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Non-Negotiable Conditions for a Scientific Stereoscopy

Is it really necessary to trace the argument back to the disjunction between science and philosophy, or even between idealism and materialism? The solemn vow of utility made in the introductory pages rather exhorts a careful circumvention of these ancient disjunctions, decidedly "easier to grab, than to let go". Unfortunately, if the devil is in the detail, it is also at work in the "foundations", "bases" or principles. Not to mention in the "obvious". At a time when the militancy of indecision rules, evading obscurity at this level would be the equivalent of rushing towards fatal stalemates. However, even if science displays more solidity than what relativism acknowledges, interdisciplinarity, for its part, is characterized by a native instability which severely weakens its initiatives.

The expression "non-negotiable conditions" which opens this chapter sounds like a childish provocation intended to irritate relativists. Let us plead guilty as regards the mischievous spirit, but the case goes well beyond this epidermic character. By no means does non-negotiable imply insensitive to criticism. Bias is attached to the researcher's position when he undertakes a task: at this moment, ontological considerations regarding knowledge, or its object, should leave the scene and leave room for fixed rules, shared by the protagonists. Discussions regarding correct applications should leave Hamlet's terrible question aside.

On the opening pages of a now classic collection, *La science telle qu'elle se fait* [CAL 90], Michel Callon and Bruno Latour also started with a bugle call: "We can either discuss the legitimacy of a sociology of scientific

knowledge, or we can put it in practice". Let us inflect the claim: "we" can be immersed in both activities, and producers are expected to become involved in their assessment, *but above all, not at the same time!* This incompatibility exists in every science. However, it does not entail insurmountable difficulty for a biologist, whereas the suspicion that weighs on sociological knowledge constantly makes the amalgam a tempting one. Devastating dissymmetry accompanies the cooperation between naturalists and sociologists: while the first ones can easily dissociate assessment and action, the latter retreat to prerequisites by reflex as soon as their partners become cunning. And this under the risk of witnessing the mixture between science and meta-science inhibit any practical achievement.

Far from wanting to support a healthy philosophy against a bad one, or to argue for some kind of neutrality (everyone knows that this pretension would already imply an orientation), the aim here is to make the separation between philosophical discourse and scientific dissertation perceptible, the first one confronting the second one according to two opposite modes: on the one hand, that of external intuitions (sometimes interesting, or tonic) and, on the other hand, that of parasitic infiltration, which secretly undermines research. The challenge then is to conceive a breach other than in the form of a watertight partition: while communication between these areas cannot be proscribed, how can we reorient the constant flow of misunderstandings?

1.1. Operating principles against metaphysical principles

A second unacknowledged dissymmetry concerns the outstanding fact of their dialogue: in fact, an essential gap opposes the assimilation of a scientific conclusion by philosophers and, contrary to this, the reception of an argument proposed by metaphysics in the field of science. In short, this can directly recover the information produced by the "scholar" without any inconvenience, but should, all the same, always transpose the idea of the "wise man", by translating his terms to the concrete media of every research project. *A priori*, the difficulty seems quite incidental and technically easy to overcome. When Goethe spoke about nature, Bergson about life, or Sartre about history, their visions camped on a horizon: nobody would have to motivate them to have to prove these as reliable elements, since the reprimand would have quickly become offending. "Even" in the human sciences, a strict caution policy is generally enforced.

Unfortunately, the philosopher does not always display their identity, while at some other times, the scholar tends to weaken their own, by spontaneously fading its boundaries. And here, the situation is sometimes altered in fearsome proportions.

1.1.1. *Ventriloquist philosophy*

This critical complication elicits very few comments. Worse, when the relationship between science/philosophy tightens, this generally results in a ruling ethereal thought, and this even when the reflection emanates from a scientist, proud of being such: many sociologists – too many, maybe – have been trained by the followers of Plato and Aristotle, and there are countless biologists at the end of their careers who like to pontificate about the deep meaning of nature. Torrents of purely philosophical words regarding the access to truth and *episteme* emerge from all walks of life, whereas the caution that science should frontally keep against the authority of the specialists of the absolute is dissolved drop by drop. What follows is wishful compensation: next to the debates regarding the epistemological principles that govern the mainspring of science, it is important for science, under its exclusive responsibility, to restore or enact, the *sine qua non* conditions of its activity, starting by those related to its autonomy. It does not have the status of a protectorate, and complying with the requirements of a distant legitimacy attached to its project, would be equivalent to accepting the inhibitions derived from a never ending suzerainty, or even forgetting that for many centuries, science *has already existed* in terms of historical and social cohesion.

As a complement to the classical epistemology forged by those who look at things on the other side (a *meta*-), scientific researchers should grant an internal epistemology with greater consistency [GUI 97], questioning current *operating principles*, regardless of the *metaphysical principles* to be achieved. In the context of major resolved options, the progressive development of an interdisciplinary methodology hypothesizes the reinstallation of a set of discussions which regard the assemblage of multiple technical requirements, coexisting within a frame of ample cooperation: in fact, such deliberations seriously circumscribe the participation of the artisans of research.

Despite their abstract expression, these remarks quickly affect our argumentation because, in concealing the dissonance between external and

internal interventions, epistemology implicitly allows all kinds of clandestine smuggling between science and philosophy, especially when the questions involve multidisciplinarity, since the researcher of a district does not always distinguish the extrapolations permeated from another area. The destruction caused by these misunderstandings on the bibliography regarding the relationship societies/biocenosis has reached such proportions that the list would be endless. While it is convenient not to linger on the caricatures offered by scientism or reductionism[1], we can agree that the introductory presentations made by sociobiology [WIL 75, DAW 76], or by cultural materialism, with its "struggle for a science of culture" [HAR 79], have provided textbook cases in this respect. Students should be trained to identify the expedients which contribute to the corruption of a so-called uncompromising scientific ambition[2]. In a totally different context and with far more subtlety, Bruno Latour opened an essay which led him to be a recognized sociologist and, on its last page, he confessed to being satisfied with having accomplished his own "philosophical work" [LAT 91, p. 198]: the invisible trespassing of *a later denied threshold thanks to this study*, and which led to far-reaching consequences on the intellectual journey of its author. Except that the transition was not visible while it was taking place: practice silently preceded theory.

For a condensed view of the confiscation of this duality and its replacement by confusion, let us quote a double example in which juxtaposition leads to mystification: "negentropy" was welded into entropy [SCH 44][3], whereas "maladaptation" was added to adaptation [RAP 84]. In both cases, a rigorous and binding concept was suddenly decorated by a strangely liberating complement, invented *ex nihilo*. Apart from the adulterated legitimacy that the derivation sought, our attention should focus on an unnoticed aspect: not only were these "negative" embellishments scientifically unsound, but their advent tended to corrupt their source. A reflection which simultaneously refers to *negentropy* and *entropy* uproots the

1 The distinction between the two is often difficult: they share a habit for betting on the omnipotence of a chain of causality, in the short term.

2 Deterministic theories of cultural ecology and cultural materialism have suffered severe criticism, blow by blow, but few attacks detailing the argumentation mode itself. See [GUI 85, GUI 94, PAR 17] for sociobiology. As for cultural materialism, let us recall a forgotten article by Philippe Descola on "starving determinism" [DES 88].

3 Erwin Schrödinger was certainly a great physicist, but it was while he was making a dissertation on life that he assumed the position of a philosopher and that he sensed negentropy.

archetype, which is then metamorphosed into an irreversibly philosophical idea. And *adaptation* only retains its meaning by standing away from *maladaptation*, or else the two terms would form a tautological couple (the content of one would finally be inferred from the lack of the other).

That being said, processes regularly acquire the more capricious twist of a rebound. In the same way that André Breton defined pornography as the "eroticism of the others", philosophy willingly reemerged in the guise of an opposing science: biology was challenged by the anthropologist whereas anthropology was mocked by the biologist, and was thus transformed into a gateway for a clandestine philosophical discourse which the interpreter could place under the responsibility of their own discipline. For this purpose, it was enough to criticize an external abuse and to "infer" an appropriate inner position, which, however, would only owe a distant inspiration to the official scientificity defended by the accuser. In other words, while science X denounced the philosophical bias of science Y, it then inherited "corrected" wisdom, as a rebound.

While countless masters of ethology, genetics, or evolution theories have more or less candidly adopted this tactic, their counterparts in anthropology have not disdained it either: for example, when they stepped up against the impudence of sociobiology, Marshall Sahlins and Claude Lévi-Strauss [SAH 77, LEV 83], retrospectively consolidated broad convictions regarding the essence of the social through diverse extrapolations, by taking advantage of the presence of sensational opposition [PAR 17]. The counterproposal, supposedly the outcome of the rebuttal, blithely went beyond the zone of critical efficiency, and set out to compete against adversaries within the truly disproportionate and unjustifiable framework of "scientific" ambition.

Levi-Strauss thus offers us the best opportunity to complete this overview with a last textbook case: the one in which philosophy itself occupies the ungrateful role of the target, and innocently favors the distillation of a stealthy drift inside a scientific construction. The structuralist author published *La pensée sauvage* in 1962 and, in the last chapter, he forcefully attacked Jean-Paul Sartre, whose *Critique de la raison dialectique* had recently been published [SAR 60, LÉV 62]. For the time being, let us leave the content of the controversy aside, in order to clarify the ambiguity: the contradiction that the anthropologist contributed in the name of his own competence necessarily fell on the ground of the opponent. It should have been the same for the lesson learned from denigration, but this finally "enriched" the issuer's knowledge by

pretending never to have left him. Structuralism, as a "trend"[4], drew great advantage from this episode, by way of philosophical support advocating for a sharing between the study of infrastructure as mainly assigned to history, on the one hand ("assisted by demographics, technology, historical geography and ethnography"), and the theory of superstructure attributed to ethnology, which was quickly conceived as a "psychology", on the other hand [LÉV 62, p. 174]. Nevertheless, the operation was supposed to materialize among the sciences, despite the fact that these had not been invited to discuss its terms. Conscious or not, the coup was undeniably clever, as well as powerful: in the following pages, we will see that it undermined the exchanges between the supporters of ecology and those of sociology (in the broad sense).

Lévi-Strauss received a broad training in philosophy before going to Brazil. Among other things, his triumph on the academic scene radically changed the image of ethnology, which had so far oscillated between the nest of enlightened explorers and human naturalism. The new recruits, bottle-fed by Rousseau more than Darwin, multiplied the bridges with classical studies, which the French used to call "humanities". Without diminishing the gain obtained thanks to this contest, we should be aware of the profusion of ambiguities, as well as the confusing entanglement that followed, and which proved an obstacle to the sobriety of interdisciplinary dialogues. And now, the time has come for us to initiate our advertised series of "Methodological Assertions".

METHODOLOGICAL ASSERTION 1.1.– It should be necessary to differentiate, and then to exclude, the infiltration of a philosophical thesis at the heart of a scientific argument, because this contribution does not discuss science, except from the outside: proposals coming from different backgrounds should never compete on the same level. Philosophy has the right to generate assumptions, but it is up to the responsibility of the scientist to prevent a discreet thesis from being interpolated in reasoning. A mixture between philosophy and science inevitably falls under the aegis of philosophy.

METHODOLOGICAL ASSERTION 1.2.– The refutation of an error, or an insufficiency detected within another discipline, should not become the

4 Against the prevailing opinion of his contemporaries, Lévi-Strauss always refused to accept the creation, or the organization, of a *school of thought*. On issues related to Lévi-Strauss in the face of the scientificity, see Jean-Luc Jamard [JAM 93].

excuse for a philosophical counter-proposal, that is to say, the uncontrollable expansion of information disclosed by denial.

We have to resist the temptation to consider that the global representations of the world by philosophy and ideology[5] denounce the fake against a science which offers the truth: every form of relativism is built upon the trap supported by this simplifying perception. Ideology subdues everything, anticipates everything, or it succeeds in convincing itself: only the mathematics and the physics of the 20th Century were sometimes successful in introducing disconcerting discoveries which (temporarily) left everyone speechless. On the other hand, it seems impossible to be off guard when we refer to life, humankind, society or history. Meanwhile, science cannot yet understand everything, otherwise, what would it seek? From time to time, it can demolish external certainty in one sector or another, but it has no access to universal patterns.

In this sense, the researcher who explores and the scholar who teaches are two complementary conditions which incidentally differ in their dealings with philosophy. Research can therein capture a shred of inspiration which can be reformulated within a particular problem. The professor should point out the limits reached by his knowledge, but he becomes a philosopher as soon as he opposes an ideology in a competing position. At this moment, relativism preys on it as a vehicle for a belief which is "neither worse nor better than another".

The counterproposal effectively implies the entry into the field of ideology, but this refusal of passivity does not necessarily lead to a withering. This was revealed by the anecdote of Pastafarianism, created by Bobby Henserson in 2005, in revolt against the growing success of a claim by the proponents of "intelligent design"[6] and with the aim of obtaining equal teaching time to the one allocated to the theory of evolution at schools. At that moment, this student invented an allegiance to the *Flying Spaghetti Monster*, a wacky demiurge served by a burlesque mythology which has become quite fashionable, by the way. The originality of this fable, if compared to previous ones (such as Bertrand Russell's cosmic teapot, for

5 By ideology, we will only refer to a global interpretation of the world, without prejudging its social or historical substrate: social class, institution, etc. Philosophy is only one among many various possible sources.

