.6. Temporary theory and permanent fantasy: will sociobiology soon be superseded?

Like a rationalist messiah, Lamarck comes back! By writing the name of the biologist beside the word “epigenetics” on the biggest internet search engine, we revealed a number of references to “rehabilitation” and a “revenge” of the knight. The identification of numerous cases where the environment influenced the expression of genes now revived the cursed intuitions of the end of the 19th Century. Let us render therefore unto Caesar the things which are Caesar’s: the one who suspected first that cards had been reshuffled, via a reopening of old abandoned channels on human condition, was once again Wilson: first, in collaboration with Lumsden [LUM 81], and then in 1998, in *Consilience* [WIL 98], in which he developed a rather clumsy epistemological vision dictated and upgraded by his experience as a biologist. Culture then took the form of a collection of “*epigenetic rules*”. With a disturbing irony, if he had not commandeered the “meme theory” of Dawkins (these cultural replicators copying themselves the mode of action of genes at another level), he could have invoked for his benefit some remarks from the Lévi-Strauss text mentioned above.

The epigenetic rise went hand in hand with gene weakening as the ultimate landmark of heredity: both also became obvious around 2005. Seven

or 8 years later, the double process of a popularization and a sudden extension of ambitions started as a combination consistent with the beginning of sociobiology. Lecturers talked about the epigenetic reinforcement of behaviors, speculated about the role played by such phenomena in the emergence of the cultural phenomenon, pleaded for a revolution of the theory of evolution, etc. Verses were going to change, but not the chorus.

This time, however, ethologists would have to regularly consort with the practitioners of molecular biology: dominants who are a tad strict. On the other hand, ecology, for its part, still shone because of its absence, since a reduction into pieces facilitated and sped up, as usual, the processing of the environment. The movement certainly did not rely on a strict “tutor” or a unifying principle, similar to Haldane’s conjecture. However, why would it need it, when experimentation welcomed it with open arms? At the beginning of 2016, the prestigious weekly paper *Science* thus published a pioneering work on the permanent reprogramming of the characteristic behavior of a caste of ants belonging to the species *Camponotus floridanus*: young individuals were exposed to histone acetylation inhibition through injection in the brain [SIM 16]. Let us leave aside the delicate evaluation of the result obtained according to a technically very heavy experimental protocol, be it said without creating doubt, but especially note that, 3 days later, a website concerned with health issues published a comment entitled “Epigenetics to reprogram social behavior” indicating toward the end that the epigenetic process of histone acetylation also exists in humans [BER 16].

No one needs to consult a fortuneteller to see what follows, all similar, with, in addition, no dreadful handicap plaguing the development of criticisms: no counterexamples in sight before speculations, which play on an intuitive plausibility and decide to overlook variables. Lamarck will not gain anything from this questionable exhumation, but *social Lamarckism*, incorrectly called “social Darwinism”, will find again its former vibrant colors. Let us also be assured that sociobiologists will progressively join the ranks of Spencer and Haeckel’s reductionism, which now has technical capacities that they would never had dreamed of. Some have already started the reconversion.

Social sciences might pay dearly for their guilty dilettantism in the face of sociobiology: they must be prepared for an unprecedented effervescence of biological determinism. By only half committing to, or not committing properly to the discussion, they would lose the little credibility they have left. Locating their insufficiencies in the old controversy, however menial this review, is part of the adjustments to be urgently made. The ideological

condemnation of biologism is the most important, but provided that the scientific and methodological discussion is made obsolete by the series of assertions that reductionism prides itself on, as if they were numerous discoveries. As demonstrated by Patrick Tort, the cosmogony of liberalism came from the Spencerian “social Darwinism” [TOR 83, TOR 87, TOR 96] and this deserves particular attention, which must however be differentiated from the study of the biological theses supporting it, as well as their development and reviews over a century and a half.

Still more generally, can a science that does not acknowledge its doubts and hides its mistakes still be considered as such? Asking the question does not reduce the problem to a deontological process. Because the “end of ideologies” amounts to a pathetic joke and opposite intuitions about the organization of the world are still creeping in our theories, scientific research *has a duty* to constantly sweep from its door and to put in place regular procedures of disqualification against repeated mistakes and theories floating in ideal ether. Too bad for the reprimands afflicting an “epistemological police”: it remains the only force preventing us from sinking into the resigned apathy where contemporary philosophy praises an eminent wisdom by debasing, in the process, method [FEY 79], or “pointless discussion” [SER 92].