6 A creationist theory which mimics scientific language, without respecting any method.

example) comes from Henderson's commitment to legally defend the faith in his flying spaghetti, so that his extraordinary creation of the universe enters schools, enjoying the same rights as "intelligent design". Does not every belief deserve to be respected? Suddenly, mirrored by a relativism which dilutes science into belief, scientific corrosion emerges, which uncompromisingly undermines the protests of religion.

This little tale is not a digression, given the permeability between mysticism and morality: think of their parasitic action over so many practical questions that human ecology has failed to treat without drowning under the interference of oracles barely more innocent than intelligent design, or even as farfetched as Pastafarianism. Are we not in need of a complement to post-ecologism which could bring its anathemas far beyond human civilizations, and which could blame life as disgusting planetary mildew? A *bio-phobia* deploring this blunder of atoms on the surface of physically unstable bodies, which could reduce the interference of nature lovers and restore a little scientific serenity. After all, the methane that cows yield contributes to global warming: guilty.

1.1.2. *Two materialisms and one idealism: the initial bet of science*

Nevertheless, since science was founded by a rationalist philosophy, is not the subset still dependent on the whole? In fact, considering logical continuity unveils a historical split, whereas following the temporal thread reveals a methodological rupture. Science proceeds from a materialistic challenge which is contrary to the bet proposed by Pascal, except that while the latter placed the eternal silence of infinite spaces on God, the scientist tackles the issue of matter within temporary and limited worlds.

This is the very first "operating principle", whose application detaches itself from metaphysics, precisely because it identifies itself with an *application*, which determines a program, a progressive task, a list of operations to be performed. Socrates knew nothing, Occam economized, Descartes doubted everything and, at the end of the race, Laplace revoked the intangible in assumptions. When considering being, philosophy is organized around the endless duel between idealism and materialism, but wishes to feed its discussions by acquiring data concerning the way in which things happen around man, and develops a sector destined to inform it about all kinds of issues: the data produced by science stimulate its theses, as well as its

antitheses. Nevertheless, these inquiries only animate the debate between opposing currents, by avoiding prematurely taking sides, and supporting all or a part of the conclusion in the middle of the introductory elements. It is therefore imperative to proceed exclusively from observable facts: in other words, accessible matter and the energy associated with it.

Apart from materialism, which in metaphysics is opposed to idealism, a "restricted materialism" emerges, which abstains from any dissertation on the ultimate essence of the universe: it only "bets" on its readability. Regardless of whether God exists or not, he does not visit the world as a *deus ex machina* repairing imperfections at a natural theatre, with an improvised rain of miracles. The scientific method, or "rationalism", is dependent on restricted materialism, which the secondary differences never internally question, since it designates *the* "non-negotiable condition", above all others. Historians have shown how this clause spread more quietly where monotheism consented to glorify the intelligible order of the physical world as divine message.

METHODOLOGICAL ASSERTION 1.3.– It is important to differentiate methodological materialism (belonging to the project of science) from philosophical materialism, which is in perpetual rivalry with idealism[7]. A lot of misunderstandings between philosophical and scientific discourse result from the sliding of criticism which targets philosophical materialism with amendments aimed at methodological materialism, while idealism rarely consents to discerning between them.

When an author claims to be "moving beyond" the antagonism between materialism and idealism (a common wish in the human sciences, nowadays), there is no risk in predicting that the output will significantly be oriented towards the second position, and that methodological materialism will accidentally wipe away the toughest blows. We have seen Lévi-Strauss stand up against Sartre to take anthropology away from history. Let us now instead take a look at Sahlins, once again advocating for the reconciliation of structure and history in the last chapter of a brilliant essay. The choice of the example is

7 Despite being considered from a different angle, a similar gap between philosophical materialism and materialism conceived as a condition and a tool for objective knowledge presides over the recent essay by epistemologist Patrick Tort, a leading authority on Darwinism and the theory of evolution: *Qu'est-ce que le matérialisme?* [TOR 16] This convergence on dissociation is in no way accidental: we have both been surprised to see some researchers rigorously apply a materialist approach in their professional practice, while "incidentally" remaining convinced believers.

less fortuitous than the topic explored here – the way in which the event is treated – considering that it will later occupy a crucial position between ecology and the social sciences. The reader will gain an advantage by keeping this assertion in mind:

> "Clearly, the twin anthropological (or historical) errors of materialism and idealism consist in attempts to link the meaningful significance and the worldly happening in some mechanical or physicalist relation of cause and effect. For materialism the significance is the direct effect of the objective properties of the happening. This ignores the relative value or meaning given to the happening by the society. For idealism the happening is simply an effect of its significance. This ignores the burden of "reality": the forces that have real effects, if always in the *terms* of some cultural scheme". [SAH 85, p. 154]; in italics in the text)

Sahlins irreproachably dismantled the anomaly by revoking a simplistic type of causality, with its equally deplorable contradictory versions. The relevance of the attack on the scientific level raises no doubt: moreover, it is a worn-out affair, since functionalism has always been reproached for attributing a more or less concrete utility to each social fact, and the structuralist tendency to neglect the action of material variables elicited early reprobation. Marxist anthropology undoubtedly disapproved of this double mistake, by attributing it to a psychologism incapable of admitting the autonomous coherence of the social order. Let us recapitulate: we have mentioned functionalism, structuralism, Marxism, and psychologism, to which we should add historicism and economism. So far, we have a terrain heavily loaded with various markers.

So, why directly evoke idealism and materialism? Undoubtedly in order to go faster, but towards what, other than the translation of a scientific difficulty into a metaphysical duel? It is literally a false evasion: then, the discussion escapes interdisciplinarity by stifling anthropology in the face of philosophy. To be convinced, let us read the words which immediately follow the passage reproduced above:

> "The same goes for theory and practice, taken as phenomenal alternatives: this objectified distinction between cultural concepts and practical activities that is itself untrue in practice

> and absurd as theory. All *praxis* is theoretical". [SAH 85, p. 154].

The geographer, the agronomist, or the technologist who could willingly address the relationship significance/event posed in the first quotation, might probably abstain if asked to accordingly consider the relationship theory/practice. This junction closes the door to a room where the wise man shares a few words with the *complicit* anthropologist. Although they may intuitively look like evidence (to the eyes of "properly" trained minds), such analogies politely reject scientific participation[8].

METHODOLOGICAL ASSERTION 1.4.– When an argument is committed to surpassing the antagonism of materialism and idealism between sciences, it actually challenges the preservation of materialism on the methodological level, even if this monopoly historically represents an intrinsic portion of science itself. The questioning tends to camouflage a philosophical nature and to overshadow its exteriority, compared to the problems actually assumed by scientific research.

Yet once more, that is the reason why the title of this chapter dares to use the expression "non-negotiable conditions", which could have been withdrawn, or at least nuanced, if mere provocative infatuation had inspired it. Everything depending on the materialistic bet of the method is not to be discussed within the context of science, because its very existence depends on it: pushing the bet is equivalent to leaving science, doing science is equivalent to respecting it. While this may look very simple, we should bear in mind that from the perspective of an interdisciplinary effort, every form of participation should not only comply with, but most importantly, should rely on the commitment of others.

1.1.3. *Ontology: a catch-all concept and a bottomless pit*

Emigration towards the "meta", the "trans", or the "post" will not automatically invite subterfuges, be they conscious or not. Some people approach philosophy solemnly, in the way that others approach religion:

8 Referring to the case study offered by a brilliant anthropologist, elsewhere we have analyzed other "moments" ([GUI 10, p. 175 *sq.*]; [GUI 13, p. 343 *sq.*]). Notably, around a formula (here, a symptomatic one) according to which "the more it is the same thing, the more it changes" [SAH 89, p. 149]): structure can certainly digest novelty, but it can also provide an adaptive skill to its own inertial force.

with a mixture of spiritual vocation and disaffection regarding the temporal factor, or resentment, precipitated by coldness in front of the incompressible disorders of history. In most cases – for naturalists and physicists, mostly – this shift formally appears as a trip towards another type of cogitation. Mathematicians and anthropologists, however, therein feel an expansion, or an extension, rather than a shift in territory. Nevertheless, the transfer does not upset the migrant's original beliefs, as regards the construction of the universe: the deterministic is still deterministic, the possibilist remains so, etc. Convictions may inflect, but do not finally change. *Except in the social sciences*, where the penetration of philosophy is often in harmony with a repudiation of materialism in favor of idealism: too frequently for us to believe that it could be an accidental series. Is not the process akin to the one that, during the second half of the 20th Century, transformed the disavowal of Stalinism by seasoned militants into a kind of mysticism? The rapprochement is particularly imperative in France, where prestigious Marxist anthropologists jointly went through both alterations.

The austerity of the scientific method seems to exhaust the minds aspiring to acts of bravery: they chomp at the bit, and then lose patience. The anthropologists who admired Clifford Geertz for his *Agricultural Involution* [GEE 63] were a bit confused, later, when he published *The Interpretation of Cultures* and *Local Knowledge* [GEE 73, GEE 83]. Those who read the detailed ethnography of Philippe Descola in *La nature domestique* [DES 86] found a considerable hiatus when they discovered *Beyond Nature and Culture* [DES 13]. The same applies to Tim Ingold, between *The Appropriation of Nature* [ING 86] and *The Perception of the Environment* [ING 86]. The contrast between old and new titles is eloquent. In 1996, Ingold and Descola took part in a major conference on "the topic of Maurice Godelier", and their contributions then gave the impression of wanting to get to the bottom of the question, rather than to look for an exploratory path [DES 99, ING 99]: while it is true that a discussion centered on a work often leads to a confrontation of syntheses, does that imply that the erasure of methodological curiosity by metaphysical satiety will necessarily follow?

In this register, perplexity nonetheless peaked with a debate organized directly between Ingold and Descola on the question of ontologies and the relationships between societies/nature[9]: *être au monde: quelle expérience*

9 What emerges from the dialogue does not justify us stating "nature" in the plural, and this could be the start of criticism itself.

commune? (being in the world: what is the common experience to that?) [DES 14]. A whole program, certainly, but for whom? We will not give details of the impressions exchanged by these two authors, however sparkly and seductive[10]: the review would take too much space and would take us far but, in the end, it would not inform us about the restorative chances of an interdisciplinary project. In addition, the danger of pecking a "shock phrase" here or there would acquire absurd proportions: can we scorn Ingold at the precise moment when he concluded his participation asserting that the role of anthropology is to "turn human life itself into a conversation" ([DES 14], p. 75)? Yes, only if we adopt the position of a shallow scientist brandishing the suspect flag of ignorance. Or from an opposite perspective, on a "meta" level. Ingold trumpeted a playful opinion, but bitterness oozed no content. It arose from the circumstances: namely, a philosophy pouncing on an ossified science, using it as a springboard.

But to avoid slogans? Beyond any kind of stylistic provocation, Descola retold how, regarding the question of the Achuar people, he perceived "that it was impossible to apprehend this community as adapted to its environment", among other reasons because this "society perceived "natural" beings as partners, rather than as resources" [DES 14, p. 29]. The logical mystery of the relationship between the observation of thought partnership and the absence of practical adaptation also paves the way to the irony of the edgy positivist. And the vision inadvertently opens a side door which leads to the void: men became "adaptable" after the abandonment of animism. For the time being, this point requires some caution: was Descola disqualifying adaptation or adequacy? Equilibrium conquered by evolution, or the one modeled by history? The struggle for existence or coordination? Under the guise of a dubiously ambiguous notion, was he not dismissing several embarrassing variables? The anthropologist's hearing suspects what he means by "adaptation", and he spontaneously goes through a host of suffering theoretical difficulties. A bit like those modern art paintings, in which the viewer is invited to teleport his own signification.

The ecology Descola now refers to has nothing to do with scientific ecology, and although a biologist from the outset, neither does Ingold associate himself with a cooperative scientific project practically involving the human sciences. There remains an optional but attractive philosophy,

10 It is not merely a polite form: the *philosophical* quality of these thoughts is not intended to be ironic.

alongside Sartre, Husserl, Ricœur, Foucault and so many others, without discounting the importance of field experience, which the sponsored philosophers themselves did not have. Simply put, the translation of these suggestions into the concrete questioning of the researcher will depend on the mood of the latter, and we have every reason to believe that it could be counterproductive to engage it at the beginning of an interdisciplinary enterprise.

Should we then banish "ontology" from scientific vocabulary? Interpreted in its classical definition as a cold questioning about being, or in the wake of Heidegger, its emergence actually seems to imply the rejection of a boundary. However, in epistemology, the word gradually acquired concentrated meaning, by targeting the *degree of reality* of a phenomenon highlighted by a science, with sociology in mind, in which uncertainty continuously hovers over the status of every social phenomena [LIV 00].

An illustration will briefly take us away from our favorite themes, with the compensation of suggestive clarity. In 1973, Pierre Bourdieu published a scathing article, "Public Opinion Does not Exist": in it, he denounced a "pure and simple artifact" with a concealing function [BOU 84, p. 224]. Despite the fact that the argument, dense and powerful, has lost none of its brilliance, the following decades altered its effectiveness due to an ontological point: why deny that an artifact may have an existence? Until further notice, Bourdieu was not claiming the "natural" character of real social phenomena. It was against the overvaluation of reality that the sociologist was aiming his attack. However, he wrote this text on an old typewriter, before the time of computers, before the Internet, before Facebook, Twitter, etc. While polls certainly retain the concealing force reported in 1973, they now serve a wider range of social derogations, and public opinion now represents the reptilian brain of a society: we do not talk to it, we excite it. To state that it has no authentic existence is no longer appropriate at a time when its emotions act (or, at least, react) in a formidable and unforeseen way: its avatars impose it as a complex and intriguing research object. An evidence of this is the strange category inserted within media, among journalists (information) and experts (technical knowledge): "columnists", these socially typical authors who are responsible for providing entangled models combining reason and feelings and spurring public opinion, not an audience.

Since theoretical incriminations cannot escape fashion trends, the offense of "reification" (or "objectification", an abstraction treated as a concrete object) has flourished: suspicion confirms condemnation. Occasionally, this type of trial affects the design of science itself. However can we really darken the fantasy by looking at it as an object? As a form of knowledge, the debate will drag on as long as philosophers want it to. On the other hand, a sociological point of view will promptly eliminate the complainants: its consistency is recognized in the history of some nations which endow it with some institutions, with communities of actors, or officiating priests. To deny the "being" of science despite its reality would be like counterbalancing the "materialistic bet" with an opposite bet: those who rank science among beliefs rarely concede that it has built a solid and stately church. And that it continues in a constant effort to *reify itself*.

In contrast, Bourdieu's reflection reveals the evanescence of anthropological ontologies, even while a restricted ontology, attached to identifying the real marked by the "fundamental notion", might not go unemployed: ethnicity, society, the system and the structure float in the middle of the "undecidable", something we will need look at more closely in the coming chapters. Let us not neglect ecology itself, since the existence of a science in a society – and this in particular – justifies our attention: a discipline methodologically deducted from Darwinism, seven years after the burst of the theory, in view of producing summaries on the relations between organizations and their environments, we now find it torn between philosophy and morality, wading in a metaphysical swamp which severely slows its progression [GUI 14]. Descola published a booklet entitled *L'écologie des autres* [DES 11]. If this otherness refers to peoples whose culture permeates many finds of Western civilization (science, market economy, industrial technology, etc.), that means that the others do not have ecology: they have cosmogonies. They think the world far from Darwinism, which invented ecology for its use, and this does not prevent them from interpreting their environment. Political movements which have recovered and parasitized a very poorly controlled name, were not, however, bound by the definitions developed by colleagues for referring to notions which could ease the sharing of ideas. Paraphrasing Bourdieu, Descola's ecology is "pure and simple artifact", with, in its way, a concealing function at the expense of scientific activity and in favor of philosophy. Because "the others" earn nothing to see their cosmogonies transfigured into ecologies, while "we" inadvertently disable (just for fun), a reflection instrument whose efficiency will be sorely missed.

Our apparent resistance to this professor at the Collège de France has no other motive than the eminent influence exerted by him on his disciplinary community. Descola is placed in excellent company here, and our criticism of implicit analogies will have more reach against him by calling to the stand an archetype like Roy Wagner [WAG 75], or an acrobat like Eduardo Viveiros de Castro [VIV 09]. Let us add that the diversion of ecology discovered two great ancestors in 1972, that is to say, just before the rise of environmentalism outside from university: Gregory Bateson, with his *Steps to an Ecology of Mind* [BAT 72] and Lévi-Strauss in a lecture given in the United States, *Structuralism and Ecology* [LÉV 83]. An analogy which strives to make one forget their own foundations, or their sources, so as to substitute a virtual elastic to the initial reality, quite a disturbing job.

In light of our stated goals, should we dwell on these topics? The answer is yes: the dissolving of scientific concepts and their philosophical dilution generalize a widespread demobilization and a lessening of the responsibility of scientists who, bounded or not, are obliged to relearn how to collectively protect their activity against infiltrations which could practically paralyze their activity. A lack of appetite in front of this form of understanding comprises both a political and professional dimension. And, with the silent but active complicity of dominant philosophy, public opinion fosters a "cover-up" of concrete priorities.

METHODOLOGICAL ASSERTION 1.5.– The ability to differentiate a scientific argument and philosophical reasoning thanks to criteria shared by all those who do science fully determines the chances of efficient interdisciplinarity, by bringing into play fields which have disparate relationships with ideologies. Otherwise, researchers of a discipline would lack the necessary skills to identify and reject the offending incursions produced in another area.

Even when (and especially when) they imagine the opposite, there is no worse philosopher than a biologist who stigmatizes the defects of the social sciences, or a sociologist who measures the potential of the life sciences. But a philosopher will probably correct this statement by deriding a still more terrible case to his eyes: that of an individual so naïve, or vain, who might desire to separate science from philosophy, out of the protection extracted from one of the giants of epistemology: Karl Popper, Gaston Bachelard, Thomas Kuhn, Imre Lakatos, Gerald Holton, etc.[11].

11 A highly subjective hit parade, certainly.

1.2. A "strong agenda" for interdisciplinarity?

Actually, the presumption of such a quest would be ridiculous if it became engaged in a competition against the works of these authors. Even more so considering that the diversity of their genius has not reduced the difficulty, quite the opposite. So how dare we tackle an enigma of this size head-on, and what is more, deal with it in half a chapter? The explanation lies in the fact that we are not going to play the same game: "they" wish to define science from a philosophy nourished by logic, physics and mathematics, while these pages wish to track philosophy from science, and will therefore benefit from a decisive advantage. Because "they" have theorized science while being careful not to illuminate the philosophy from which they operate, whereas here, science acknowledges the bet once made on methodological materialism. Strictly speaking, this bet does not admittedly "justify" it, nor does it specify its plan of organization, but has generated it and maintains itself upstream, as a founding resolution: an axiom. Philosophy may theorize about a mode of existence at its discretion: this will not evaporate, regardless of the social and historical density of the fact.

1.2.1. *Popperian demarcation, or exclusion decreed from the outside*

This distinction completely changes the situation, as will be understood in the light of a decisive turning point: Popper's introduction, in 1934, of his famous "demarcation criterion", according to which the author "shall certainly admit a system as empirical or scientific only if it is capable of being tested by experience" [POP 05, p. 18]. Thirty years later, the epistemologist described how this concept came to his mind: with this triumphant and haunting principle of falsifiability, "it was the problem of drawing a line (…) between the statements, or systems of statements, of the empirical sciences, and all other statements" [POP 62, p. 38].

To our knowledge, nobody detected the anomaly: contrary to the opinion issued for decades by countless reviewers, the boundary between authentic scientificity and surrounding counterfeiting was not deducted by using the demarcation criterion. The power of falsifiability emerged in the opposite direction to a prior imperative: subtracting true sciences – *already listed ones* – from the "rest". Popper was not striving to recognize a whole: he already had one from the start. He wished to design a process for justifying the separation performed *a posteriori*. The exclusion of history, anthropology

and sociology then represented a cause, not an effect. Subsequently, this knowledge legislator was vexed by an additional consequence: falsifiability also ousted estimable Darwinism. As a consolation, the benevolence of the epistemologist conceded a certain value to the method developed by the English naturalist [POP 81].

This time, the fragility of this arrangement did not go completely unnoticed, and the sovereignty of the order lost a bit of its serenity, despite the many conciliatory efforts established by the Popperian goat and the Darwinian cabbage. Convoluted and rather comic attempts, thanks to which the hope of recovering a place in the paradise of sciences "worthy of this name" also made sure not to disavow the precious eviction of human and social disciplines. Unfortunately, these wishes were incompatible: rescuing Darwinism inevitably brought these unacceptable neighbors back, in the measure that the single effective disqualification of the Popperian decree involved the rehabilitation of comparative analyses, with their potential rigor. Following Lamarck, Darwin developed and extended morphological, physiological and taxonomic comparisons by opening them to temporality, and consequently, to dynamics. Skeptics may have gossiped as much as they wanted regarding the banter of sociology, but this could not change the fact that in parallel, the latter shared a similar approach [GUI 89].

The success of Popper's theses is partly due the connivance of his readers with the hierarchy of sciences and the top position of experimentation. Falsifiability thus imposed itself both as its consecration and as its adornment. The absurdity of the whole affair nonetheless became obvious with the achievement of logical inclusion: *in the register of observation, the totality of scientific experiments makes up a subset of comparisons*. Fifteen years before Popper published his determining book, the mathematician and logician Alfred N. Whitehead, who later became a philosopher, had stated the following, at the end of a series of conferences: "Our knowledge of the peculiar characters of different events depends upon our power of comparison" [WHI 07, p. 189][12]. After the burst of the "demarcation criterion", the topic experienced a sharp drop in epistemology[13], which nonetheless represented a frightening desertion in terms of the global scale of the challenges.

12 Initially published in 1920.

13 We have vainly sought significant counter-examples in classic books published in this field after the advent of Popper's criterion: it almost looks like a fit of bad temper. And the

Experience anticipates and sharpens the comparison between the before and the after, depending on the presence or the absence of a characteristic event. It opposes situations which differ only by the participation, or the absence of a certain variable. Secondly, the technical mastery of preparation *often, but not always*, enables the reproducibility of the operation and the quantification of results through unlimited repetition. We will often insist on this restriction so frequently blurred at will: reproducibility is not necessarily attached to falsifiability, to the point of becoming indispensable to it. Popper also held contradictory statements on this topic, he who, for example, had enthusiastically praised the work of Arthur Eddington during the solar eclipse of 1919 (measurements being then compared to the predictions of relativity theorized by Einstein): by anticipation, astrophysics carefully shaped experiences dealing with situations for which the heavens hardly permitted an exact repetition[14].

In these conditions, a rough, but inevitable question arose: what could be the value of an epistemological theory which dissected the subset of experimental conditions, disregarding the whole of comparisons which, nonetheless were included in it? And by this, not only do we refer to Popper, but also to every student, dissident, rebel or opponent who has persisted in this huge gap. At no moment could any of them explicitly question which elucidations comparison would be likely to provide, or which truths it could make available. Who could believe in an innocent omission, when ecology and the social sciences themselves, highly comparative types of disciplines, were also experiencing discredit due to their lack of contact with at least *one* comparative operational procedure among others?

METHODOLOGICAL ASSERTION 1.6.– Experimentation is a type of comparative procedure, characteristic for the predetermination of maximum control over the observed context. It all the same remains a category subjected to a whole set of comparisons. And epistemology commits a prohibitive methodological fault by assigning scientificity to the experimental, which it denies to other modes of comparison, since it has never mobilized its competences in a systematic analysis of the skills throughout the general set, nor in the relationship between these competences and the qualities specifically attached to the favored subset.

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consultation of the indexes, even in pedagogical volumes, increases this feeling of anomaly: such abandonment deserves a particular study in itself.

14 See [GUI 15a].

METHODOLOGICAL ASSERTION 1.7.– Philosophical criticism of the potential of a scientific field may become insignificant, unless it is accompanied by a complementary analysis of the activity deployed by the philosophy regarding the topic in question. Epistemology has pronounced an imperial verdict, despite the fact that it has barely visited only one section of the field of research which is supposedly its own, by definition.

Whitehead's remark, reported earlier, promised such a broad exploration and a far less arbitrary exploitation! Despite its brilliance, Popper would have reduced it to ridicule. Given the prestige surrounding this name, the present comment will have difficulty in dodging an accusation of presumptuousness: yet, rejection does not aim at the sagacity of the invented criterion, but the abusive purpose of prioritizing research through its intermediary. Reverence maintained towards the microcosm of experimentation, until the appearance of evidence to the contrary (or refutation), the practice of comparison unreservedly offers itself to the materialistic bet of science. Why has no epistemology been designed to theorize on this exercise, if it were not for a stubborn prejudice against allegedly "soft" sciences? A corollary can be deducted, together with the previous "assertion".

METHODOLOGICAL ASSERTION 1.8.– Philosophy defines science at its discretion. But, in the name of its practice and its method, science must also define itself and, therefore, delimit its demarcation in relation to philosophy from the inside, at least to give back to philosophy thoughts which fall under its sole responsibility.

This assertion is crucial. Soft sciences are those which are not strong enough to defend themselves against philosophical contamination. Experimentation may look like a vaccine against it, except that it has the disadvantage of not producing antibodies, contrary to what Popperians may have seemed to imagine.

1.2.2. *Scientific self-management and the requirement of symmetry*

The hierarchization of knowledge is the result of a philosophy which science cannot ratify. In the opposite sense, the denial of hierarchy belongs to the opposite philosophy, which science is not much more obliged to credit. A judgment formulated in terms of natural inferiority or superiority,

as well as the antithesis of the said judgment, is equivalent to making an assertion in *absolute* terms, implying metaphysical ends. Otherwise, the advantage, or disadvantage, would be related to the provisional choice of a target resulting from specified motivations.

METHODOLOGICAL ASSERTION 1.9.– Science has historically developed as a radically dissenting type of knowledge. It has refused to be mixed with knowledge independent from its method, but has refrained from any hierarchical clause against it: an affirmation of its superiority never arises from its practice, and is not pronounced within its framework. As for a possible authority on the political or economic level, this could stem from social success or extrinsic presumptions.

For the time being, it is of little importance for laboratory ethnography to mention regular and blatant breaches against the previous assertion. This is a rule of internal epistemology: one of the "operational principles" previously mentioned. Let us observe that two current messages (or slogans), repeated over many pages, will increasingly lose their tendentious solidarity. The first one posits that every belief is respectable and that no precedence can be established among several beliefs. As such, science is only one among the many, "neither better nor worse than the others". The second assumption asserts that there is no good or bad science: only science "as it is done" (without attributing special responsibility for this drift to Callon or Latour). Thanks to an effect of symptomatic blindness, no one seems in a hurry to draw an immediate logical conclusion: *science, estimable in the same way as any other belief, must retain the right to define itself, in its own terms.* Those who deny the distinction between good and bad science consider the "scientific belief" from a distant viewpoint. Its supporters, for their part, only agree on one point: the irreplaceable contribution of a stream fed with an accompanying controversy, intended for reinforcing exactness and evacuating error. In science, there is good and there is bad, which, from the outside, could resemble equally estimable beliefs.

Nobody besieges a fortress while living there and vice versa. The relativistic discourse implicitly relies on a gift of ubiquity, as if it were Schrödinger's cat orienting the experience of its life by wondering whether it can still meow inside the box. The paradoxes which inexorably haunt relativism in anthropology (see [JAM 10]) fall in the face of a stubbornly internal epistemology coming out from the trap, by remaining within the boundaries of science. This would help "scientific belief" to conquer itself

back by "extraditing" the good advice of philosophy, with a make-up layer of sociology (or not): interesting, maybe, but optional.

METHODOLOGICAL ASSERTION 1.10.– Science does not result from the appropriation of a revealed truth, but from an effort to approach truthfulness. Paradoxically, academicians who "respect" beliefs by principle, and refrain from judging a dogma, feel free to encourage science to stand out, by not defending any acquired truth. However, these arbiters always begin by attacking the core of scientific belief, which should nonetheless also benefit from unconditional respect: method[15].

By accident, fashion sometimes seems to quickly catch an original idea: the question, a recurring one, of "symmetry", occasionally mentioned during the previous pages, should now be clearly exposed. In Great Britain, in 1976, David Bloor published *Knowledge and Social Imagery*, an unusual book which advocated for a "strong program" for the sociology of knowledge, by following four "principles". In summary, it should be:

– *causal*, that is to say, it should find the conditions that generate beliefs or knowledge conditions;

– *impartial*, by jointly explaining, and this with the same concern for accuracy, the true and the false, the rational and the irrational, success and failure;

– *symmetrical*, in the sense that the same modes of explanation should be equally applicable to true and false beliefs[16];

– *reflexive*, in the measure that the explanatory forms will tend to turn around towards sociology itself and be applied to it [BLO 76, p. 7].

Modestly, Bloor stated that he was adding really nothing new, but contented himself with sorting out omens from classical works. A short word about the author and the context, so surprising has its "recovery" has been. Bloor holds a cognitive relativism marked by a voluntary optimism: a

15 The most astounding cynicism oozes in the excuse given to the derogation which overwhelms science: a typically Western "arrogance" is attributed to it. But the translation of scientific findings into evidence and supremacy is systematically performed *after* the work done by science, and philosophers occupy a prominent role in that. We could as much blame researchers for the ridiculous "scientifically proven" tag, trumpeted in *advertising* spots.

16 We nowadays refer to "restricted symmetry", as opposed to "general symmetry", a philosophical form favored by Michel Callon and Bruno Latour.

position which is not at all demobilizing, but packed with contradictions. By the way, he finished his book discovering he was doubly exposed to the accusations of relativism and of scientism: on the one hand, a certain allergy to history, and, on the other, trust in the solidity of science, issued from the fact that it never stops.

At the beginning of this writing, we planned to expand on the "principle of symmetry". In our opinion, its approval does not necessarily lead to any adherence to any particular kind of school, or to the widely acclaimed extensions by Bruno Latour[17]: the above-mentioned content of MA 1.7 testifies to this ability. Nevertheless, a preliminary remark regarding the purpose of the "strong program" suddenly caught our attention: Bloor was expecting to incorporate values held for acquired by other disciplines. In order to join a concert? Could his project, focusing on a common canvas, actually support interdisciplinarity? For all the sciences not immersed in beliefs, the proposal would then have to be re-read by replacing "belief" by "phenomenon", as Bloor urged us to superimpose success and failure to true/false or rational/irrational relations. In this case, a temporary blur is not a problem as long as it is not masked: the treatment of analogies gravitating around complete/incomplete, coherent/contradictory and, on the horizon, perfect/imperfect, can wait a bit. Let us stand for a distinction based on the essentially idealistic content of belief regarding the phenomenon, which, for its part, does not dictate precedence when it absorbs the ideal, the material and their interweaving.

A quick overview leads us to the conclusion that the three *causal*, *impartial* and *symmetrical* clauses will smoothly integrate the hypothesis of a "strong interdisciplinarity program", framing the cooperation between the life sciences and the sciences. However, reflexivity obviously raises a difficult problem. Everyone knows that a disdainful pragmatism is regularly ironic regarding the pompous but futile complications of an "epistemology of epistemology", a "sociology of sociology", or a "history of history". Let us concede to pragmatism that these circularities elicit the danger of comfort: anyone entering this loop must guard himself against the desire to stay there *ad vitam aeternam*. But let us correct the disdain: the accusation of futility crumbles for those who manage to get out of the whirlpool with real lessons.

17 Although this contributed largely to promoting the work of the British author, Bloor opposed subsequent "extensions" in a text entitled "Anti-Latour" [BLO 99].

1.2.3. *Symmetry and reflexivity in the nature/culture couple*

So let us try. The dazzling principle to which Bloor owes the notoriety of his proposal is symmetry. Its general adoption has particular priority within the frame of interdisciplinarity nature, since it conditions the conquest of impartiality in discussions. It also clarifies the fact that social science makes a prohibitive fault when it pretends to place itself between the social knowledge that it observes and the society which surrounds it. It should be included among the specifications of any science that a pact should never be concluded with common sense, which Maurice Godelier confirmed in terms of perfect clarity regarding his own discipline: "anthropology cannot exist and develop as a discipline belonging to the field of sciences, except by inventing the procedures which enable those who practice it, to become methodically decentralized, not only in relation to the categories of Western thought and values, but in relation to all the cultural universes of reference" [GOD 97, p. 108], our translation). Otherwise, discipline will sooner or later be led to endorse the habit of the lawyer or the prosecutor, pleading for a "common sense" against each other. Thus, ethnology has mainly inclined towards the exotic against Western[18] thought for half a century, at the expense of the original scientific viewpoint it would have had to refine.

The theme of cannibalism illustrated this, when "benevolent" researchers deployed cunning tricks in order to obtain a dismissal for lack of evidence[19], rather than to question the indictment of a *crime against philosophical humanism,* imputed without fully spelling it out, by Western culture to man-eating warriors. It would have been more instructive to first observe that "cannibals" see no disruption between nature and culture, contrary to scandalized societies. The reference to symmetry thus becomes relevant in this regard. Taking things a step further, considering the relationship between nature/culture, what to think about the fact that its existence was recently reduced to the condition of Western fantasy, to be stored among obsolete ideas, because it bumped into the existence of cureless societies? Again, repudiation takes us, oh once again[20], back to Descola [DES 13] and Latour [LAT 99].

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18 Much more firmly than in the days of colonialism, when it used to serve the other side.

19 See [ARE 79, OBE 05] and our critical analysis on this topic [GUI 10, 12 GUI, GUI 13].

20 In 1999, at the threshold of the accreditation to supervise research (*Habilitation à Diriger des Recherches,* in French), a jury of "pure materialists" already indulged in wishful thinking.

The fact that the discontinuity between nature/culture – we will refer to it as *N/C* from now on – became filled with sense in the wake of monotheism, and not elsewhere, is not a lesson in itself. In the first place, the principle of symmetry requires us not to judge the position of a "side" with arguments reserved to the other side. But does the scientist have to choose between one of the two options? Surely not: even if the "traditional" Western attitude acquires an "exotic" turn, nothing forces it to espouse the shaman's point of view. Symmetry takes place at a second stage, equally dealing with the versions in presence, in order to approach a theory related to culture, extrinsic from *science*. We will then imagine the diagram of an isosceles triangle in which the rationalist pole gradually recedes from the base. Except that the situation becomes widely complicated in front of a socially constitutive dissymmetry of the sides in presence. Peoples who do not consider the hiatus generally have *an* orderly culture and *a* structured perception of human relationships with their environment, and an internalization of the historical dimension (India, China, etc.), or may not have it (Melanesia, Black Africa, the Amazon, etc.). Those who draw a border, especially when they spare a place for science, display a variable range of approaches which spontaneously take history into account, without exception. *N/C* varies from one monotheism to the other, from one philosophy to another, but also from one science to another. And, incidentally, it thus appears easier to elude it in a "cold society" turning away from history, than in a "hot society", recording its presence[21].

The objective need of the principle of symmetry considers intuition backwards, at the same rhythm that the accumulation of dissymmetries faced by the researcher increases. The imbalance of images between the homogeneous culture of a cold society without *N/C*, on the one hand, and the proliferation of contradictory versions of *N/C* in a composite hot society, does not make the rule idle: it breaks down its use in a succession of degrees. We cannot place on the right scale of the balance a few shamans, and on the left scale, St. Augustine, Lamarck, Darwin, Freud, the Chicago School, the IPCC, Donald Trump, etc. Besides, on which of the two scales should we place Descola and Latour? After all, the situation changes depending on whether we belong to a culture which never conceived *N/C*, or despite our

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Philippe Descola and Bruno Latour "nonetheless" agreed to become a part of the jury, the first one as president: we are grateful for this elegance. A mafia thug would probably say about these disagreements that "it's not personal".

21 The great duality of "cold" and "hot" societies comes from Lévi-Strauss [LEV 73].

denial, we belong to a context in which this relationship traditionally occupies a central and dominant role in how we perceive our world.

Better not to follow this path, for it could take us back to the fog that envelops the relationship between philosophy/science: we would learn nothing new. For the moment, let us focus on an independent topic: scientific disciplines tend to stay away from political, moral and ethical debates seeking to repudiate *N/C* (or to rehabilitate it), but, in the measure that the enigma makes sense *in their territory,* they prefer to discuss within their walls and on their field, that is to say, at a distance. The disruption that Descola refuses by adhering to the feelings of exotic societies only illuminates us incompletely as regards what they practically live[22], their evolution and their reactions to events. This anthropology does not abandon cosmogony and its timelessness. It nevertheless happens that, for some sciences, culture dissociates itself from the nature it belongs to, by an incredible ability to change its inscription in an environment: this is a natural phenomenon, certainly, but it deserves a name, like the innumerable natural phenomena characterized by originality. Paleontologists (and filmmakers) have speculated that the Tyrannosaurus only detected its prey if it moved. The same would apply to us regarding culture: we see it when it moves. And this point is interesting both for biologists and for sociologists, or psychologists, albeit for various reasons and from different angles.

The principle of symmetry will never prevail between science and philosophy. On the other hand, it fixes an essential imperative for interdisciplinary communication. A precious chance to solve controversy, rather than by power balances at the level of institutions or ideologies. Every definition of *N/C* sets in motion two specific definitions of nature and culture: as irreconcilable as they are in philosophy, those who adhere to the "materialist bet", enter the arena of scientific discussion in order to sort it out. Here, a crucial dilemma will suffice to "profile" the expectation: at the same time, *N/C* traces a biological/psychological threshold and an

22 A few years ago, in order to formulate the idea, we would have resorted to the *etic/emic* duality, with Descola taking sides for the emic in order to abolish *N/C*, but not solving anything at the etic level. Nevertheless, as Jean-Pierre Olivier de Sardan [OLI 98, OLI 08] has shown, the idea can too easily lead to landslides and uncontrollable overlays, at least in its current state. In addition, the emic easily splits its points of view (that of the ethnologist and of the shaman, for example) and quickly reduces the etic to what remains once culture has taken it all.

ecological/social human threshold. An ideological rivalry between controversies has underpinned this confusion for more than a half century.

Let us summarize. Spontaneously, biologists see *N/C* as a synonym, or as an immediate extension of the innate/acquired pair, at its best. Cultural anthropology in the United States once encouraged this perspective, by analyzing the relationship between the pair *nature/nurture*, to which it basically adhered. However when Lévi-Strauss, quickly followed by most social sciences (at first), ranged the universal features of human groups on the natural side, and their variations on the cultural side [LÉV 49], he enthroned a problem unparalleled to the contradiction between instinct and learning: *N/C* potentially escaped the authority of the psyche and became a prominent challenge for those behind Durkheim [DUR 95], considering that social facts are no more reducible to psychological facts than to biological facts. However, this position has triggered permanent discord since its emergence. The idea that society does not result from an aggregation of psyches is not self-evident, and thanks to Lévi-Strauss and Sahlins, we have seen to what extent the power of the sign or the symbolic faculty have implicitly challenged it.

We will neither deepen the content of this discordance nor the declination of the constituent patters of *N/C*[23]. At this point, an observation will suffice: the ecology which addresses the nature/nurture relationship in individuals does not have the same interlocutors nor the same conversation topics as those interested in the sharing of invariant/diverse in collectivities. Three interdependent dimensions dispute causal precedence in the emergence of a supposed cultural phenomenon: the cognitive potential, the transmission from one generation to another and the differentiation among groups [GUI 98a]. So far, each of the three has maintained asides within a reduced multidisciplinarity, under the umbrella of complicity. However, in the event of an interdisciplinary project that is coherent and comprehensive, success would mostly depend on the uninterrupted monitoring of a *sine qua non* condition for dialogue: the respect for the principle of symmetry, the sole way to ban the metamorphosis of the arguments in their excursions between sciences. For example, nature/nurture and invariant/diverse cannot communicate with each other: one is in line with the individual, whereas the other sides with the collective. An agreement on the

23 For a more complete analysis of these divisions, see [GUI 94] and a beautifully titled symposium called *La culture est-elle naturelle?* [DUC 98].

separate treatment of the two levels might pave the way for the introduction of methodological symmetry and, from there, the access of the arguments to "causality", and later, to the "impartial" reception of allegations.

Demand, today, smells a candor inherited from the time of the Enlightenment: utopian voluntarism. However, such an impression is derived from resignation. Considering the blatant rudeness of negligence which takes advantage from surrounding indolence, how not to estimate that progress in these areas is in no way super-human or super-natural? Primatology illustrates the stagnation resulting from such nonchalance. In the early 1990s, William McGrew revealed stable behavioral differences among groups of chimpanzees, related to technical activities and which were not in reply to a variation in environment-related influences [MCG 95]: a statement which was later confirmed and specified by several teams[24]. And this represented an extraordinary opportunity: another species, different from ours, meets all the criteria of the ethnological definition of culture, without the help of articulated language! Who could dream of a more magnificent platform for launching once more the exchanges between anthropologists and zoologists? But while some persisted in trivializing the case and reintroducing it in orthodox ethology, others simply ignored this picturesque trifle. If such a project had succeeded, the respect for the principle of symmetry would have encouraged a common inventory of questions so that they are meaningful for everyone involved, and this would have prevented the usual degradation of the questioning of the other as an unmanned avatar. However, the project is still waiting, and the expectation regarding the rule of symmetry only keeps its naïve appearance because these confrontations attract very few people[25]. Does the silliness of interdisciplinarity managed in "clubs" based on complicity seem any less childish?

In short, in the expectation of a serious and lasting renewal, the principle of symmetry promises to guide researchers towards mutual respect, other than by the force of good intentions. And above all, it will facilitate the immediate detection of the expedients deployed so as to avoid it: for example, when one of our favorite scapegoats, Sahlins, claims that culture is not directed by the primitive emotions of the hypothalamus, such emotions instead being organized

24 For a recent review of the facts, see [BOE 12].

25 There is no fatality, except for the fact that the scarcity of success regularly brings us back to the same cases. Too bad: in 1966, the richness of the debate in the *Man the hunter* symposium aroused high hopes [LEE 68], no direction being frozen at the time.

by culture [SAH 76, p. 13], he loses the right to condemn the elementary determinism of sociobiologists, since its reversal restores the grossness of causality[26]. The same happened when he launched the symbolic faculty to the assault of animal proteins, under the flag of cultural materialism, claiming the foundation of Aztec cannibalism [SAH 79]: strengthened by the fortuitous meeting of a sewing machine and an umbrella on a dissection table[27], a surrealist might arbitrate the game by using a pan flute.

The rightful place attributed to such phantasmagoria in posh scientific publications is disconcerting. "Hard" materialists are certainly worse and the scabrous theme of cannibalism (which held us for years in the perspective of a symmetrical ethnology) provides an unequaled anthology: nostalgia clings to the memory of the anthropophagic mode of production characterizing the ancient Paleolithic, the fall of Neanderthals due to the spongiform encephalitis spread by predation on their incongeners, and this Papuan village of about 60 people who would acquire 35% of their protein needs by eating a biped per week, that is to say, 60 consumers eating 50 related species per year, at the risk of traumatizing regional demographics! None of these whims can resist an interdisciplinary discussion, yet if it was held, we would end up continually drawing illustrations on this register, so as to benefit from the "magnifying effect" produced by obsession.

METHODOLOGICAL ASSERTION 1.11.– Interdisciplinarity and the principle of symmetry include each other as a condition for practical existence. Only the symmetrical application of an argument on two disciplines welcomes a joint discussion, with a linking of the contexts in which each argument is deployed. In return, supporting interdisciplinarity creates all the occurrences of symmetries between sciences, without further excursions to their surrounding boundaries.

The following chapters will gradually give relief to these abstractions and will try to turn them into practical guides.

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26 Michel Panoff, student but not a disciple of Lévi-Strauss, dismissed the circular rationalizations of functionalism and structuralism in an essay which, 40 years after its publication, extraordinarily preserves its up-to-dateness [PAN 77].

27 Emblematic image which reminds us of Lautréamont.

1.2.4. *Two modes of interdisciplinarity*

And what about *reflexivity*? *A priori* suspected of representing an obstacle, it has been floating in suspense for a long time. In fact, this could largely explain why a theoretician as subtle as Sahlins could have "fallen" to expeditious replies in the face of sociobiology and functionalism. By subsequently analyzing the role of *relationships* and *interactions* within their lectures, we will grasp why reflexive disciplines are somehow muzzled vis-à-vis frank causalities: Sahlins could not erect "symbolic power" as primary cause in a problem exclusively controlled by social anthropology. If that happened, the determinism of the ideal would have jumped to his colleagues' eyes and compelled them to react "internally". But when facing an entomologist who assumes the selfish temperament of the gene, or a colleague who reduces religion to mental tinkering wishing to excuse the intention of devouring war prisoners, controversy accepts the rusticity of a counter game. In other words, reflexive thoughts suffer the temptation of using non-reflexive theories, as when biologists do some sightseeing in philosophy before returning home with convenient convictions: a totally anti-symmetric back and forth. On the other hand, researchers in fields hardly exposed to reflexivity regularly ridicule themselves by ignoring them when they intend to denigrate the rivalry of the social sciences: unable to criticize from the outside what reflexivity can contribute to the latter, they coldly deny it. Here again, these remarks will later become even more significant, especially in the face of the *anthropological event of domestication*: contrary to what has been argued by some, it can be approached by an objective definition applying interdisciplinary symmetry, but it requires another type of interdisciplinarity, a reflexive one, for apprehending the socially materialized parallel of the relationships between human/animal and human/human[28]. *If there is reflexivity on the side of the anthropologist, the principle of symmetry requires it not to be obscured or marginalized on the side of those he observes.*

28 A simple and sinister illustration will be easily memorized: depriving oneself from the recourse to reflexivity, denouncing genocide would require the confirmation that the murdered community actually corresponds to a race, or is united by a biological bonding. Genocide follows from the interpretation given by the killers, not from the genome of the victims. In France, in 1982, a Prime Minister thus inadvertently mentioned "innocent victims" to refer to non-Jews affected by an anti-Semitic attack.

Following an opposite (symmetrical?) perversity, the accreditation of reflexivity irresistibly slips out to an appalling abuse: namely, a theory that "analyzes itself" and inadvertently leads to the epistemological justification of its ambitions. *Theory* is then metamorphosed into *discipline*. Of course, we are aiming at Lévi-Strauss, who leaned on the triumph of symbolic structures in order to place social anthropology in synonymy with semiology, as glimpsed by linguist Ferdinand de Saussure, at the risk of transforming ethnographic materials into residual mass[29]. Let us suggest a transposition into the vocabulary of Kuhn [KUH 62], Popper's great rival: the "paradigm" extends reflexivity until this is considered a "normal science". A mutual supervision of reflexivities by disciplines which use them would be the only imaginable obstacle against such excesses but, to say the least, a collective effort in this direction can hardly be foreseen in the short term. Contrary to the prevailing opinion among the followers of Kuhn, paradigms abound in the human sciences which, on the other hand, cruelly suffer from a lack of normal sciences.

Pending the examination of the impacting points of these imbroglios, from a strictly logical point of view, everyone can understand as from now the impossibility of a symmetry between a discipline regularly relying on its reflexivity and a discipline which cannot access it: the sociology of sociology has a certain meaning, but the molecular biology of molecular biology, or the morphology of morphology, can barely tickle imagination through analogies which tacitly transform expression when repeated. The following three statements are solidary.

METHODOLOGICAL ASSERTION 1.12.– In the context of a multidisciplinary approach, no-one should bring forward a challenge to the legitimacy of another discipline during the phase of practical cooperation, under the risk of completely ruining common effort.

METHODOLOGICAL ASSERTION 1.13.– An interdisciplinary approach that incorporates the reflexivity of arguments, that is to say, a prescriptive

29 Symptomatically, the operation arose at the moment when Lévi-Strauss gave his inaugural lecture at the Collège de France, that is to say, when he accessed the top of French anthropology [LEV 73], see also [JAM 93]. Let us agree that, for structuralism, this complicates the reproach that sociobiological theory deserves when it is assimilated to the social-ecological discipline, without encountering any opposition [PAR 17]: the charge would ultimately concern the fact that the process is deprived of all subtlety apart from reflexivity.

reversal on the issuing discipline, can only look for problems it can use, otherwise it would be dismissing symmetry.

METHODOLOGICAL ASSERTION 1.14.– Two primordial forms of inter-disciplinarities combining the life sciences and the human sciences diverge from the outset: while one incorporates the rule of reflexivity, but reduces the range of invited disciplines, the other practically prohibits the recourse to reflexivity, so as to preserve symmetry to the largest possible extent.

We undoubtedly need both of them, under the express condition of not mixing them up. A delicate issue concerns the ability of a discipline to muzzle its own reflexivity during the time frame when it dialogues with types of knowledge reticent to its intervention. On this point, Whitehead was quite optimistic: "we can think about nature, without thinking about thought. I shall say that then we are thinking *homogeneously* about nature" [WHI 07, p. 3][30]. In this case, it would lead to a "homogeneous interdisciplinarity".

METHODOLOGICAL ASSERTION 1.15.– The principle of reflexivity is only scientifically admissible if its application effectively protects against the tendency to circularity.

An uncomfortable remark should be inserted here: a sociologist who speaks about ecology does not necessarily participate in interdisciplinarity. Most of the time, he does not even approach it: the vast majority of seminars, symposia, or books regarding the relationship between social entities and the environment infiltrate a reflexivity which marginalizes scientific ecology and limits its response to a technical provision of information. However, such works frequently join the catalog of interdisciplinary research which is thus artificially enriched, at the cost of a misleading dilution, which then benefits anthropology: authorized by the symmetry of abuse to inflict exotic societies with a process that sociology applies to its "parent society". More abruptly: it is nowadays possible to call oneself interdisciplinary while exclusively referring to oneself, to one's own position regarding the problem at hand, and the meaning of an inquiry on the outside, if we still had time for that.

METHODOLOGICAL ASSERTION 1.16.– The anthropologist is never required to choose between the perceptions of the community he is studying and

30 The term is underlined in the text.

those of his own society, because he himself does not appear in between them: science is in no way affiliated to any "sense" nor to any "common sense", whether at home or in any distant land. The only common sense with which a science should argue is the one that dwells in another science.

Going back to the duels that Sahlins initiated against all kinds of reductionism, it becomes obvious that these impudent theses made their initial mistakes by rejecting the first three necessities (of which they were not completely ignorant, since they were careful to indirectly reproach them). That said, the answer of the defender of the "symbolic function" also set himself free from them and succumbed to the intoxication of circularity. Symmetry protects reflexivity from the inside and vice versa: we fall into absurdity if we attack one by means of the other[31]. Far beyond Sahlins and Levi-Strauss, the lawsuit brought by the specialists of the social to sociobiology thus cowered on the sole ground of reflexivity, at the risk of placing an opaque screen in front of a challenge related to observable social facts. Yet, the reply could have been more powerful in terms of biology itself had it included technology, economy and demographics, rather than exclusively targeting the elusive power of the sign[32].

Quoting Lévi-Strauss and Sahlins is equivalent to relying on charismatic *leaders*. However, around them and behind them, an immense stream has drowned reflexivity in order to better circumvent this symmetry, which a non-negligible share of zoologists would have received with satisfaction. Why such an avoidance, if not because the flourishing of methodological materialism restoring the importance of the gesture at the same level as the brilliance of speech[33] incarnated a more formidable and dangerous threat? Without introducing a scheme, the rejection of rustic determinism has "disguised" a more general disdain, by calculating a half-word pleonasm contained in the expression "vulgar materialism". While it is true that they praise interdisciplinarity, they do so by making sure to place the scrambled

31 It is here that the thorny renovation of the division between a non-reflexive *etic* and a reflexive *emic* could perhaps become relevant, and that is the reason why we brought it up.

32 In 1983, one of our first books insisted on this point [GUI 83]. This reminder does not intend to boast: it sets in time an already noticeable decrease in the debate.

33 Unknown to the Anglo-Saxons, in the late 1970s, the ethnologist, technologist and prehistorian André Leroi-Gourhan, author of *Le geste et la parole*, was considered in France as the only credible rival of Lévi-Strauss.

contingencies of action at a highly respectful distance from the perfect structure of the symbol.

Some will revolt against these considerations by judging them grotesque or spiteful. They will barely pronounce a too predictable verdict: we have not understood the great authors we are talking about. However, *big men* sometimes express insolent coquetries likely to make their colleagues uncomfortable. At the beginning of *Mythologiques,* Lévi-Strauss already anticipated the criticism to his method, without seeking to significantly modify it. On the contrary, his theorist pride rebelled against diplomatic compromise: "it would be hypocritical not to reach the ultimate consequences of our reasoning". And he abandoned himself to a sumptuous effrontery, to which we will respectfully leave the last word. Regardless of the tiniest concession, the allegation regarding the potentialities of reflexivity (with which we certainly do not agree), has the merit of confirming the consistency of the "core" of the question:

> "For if the final aim of anthropology is to contribute to a better knowledge of objectified thought and its mechanisms, it is in the last resort immaterial whether in this book the thought processes of the South American Indians take shape through the medium of my thought, or whether mine take place through the medium of theirs. What matters is that the human mind, regardless of the identity of those who happen to be giving it expression, should display an increasingly intelligible structure as a result of the doubly reflexive forwards movement of two thought processes acting one upon the other, either of which can in turn provide the spark or tinder whose conjunction will shed light on both" [LÉV 83, p. 13][34].

1.3. Materialism in the face of the ideal

Materialism challenges idealism in philosophy, whereas "the mental[35] and the material", according to the shiny title of a book by Maurice Godelier

34 Michel Panoff pointed out this passage to us. Although a regular reader of *Mythologiques,* he came upon this rather disconcerting statement only later.

35 TN: Godelier makes a distinction between *the ideal*, in the French sense, understanding it the perfection of thought to be achieved, and *the mental*, which simply represents the consistency of ideas.

[GOD 84], regularly meet in the scientific efforts of anthropology[36]. But what about diagonals? That an idealism hoping for a soul to transpire in an atom, scrutinizes its materiality at the microscopic or cosmic confines of physics is in no way surprising: we can move on. When Einstein revolted against the idea that "the old man" plays dice, the affair became more delicate, but that does not concern us any further. On the other hand, mentioning the attention paid by materialism to the ideal may somehow disconcert thought. Not because of a tradition that tends to conceive the ideal as an epiphenomenon of the material, via the earlier epiphenomenon of life, but because within the scientific bet, we might expect methodological materialism to refine an appropriate approach, apt for assuming pending interdisciplinarity. However, this vital confrontation almost never leaves the stratum of innuendos and tacit arrangements.

Let us be clear: technology, economy, history, archaeology, or geography frequently scrutinize the impact of ideal ripple effects on the objects they observe. In fact, they have no choice but to see them everywhere. But on the opposite path, the specialists in the symbol and the sign are infinitely less curious to clarify the various routes that their topic of interest acquires when it practically takes place. *Grosso modo*, once they were able to isolate a kind of idealistic cohesion, they jealously preserved this impermeability, apparently a guarantee of the purity of their investigations. Emancipating from the contingencies of history involves stepping away from the things that become impregnated with it. Instruments only exist in a fleeting and localized context for their services; structures suggest that they are free from these dependencies and hover over variables. Does philosophy insinuate itself within this hiatus? Undoubtedly yes, and it follows that a materialist method should decipher, on its own, the entry conditions for the ideal in the sphere of its activities.

1.3.1. *The illusory sphere of the ideas*

A thesis supported by Patrick Tort here acquires a special flavor. Although issued in a predominantly distant context, it certainly keeps its reliability in our framework, for reasons which are ironically related to reflexivity: "There is no "sphere of ideas" which can be separated from the

36 For an incisive, invigorating and rare analysis of this tormented encounter, see the decoding of "French anthropologies" by Jean-Luc Jamard [JAM 93].

sphere of *actions"* and "ideas are the *gestures* of intelligence" ([TOR 16, p. 175]; italicized in the text, our translation). This reflection aims at the conditions for an effective epistemology and the "sphere of ideas", whose illusion the author denounces, is in this case related to "the history of ideas" which once reached the heavens. However, it is as a mirror of the latter's methodological inconsistency that, according to Tort, epistemology should make the "bet" of being a science [TOR 16, p. 176]. A congruent bet with the one which has taken us so far (science relying on the tool of materialism), but also, after all, a "reflexive" bet in that the argument supported by epistemology is equally applicable to any science which more or less dissects ideas. If the "sphere" in question is reduced to a trick on the epistemological ground, it is because it does not materialize anywhere in a human culture, and if we could practically isolate it in a culture, it would immediately find its rightful place in epistemology. Have not anthropologists of all kinds lately repeated it enough, that a science corresponds to a culture?

After all, nobody imagines ideas for no reason. Or at least, nobody real: those who succeed do exist under the aegis of myths where, to their misfortune, they are manipulated by spirits of the lowest possible level. Anthropologists have assiduously dreamed about the sphere of ideas for more than half a century: in this respect, kinship structures had the disadvantage of focusing on the biological and social reproduction of human groups (even if the majority of specialists were content to incidentally observe the connection, before plunging back into the meanders of pure logic), whereas with *Mythologiques* there emerged the dream of a "discipline of fantasies"[37].

We cannot imagine challenging the success of Lévi-Strauss on this level or changing a single line of the combination of mythemes and their impressive repetitions, immune to the social friction which trouble men's foundations. However, it is a different problem to decide whether these structures really belong to a sphere of ideas, even if it means creating one. Are the sign layouts underlined by structuralism in any way related to "gestures of intelligence"? Yes, as far as their variations are concerned (but these "adapt" to the circumstances and are rooted in stories). No, if the idea is to focus on invariants which channel these alterations, since the human mind has, by definition, no influence over them: mental *reflexes* hypnotize the quest. What

37 This was evident by the insistence of the followers of Lévi-Strauss who, from the 1970s onwards, claimed that the second part of his work (myths) far outweighed the first part (kinship).

can be isolated in this field would then be limited to a fixed grammar which ethnology might elucidate to the benefit of the study of the psyche and, ultimately, of neurology [JAM 93], without a conceivable reintegration among the social sciences. The observation does not make the contribution of structuralism any more appealing, it barely shows what it could contribute to an interdisciplinary approach since, on this point, interested confusions proliferate: if this ethnology vindicates itself through psychology [LÉV 62, p. 174], it does not necessarily enrich a stereoscopy with a sociological perspective. Let us conclude: an ideal that is "connected" to neurons is dissociated from an ideal which waves between men, and none of the two constitutes a sphere that suits it.

METHODOLOGICAL ASSERTION 1.17.– When the studies devoted to the real detach themselves from the sustainable material context in which they are expressed, they become exposed to an increasingly inextricable amalgam with idealistic philosophies. A temporary isolation of the ideal becomes scientifically faulty when it loses interest in the terms of a return to what is factual.

That being said, the ideal does not interfere with a materialistic method when it is prone to observation, in one way or another. The articulated phrase of a man, or Béjart's choreography, do not stem from the material world any more than the cry of a bird, or its courtship dance. Physics has simplified a part of the debates by establishing that matter and energy are somehow consubstantial within the core of their equations. We cannot deduce anything grandiose from the small universe of biocenosis and societies, except that materialism does not have to justify its need to bring together the observation of things and actions, of producers and speakers, or of actors and thinkers: on the contrary, it is the temporary dissociations which must be accounted for, by explaining their sources, their objectives and, above all, their limitations, on each occasion. Idealism insinuates itself in this reversal of the priority initiative when it demands first motivating juxtapositions and combinations, which postpones the evaluation of established disjunctions, protected by the surrounding "common sense". In a later phase, the same idealism offers other assemblages distilled from a sole ideal, by failing to report that this pullout from the material is in no way self-evident, and barely changes the "nature" of the conversation topics.

METHODOLOGICAL ASSERTION 1.18.– In the eyes of the materialistic method, the ideal is a subset of the material, just as culture is a subset of nature. These parts certainly stand out by producing phenomena which

remain unknown to the "rest" of the totality. But they also share a certain number of features with all totality that generated them. The alternatives proclaimed between discontinuity and confusion, or between sharing and unification, reveal themselves as a philosophical simulation, intended to subjugate a scientific research which constantly strives to dissociate in order to better recompose, and to gather so as to better differentiate.

Thus, we can better understand the phobia of structuralists and post-structuralists in front of "genetic" issues in the literal sense, that is to say, facing the genesis of an innovative reality (language, kinship, or religion). Curiosity systematically concerns the emergence of an ideal from a material context from which it is derived and with which it never fully breaks. But the image is not appropriate for everyone: when Sahlins made an effort to go back to history, he brandished a symbolic faculty which was distanced from his original ordeal, emancipated by a mysterious wrench. A conscientious materialism could not possibly accept this kind of impasse on the pretext of protecting itself from reductionism: the sociology of ideas is not a sociology, the ecology of the mind is not an ecology and the epidemiology of abstractions is by no means a science, because they philosophize about slips, or about harmonies which defy observation, while floating above the real. The contradiction cannot reach them at any specific moment nor at any given location, since they possess a gift which makes science fiction salivate: teleportation.

As a consequence, the present charge will only push already open doors to the vacuum. So, let us go back the other way round, by taking advantage of a happy dissonance at the antipodes of obsolete materialism: the ethnologist Françoise Zonabend, a member of the Laboratory of Social Anthropology (founded by Lévi-Strauss), worked in the rural spaces of Cotentin, near the nuclear facilities of La Hague. When the European area director gave her the order not worry, this activated her researcher reflex: she did so, and went to observe the daily lives of the people who worked in the reprocessing plant. Her research yielded the utterly disconcerting result of a rustic reinterpretation of danger, as distant from the perceptions of atomic physics engineers and researchers as from the apprehension displayed by ecologists. The author summarized the experience she went through, as felt from the inside:

> "Workers at La Hague can be divided between the "kamikaze" who look for risk, somehow brave it, and the "annuitants" who

> constantly calculate and multiply security measures, both groups seeking to tame the radioactivity which surrounds them, by verbally playing between 'contamination' as a source of impurity and disorder and the "irradiation" which gives strength". ([ZON 14, p. 20–21]; our translation)

An autonomous symbolic construction opposes, on the one hand, the irradiation of rays, which is clean and can give strength and dusty, dirty and rotting contamination, on the other hand [ZON 14, p. 188]. The analysis was widely disseminated and studied, but almost left without comment, and even less discussed. The dominant feeling in front of this "wild thought" which interfered with a temple of high technology was that of shocking incongruity, the discovery disturbing both the opponents to nuclear power as well as its supporters[38].

Now the question is: should deliberations on this topic continue to be carried out within a "sphere of ideas"? It would be enough to proceed by analogy with similar dualities in exotic societies, but this rapprochement would immediately fall into a ramble about the resurgence of primitive views in modern thinkers. This investigation requires a constant relationship between an "autochthonous" symbolization and a restrictive material context, and that within an environment already sprinkled by moral beliefs, technical diagrams and political understandings from the exterior and which have the habit of discussing the long term. Except that the multitude of these designs and perceptions has favored the creation of a vacuum: a silence about the daily lives of people confronted with irradiation and contamination, mentally unable to pretend or think "that nothing has happened". Does not the discomfort of specialists stem from the revelation of their indifference regarding this point, satisfied without their knowledge?

Lévi-Strauss concluded *Le cru et le cuit* by encouraging us to "revere" the fact that "matter is the instrument, and not the object of signification". Let us bow our heads. A few lines later, he adds: "Mythic thought only accepts nature on condition that it is able to reproduce it" [LÉV 83, p. 341]. That being said, in La Hague, meaning is the instrument, and not the object of matter, because the myth reverberates within a wavering story: thus, matter interlaces radiation with fear, and signification seems to be employed

38 This type of research belongs to the cases announced in the introduction which will be examined from different angles.

so as to repeat nature. The master of structuralism broke symmetry by deciding to step out from history: the reversal of his formula emerged within the real he so much had wanted to keep at a distance.

METHODOLOGICAL ASSERTION 1.19.– Ideas which are rooted in a concrete and observable environment are not comparable to ideas considered by themselves, without any reference to a historical, local, psychic or social context. An "independent" idea cannot abandon philosophy to access science by its sole means.

The ideal belongs to the materiality of relationships in action, and can sometimes detect it. Functionalism, which strived to uncover unnoticed material stimulation in order to find the "foundations" for mystical ceremonies, never realized that, in that way, it isolated an illusory "sphere of ideas" in the form of a trophy to be conquered: it *idealized ideas*, while these were dematerialized by the other side. From one side to the other, the operation differs dramatically, but at the end of the day, it is the natural imbrications between the manifestations of the part and those of the totality that we refuse to acknowledge. Denying the distinction between nature/culture in (or by) the ideal does not generate the same worldview as denying it in (or by) the material, but this would be equivalent to placing one as an extension of the other, that is to say, to reforming altogether an underlying avatar of the nature/culture relationship.

The confusion will become evident by transposing the strategy to a slightly misadjusted ideological obsession, namely, the emergence of a new phenomenon within nature: sexual reproduction (written here as SR). Is there a nature/SR relationship that may account for a rupture with SR? Males and females are materially different, or does the ideal also preside over this distinction? Should we go back to parthenogenesis so as to abandon the tendency to domination which SR too obviously supports? The example was not randomly chosen, since it illustrates the principle of symmetry: the implications of sexual opposition go hand in hand with those of culture. However, if we agree to consider SR and culture as two significantly different innovations in nature, it is then difficult to advise students: reproduce all the key arguments of nature/culture by replacing culture by sexuality. Using this mirror, the masks of many refined sophistries concerning the essence of culture would spontaneously be torn off.

The assemblage of the material and the ideal in the representation of reality creates a major difficulty experienced by every socioecological interdisciplinarity, at varying degrees: its resolution primarily depends on effective resistance against the trend of self-satisfaction of reflexivities. For scientific methodology, all circles are vicious, virtue only emerges from spirals: this statement exceeds the joke, which will pave the way to some extensions in Chapter 4. In the meantime, let us observe that many philosophers love the feeling of completeness derived from going back to exactly the same starting point, as if to confirm the signature of a "smoothly" executed reasoning.

1.3.2. *The three entries on human worlds*

Regarding an interdisciplinarity which might confront the natural sciences against others, the "humanities" for example, the ultra-dominant feeling, for more than a century, has attributed failure to the second group. This applies to the crude observation, as long as it does not even suggest congenital infirmity, or a curse fallen from heaven: the deficiencies of research dedicated to humans should not conceal the oddities that these concretely disseminate in the middle of the "normal" universe and which significantly complicate the task. Let us give a concentrated expression of this objective difficulty through the prism of its emergence in nature[39].

Outside from our species, animal reality depends on a unique door: its biology. A broad passage built upon the multitude of seizable agents in the body: genes, proteins, enzymes, pheromones, but also varied and multiple hosts, regular or sporadic, hostile or not. Then we have relations, also biological ones, with the components of the environment. But in humans, the observation meets three immediate openings: its biology, of course, remains active, but has to compromise with two other *materialities* which it cannot completely influence, whose organization it cannot determine, which are the technique and the sign. In the name of nature, some claim that a long chain of causalities will directly lead the "biological base" right back to pagan rituals and Shakespearean tragedies: one should therefore rely on the unalterable authority of "first comers" and consider subsequent innovations as final accessories. In the name of culture, others design a kind of cognitive revolution, repealing beastly ancestry. But

39 Here we introduce the methodological core of an approach exposed elsewhere [GUI 94, GUI 14].

these perspectives ultimately correspond to symmetrical denials of materialism, even if the first defends itself by placing all the blame on the second one. After all, knocking down the original in synonymy with the principal is equivalent to decreeing the superficiality of change. The same strain of mammals produced predators and their preys: let us bet that concerning wild animals and herbivores, the common past, even if memorized, might seem quite secondary. The eminent factor over 50 million years has no automatic precedence over the last millennium, or the last annual cycle: this chronic distortion will haunt future questions even more if interdisciplinarity voluntarily facilitates this obliteration.

If, following Lamarck, we define biology as the science which studies the common characters *as well as* the differences between living creatures, then the technique and the sign, arranged in an original way by the human race, would be no less "biological" than smallpox or stereoscopy. However, if *the* biological is discernible exclusively from living matter, the situation becomes more complex. Refined constructions which a great number of social insects or some birds elaborate, spontaneously close to "technique", are clearly inserted in this register. The same applies to the most spectacular forms of communication among these species. Apart from some quite specific exceptions identified by ornithologists, related to individual learning efficiency[40], these skills are highly involved in speciation processes. Let us take the argument even further: the use of clothing among men who live in harsh climates might not suffice to indicate independence from "pure" biological materiality. The techniques and the words of our species cause an unprecedented situation from the moment that they evolve, without human biology rewriting these changes. Leroi-Gourhan explained that "in our particular zoological group the ethnic unit replaces the species: human individuals differ ethnically as animals do in terms of their species." ([LER 93], pp. 232–233). The biological substrate loses the monopoly of memorization and the dissemination of acquired features. The relationship between nature/culture softly blossoms in this substitution, as stated clearly over half a century by a famous master, but nonetheless evaded by all schools: nowadays, it is still awaiting the honor of a frank discussion.

The term *memory* occupies the prominent position and presents the disadvantage of irresistibly associating the risk of confusion which rises with certain computing devices to a psychological phenomenon. In this case, the consistency aimed at through this designation constitutes a pending research

40 In the same ways as "dialects" in the cries of birds, quickly characterized as "cultural".

object which attempts to define an ability to accumulate information, store it and regularly restore it, so as to sustain or reproduce a phenomenon. In the world of the living, the genome checks this triple ability: it embodies a selection of what populations "store" during their respective evolutive processes, speciation then corresponding to a growing dissimilarity within an initially common memory, divergence eventually inducing the possibility of a split. Human societies have added two other complementary or competitive "memories", depending on the situation: they materialize in languages and techniques[41]. From this results the cohabitation of three modes of "use" conservation, with fields of application which do not exactly overlap.

Behind the avatars of technical systems and semiological structures, biology evidently does not crystallize: not only has the genetic heritage of Western societies felt the effects of the "industrial revolution", but "non-Western" or "exotic" societies remotely reacted to a plethora of repercussions. An example of this is when a colonial medicine resolutely decreased infant mortality by leaving the birth rate intact. Nevertheless, the modulations recorded by the biological substrates of these populations do not contribute, by themselves, to any memory which could foster the maintenance of a social phenomenon. Techno-economic systems and symbolic structures have autonomous organization, with their own alteration or resistance mechanisms. In those areas in which speciation would require ten to one hundred thousand years to ensure a change in lifestyle*, gesture and speech* accelerate development; a haste which is accompanied by a few jumps and contradictions, of course.

The dueling influences between the biological and the ideal, the ideal and the economic, or the economic and the biological openly fall into the absurd as soon as a third pole enters the competition, because tripartition makes each of these pairs invalid, as in a boxing ring, where the referee, tired of a contemplative role, would make his sentences known, by hitting the fighters. If biology rules over the ideal, it also has an influence on the economic, by adjusting the vice of life and that of ideas, but if it commands the economic, then ideas will in turn suffer an equivalent compression. Does economy rule over nature? At least, it rules over thoughts. The same goes for ideology, when we accuse it of ruining the biosphere because it subjects itself to

41 Strangely enough, Leroi-Gourhan, founder of cultural technology, devalued the role of technique in comparison to that of language on this level, probably trapped by the evocative force of the recovered notion of "memory".

economy, for the sake of profit. The winner is credited as such as long as it succeeds in amalgamating the vanquished.

In other animal species, the memory contained by each cell commands communication, on the one hand, and the manipulation of surrounding matter on the other. Biology triumphs over nothing: if learning prevails beyond instincts, it conditions talent. In human evolution, a phenomenon is growing: not only has technique become more refined, but they cover a larger field of activities, and a similar statement applies to the domain of discourse. The insects and the birds do not work at their communication, any more than they comment on the construction of a nest, whereas over the last hundred millennia prehistory has revealed to us a growing presence of technique in symbols (art, burial, etc.). The parallel with a widespread involvement of language in cooperation is still a terrain for conjecture, although contemporary facts infer such a conjunction, as evidenced nowadays by the term "technology" ("discourse on technique"). In short, the organic repertoire loses its monopoly due to the increasingly complex interactions that these two sectoral developments establish outside the genome registration chamber.

METHODOLOGICAL ASSERTION 1.20.– A scientific approach to human societies begins with three types of "raw matter": the organic, the technical and language. These departure points for observation, equally concrete, animate initially disconnected problems which converge towards the cross-fertilization of interactions between their three types of radical efficiency. Thus the relations between a human society and its historical and natural environment are potentially tripled, in that they are available for three incommensurable modes of assimilation.

Misinterpretation will invariably arise at the instigation of an evolutionism which continues to fascinate Western "common sense": over time, the interpenetration of these three influences would have stabilized such an important amount of connections that nobody would be able to distinguish them in the reality of current men. How can technique, welded to language through art, writing, printing, or even electronic communication, still maintain its position, or its "memory"? Our tripolarity might have, at the most, an archaeological value and could be useful for the analysis of an advent accomplished thousands of years ago. Philosophy loves to begin a thesis by solemnly proclaiming the end of existence, or the end of a

difference: if successful, the mourning announcement spreads among the human sciences as a deterrent argument, or as a force of inertia.

Since the denial that cannot come from Lascaux, nor Gutenberg's workshop, let us analyze once again the reprocessing plant studied by Zonabend. Medicine knows what to say regarding the compared ravages of irradiation and contamination. Physicists and engineers can address the matter on their own. And the workers at the plant have elaborated an unprecedented antinomy. So, at a place where the interdependencies of biology, technique and the sign have forged diverse and tight bonds, the three poles still deploy separate initiatives: intertwined within acquired facts, they continue to communicate despite all their differences. And Zonabend detected the wild dichotomy in this place regulated by technique, by shamelessly following the trail of the sign at the heart of complexity.

The scientific method demands constant attention to be paid to this triple mediation between men and their environment, in order to elucidate disharmony as well as the unequal resistance to change. The assumption that, in a prehistoric past, a fusional assemblage could have ensured their long-term synthesis in society represses the essential problem that the tripling generates: faced with temporal upheavals and unexpected traumas, the biological, the technical and the semiological do not react in the same way, and especially not according to the same rhythms. However, the examination of these distortions is imperative for sociology. *A contrario*, its absence offers philosophy a great chance to "groom" the question by theorizing a sustainable synthesis, even if, in the wake of Hobbes and Rousseau, this means entrusting its sponsorship to a contract drawn up by antediluvian communities among their members.

Finally, the restriction outlined in MA 1.14 has to be understood as a necessary and formidable constraint: the social sciences, if they want to work together with the life sciences, should impose the relations between humans/environment at the exterior of this triple dimension, as well as require the withdrawal of semiological reflexivity from their discussions. Once again, liberation from philosophical flyovers depends on it.

1.4. The line drawn on the side of science: frame of reference

Here we are at the foot of the hill: the challenge announced in MA 1.8 about a "demarcation criterion" might compete against the one exposed by

Popper, in that it would no longer be fixed from the outside by philosophy so as to pin down science, but by science from within, so as to detach itself from philosophy. We know that, *a priori,* it will not separate the ideal from the material, it will not infiltrate the prerequisite of a hierarchy and that it will have to grant the systematic possibility for symmetry between scientific arguments. It will therefore provide a means for identifying irremediable dissymmetries with the totality or a part of philosophical arguments. But above all, we know that it does not depend on a finding such as "falsifiability": science has materially existed for a long time, and the task here is not to invent an incredible discrimination, but to release a "greater common divisor" for its actual practice. As a consequence, we have permanently had it under the eyes from the beginning, and we have used it without extracting its significant implications, too primary to strike our attention.

1.4.1. *The observation of the facts and the strangeness of mathematics*

In its simplest image, science observes, compares and analyzes facts which are present in nature. And philosophy tends towards the metaphysical, "beyond nature", knowing that the absolute does not disclose itself to observation. But is this enough? A disciple of Aristotle could eventually reply that it also occurred to him to observe facts. On the other hand, mathematicians cannot be easily included among observers, unless we grind the notion of observation to such a point so as to classify them by force. And here we have to succinctly respond to such objections.

The inquiring philosopher leans on findings which he later extrapolates uncontrollably: by "pushing" metaphors as far as possible, or by subjectively pondering the relationships he scrutinizes. Meanwhile, the scientist practices the delicate art of conjecture, but by submitting his exploration to strict control. Such a crucial distinction requires further development in order to make it understandable. Nevertheless, for the moment, the second challenge destabilizes the stated position more sharply: does the *non-inquiring mathematician,* indeed look at natural facts when he inspects numbers? The answer has been in suspense for centuries. A psychiatrist was put in contact with twins with Down's syndrome who only came out of their torpor by exchanging prime numbers. He contributed to their bonding by offering them new cases gleaned from specialists, until the available resources were

exhausted: those of the arithmeticians, not those of the twins [SAC 88]. Neurology certainly has a word to say concerning the enigmatic existence of such mathematical objects immersed in highly disabled spirits, particularly, in the field of communication.

On another level, we should bear in mind that the science project outlined by the Greeks was based on logic and mathematics: the relationship between mathematics/philosophy followed a different path from the one followed by the relationship between science/philosophy[42]. Under these conditions, doubting the scientific status of mathematics does not imply any kind of degradation: it emerged as a *technique for reasoning*, by philosophy *and* science [GUI 91]. What is more, the topic has nowadays provoked a "modern" wave, with the appearance of misunderstandings between mathematicians and biologists, each side believing itself invested with an interpretation capacity, while the other provides technical assistance [ANI 17].

It is of little importance not being able to provide the bottom line of this story here. Let us content ourselves with pointing out that the application of mathematics necessarily carries an interdisciplinary content, in the sense that it comes across the observable reality through the intermediation of science, which divides this reality into territories. But every time, it refers to incommensurable interdisciplinarity with the one involving a dialogue between spaces characterized by an assemblage of certain materials: from the mineral to the human and from the molecular to populations. And regarding these systems and structures, we will see that mathematics involves a philosophical orientation whenever we aspire to produce models or methods for a wide range of sciences (by suggesting, in Popperian style, that resistance on this point would necessarily connote a lack of scientificity). However, this claim should remain the prerogative of logic, insofar as it initially took part in the project, or the "bet" of science. It so embodies philosophy committed to the defense of this undertaking, and nothing else.

The identification of science undoubtedly changes depending on the incorporation of mathematics (or not): can it *guarantee* science from the inside, or does it *dictate* "specific guidelines" from an upstream position? However, the answer given to this alternative will not influence expected

42 Mathematicians claimed full authority regarding the logic of the world of "wise men" in the middle of the 19th Century.

rationality or the intelligibility of its history. The dilemma inevitably points back to the puzzle of the "catalog of catalogs", the barber who shaves all the gentlemen who do not shave themselves, and, from time to time, to the paradox of the liar: majestic aporias which have incidentally attributed a "meta-human" reflexivity to formal logic and to set theory. The methodology and interdisciplinarity that we aspire to does not rise so high: we ignore whether mathematics manage "entities" or whether they "observe" what they talk about. However, our field of vision here only includes entities certified by observation: the latter gain a lot at being renewed, calibrated, and declined, or even "replicated"[43], so as to identify the targets more closely, and then to compare them. In contrast to an anthropology fomenting a fantastic getaway of its concepts out from the story, the achievement for mathematics would be to advance its concepts: these have no address or any deadline. The fantasy of a "sphere of ideas" draws its force from the image suggested by mathematics (and this without presupposing anything relevant in it).

However, the changeover begins exactly at this point. Becoming involved, even if a little, in the verdicts regarding the actual content of mathematical work would be to philosophize, despite the fact that they contribute to a prescriptive, binding, and exceptionally regulated type of knowledge[44]. In fact, they hold the "privilege" of being able to talk directly with philosophy through logistic, and therefore ahistorical, allegations and calculations about the infinite. On the one hand, they empower sciences confronting temporal facts, but on the other hand, they tickle metaphysics with conjectures hovering above mortals and their wanderings: the prerogative in this second domain creates a rift in the first one.

All sciences subservient to a part of nature (including society) experience and express a tension between logic and history. And this is never completely reduced to the above-mentioned presumptions, which encourage the promotion of formal idealism against materialism, by deducing symbols from lipids. In the 1980s, ecology undertook a broad comparison of plant formations in California and the Mediterranean (very close on a multitude of abiotic variables): every model clashed against the resistance of a core of

43 By the way, among the clues of a surprisingly brief curiosity of dominant epistemology, how can we avoid pointing out the absence of a systematic comparison between the respective merits of the reproduction of experiments and that of observations?

44 To go fast, scientists have, for their part, the right to reject any particular model due to the inadequacies of the suggested procedure, but not to reject a contribution *a priori.*

facts strongly limiting homology, and botanists resigned to recording these differences as a consequence of different stories[45]. On the human side, Robert Cresswell, a major authority on cultural technology, also insisted on this disjunction, far from the classical tensions of anthropology, regarding technical systems and their transformations [CRE 94].

It is important to dissociate this strictly scientific tension from the philosophical confrontation waving the banners of logic and history. Although the two, certainly, "play" on the internal and the external, the predictable and the unexpected, continuity and rupture, philosophy attempts to deliver general images and axes, while science sees a recurring question which requires a specific treatment on every occasion. An imperative of this is to ban the return of analog extensions on these fields of research "without negotiation". These extensions will encourage it to economize on a case-by-case analysis, by substituting assumptions and acknowledging the fact that ideology is treasured by all. But analyzing the case the other way around, does not danger indicate to us the minimum basis that the scientific viewpoint should defend and, therefore, its announced "changeover" threshold?

METHODOLOGICAL ASSERTION 1.21.– Mathematics offers analysis techniques to science, which have, under their exclusive responsibility, a technicality devoted to the treatment of systematically delimited facts, in time and in space. A scientific fact only exists within a specific setting in time and place. Similarly, a scientific category exists only from facts responding to this double condition.

When Schrödinger doubted the vitality of his cat, his thought experiment required that he knew the location of the box where the poor beast was enclosed, as well as its closing and opening hours. Still less than direct observation, conjecture does not allow leaving these points "out of control". Incidentally, when quantum mechanics admits to having trouble with jointly determining the position and the movement of a particle, physicists do not find the difficulty so innocuous as to disdain it[46].

45 Francesco di Castri, personal communication in 1985: his quick presentation, given as an example during an informal conversation, struck us, so unexpected it was at the time to hear an ecologist (he was Director of the *Man And Biosphere* Program at Unesco) summarize the problem in this way. Sadly, the surprise would barely decrease, nowadays.

46 Let us unequivocally agree that this comment particularly seeks to avoid a bad faith objection, fearsome in that physics is etymologically the primordial natural science.

The tension raised between logic and history always stages relationships conceived out of the real world, on the one hand, and facts positioned in this world, on the other hand. Unlike the philosopher's flyovers, the scientist faints when he succeeds in freeing himself from this burden. Taking a step further the allegation that "science observes, compares and analyzes the facts present in nature", we immediately place its reliability on the spot. The fragility of the formula is now reflected in the approximation of a parameter: "presence" is not enough and certainly does not close the door to softening procedures. Contrary to appearances, this observation does not quibble over a detail: every scientific fact inevitably becomes "historical" as soon as intransigence prevails on this double temporal and spatial tracking. A heavy emphasis on these dimensions then leads to a much sharper understanding of the scientific requirements underlying the exercise of comparison. Popperian epistemology may have an easy time praising the superior scientificity of experimentation when it implicitly contrasts it against a "soft" comparativism. When opposing facts come on stage with a complete "identity card", analysis treats them with significant guidelines: the comparison progresses slower, but more firmly. Beyond its communication with mathematics, which mobilizes a *sui generis* regulation, inter-disciplinarity immediately draws a lesson.

METHODOLOGICAL ASSERTION 1.22.– Sciences which meet through concrete objects, with a cooperation project, should primarily submit their exchanges to a common assessment mode concerning the examined objects. The need for this agreement both concerns the observation work and theoretical activity.

Two illustrative cases can be mentioned, far from mathematics: one for observation, and the other for theory. The revelation of "twin species" among mosquitoes is based on the failure of a conventional comparison with peripheral indicators: whereas morphology did not reveal any reliable variation, medical entomology, ecology and ethology accumulated suspicions regarding stable differences between populations[47]. Enzymatic analyses provided important confirmation and the coexistence of several distinct species under a single appearance became evident: the recomposition of interdisciplinarity resulted from the enrichment of the means for comparison. The second example, a theoretical one, has already been

47 Particularly regarding the *Anopheles maculipennis*, from which some genus conveyed malaria and others not, the question could not fail to permanently stand out.

mentioned: when McGrew asserted the existence of cultural variations in chimpanzees, he used a determination key previously stated in anthropology. However, the success of the operation depended on the quality of the envisioned criterion: the decisive fact had to be clearly identified by Lévi-Strauss for the revelation to triumph over any obstacle. The general refusal to renew the interdisciplinary debate, in this case, did not stem from a lack in scientific description: the *crucial disposition* which allowed for a shared conclusion was perfectly formulated.

These insights should be supplemented with a case in which mathematics is involved. In a synthesis on the theme of ecology/society, the geographer Yanni Gunnell, attentive to the dissensions between empiricism and abstract models, raised a very interesting problem, because it influenced a broad field: the relationship between the stability of an ecological system and its degree of internal biological diversity. The author thus characterized three moments.

First, the *classic conviction*, featured as appealing, in that biological diversity is "perceived as an insurance policy: various interactions between many species constitute a guarantee against the risk for a single species to dominate others. The result is greater stability and resilience. A subsidiary message accompanies this position, according to which modern agriculture, based on monocultures, is an invitation to ecological disaster" [GUN 09, pp. 123–124]. Then, a *calculated refutation*: "Unfortunately, common sense has sometimes nothing to do with the complex reality of biological systems. A new generation of mathematical models has shown that a supplement of biological diversity has caused more instability" [GUN 09, p. 124]. Gunnell quoted the example of a study on an oak forest. Finally, the *transformation of the discussion*:

> "After the 1970s, research on the relations between diversity and stability became less mathematical and more empirical. The debate therefore shifted towards the influence of biodiversity on the productivity of ecosystems... rather than on their stability. However, conceptual advances in the definition of what stability was helped to clarify some controversies." [GUN 09, p. 332][48]

48 Despite the remoteness of these pages, the passages correspond to a single reflection, condensed in the "concluding remarks" at the end of the book.

The geographer did not dissimulate his own position in favor of the new models, against a presumed conservative "common sense" yet responsible for this "empirical" knowledge refusing to give up. Since then, new questions have emerged, to which neither Gunnell nor us have been able to respond. The area of competence of the above-mentioned mathematical procedures raises immediate perplexity. Do they also effectively apply to natural ecosystems (the tropical forest as much as the savannas and the steppes) as well as to so-called "domestic" (agricultural) or "man-made" (oak forest) ecosystems? Because, in the second situation – highlighted by the exposition – regardless of the degree of biological diversity, the dominant species is known in advance, and it is essential not to impoverish it with mere calculations or adjacent deliberations. In the biocenosis that man does not regularly command, ecological "domination" makes no identifiable sense.

More awkwardness: this conjecture tacitly sacrifices the comparative dimension, between complex and simplified systems, on the one hand, and between increase and impoverishment, on the other hand. We witness how an addition in diversity increases instability, but would a decrease promote stability with the same success in a poorly varied systems as in a "rich" system? Ecologists who reflect upon the deterioration of biocenosis do not envision being confined by suppressions. When *Phytophthora infestans*, potato's mildew, arrived in Ireland in the 19th Century, it added an element to agrosystems, and subsequent instability caused terrible famine on this island. Naturalists "empirically" consider that agricultural specialization in a place multiplies the potential magnitude of a threat: can the models quoted by Gunnell extend their denial to this aspect of the problem, something which he himself incidentally evoked? A scrupulous comparison would confront the instability engendered by the additions in diversified and specialized systems. Thus, the allegedly thwarted "common sense" states that a random addition, or an accidental disappearance, tends to cause less damage in a complex ecosystem: this intuition is still valid.

According to the geographer, the debate continues: one way to stimulate its progress would be to exactly establish the distance between the ambitions displayed by the new models and the area in which they are really effective. Thus several abstractions arise concerning the relationship between biological diversity and stability: balance, disruption, productivity and even resilience. Nevertheless, the interdependence between these "heavy" notions

considerably varies depending on the problems and the theories[49]. In addition, they all incorporate a temptation of analogous extension over the distant, which could tease ideological premonitions. We therefore fluctuate between the warnings issued by MA 1.21 and MA 1.22: the fact that mathematical recipes lead to counterintuitive epilogues might "initially" suggest a lack of suspicious intention, but their subsequent use worries due to its recklessness regarding multiple contexts meant to delimit the scope of conclusions. The spontaneous overvaluation of mathematical inventions and the trust *a priori* placed on its findings seriously hampers a great deal of research: not to review the concordance of mobilized skills with the targets set reveals the responsibility default which should not be attributed to mathematicians.

1.4.2. *The permanent priority of the frame of reference*

Assigning a place and duration to facts implies the recognition of *limits*: they can be analyzed within certain boundaries, on a circumscribed area, with a beginning and an ending. And every scientific reasoning aspires to be valuable only within the context of these restrictions, within this temporary perimeter which we will call the *frame of reference* from now on, and which follows a common expression reflecting a kind of consciousness not less usual among researchers needing to identify their demonstration. Because it is not an invention on our part, but a methodological rule assimilated by everyone: it only suffers from the weakness of an insufficient explanation, at least regarding the recognition of the constraints which come with it. Nobody ignores that science strongly refuses to transplant results from one context to another without taking the necessary precautions, but, strangely, the census of these is usually done on an ad hoc basis, within an empiricism that each discipline manages by itself. Every science develops its frame of reference of its own accord, this conversation topic usually belongs to a kind of "internal kitchen". No one would think of complaining: everyone operates according to their method.

Except that serenity necessarily alters as soon as an interdisciplinarity project emerges: how is it possible to respect the methodological intimacy of another discipline, in the company of which we strive to solve actual problems? Furthermore, how to protect one's own methodology? The length and the structure of this chapter, probably confusing in relation to the usefulness wish

49 See Roy Ellen [ELL 82], only to quote a classic.

expressed in the introduction, entirely stems from this stifling contradiction: if the different types of knowledge which take part in an interdisciplinary project do not give up their full independence in the elaboration of their problems, the construction of common frames of reference will escape scientific responsibility, and, in one way or another, the assemblage will result in philosophical intuitions. The failure of the major discussions on the theme "ecosystems *versus* social systems" is derived from this pitfall: rather than going back to common methodological grounds and then gradually stretching out, tired of confrontation, participants prefer to climb onto the layer of a *meta-science.*

Philosophy does not accept the yoke of frames of reference: it freely goes from the singular to the general, from minuscule to infinite, expands or reduces its ideas, and makes them travel from one promising context to another. Its metaphors are repeated and its metonymies extend endlessly[50], at the discretion of analogies which set free from the severely regulated exercise of comparison. This mode of inspiration may come across disagreement, but not refutation. The submission of a proposal within a frame of reference simultaneously represents the privilege of science, its primary obligation and the seal of its identity. Let us be brief and take a look at the philosopher Spencer's famous example of the "survival of the fittest" in the face of its so-called synonym, Darwin's "natural selection". The first symbolizes evolutionary ideology and is applied to everything: the organism, the psyche, society, and even galaxies. And this, for a good reason: free from any context, its ends up in a tautology, the fittest can be recognized for its survival, while survival signals aptitude. However, according to Darwinists, natural selection will retain a type of adaptation A for a species B in an environment C, while it will develop another strategy if the species B is confronted with an environment D, arranged differently. For Spencer, the survival of the fittest is a tendency which occurs spontaneously, everywhere and all the time. For Darwin, natural selection expresses a synthesis which depends on the provisions of the species at a precise place and moment: the constant lies in the existence of a synthesis, not in such or such particular process, because it can go so far as for humans to renounce their most common tool, "the elimination of the weakest" [DAR 71]. This conclusion drawn from *The Descent of Man* was totally obliterated for more than a century, before its unexpected exhumation [TOR 83].

50 See the complete analysis by Tort regarding the relationship metaphor/metonymy in scientific classifications, with the extensions that drive them "off limits" [TOR 89].

This textbook case incidentally reveals that the frame of reference cannot be reduced to mandatory, but insufficient parameters, concerning the territory and the duration which surround a fact: interdisciplinarity would feel no embarrassment. It equally entices us not to confuse this concept with the "body of knowledge" that the hard sciences accommodate at will, depending on more or less prescriptive or approximate meanings (one criterion not excluding the other). A frame of reference can only be of value if it *inseparably associates a fact with a question related to it.* The fact in itself does not require a special action for elaborating the "framework".

A strange automatism, inherited from the 19th Century, has led us all to spontaneously assume an elementary reality when we consider the notion of event, or the notion of fact. We could rightly consider them in the form of atoms: in its past sense, since those from physicists have lost their supposed inseparable character. The credibility which was abandoned by the Greek atom remains intact in its Latin translation: the individual. Through common sense, our education still encourages us to start from punctual events and simple facts, and, from there, to extend the deductions to assemblages, and to extend the laws of the particular to conglomerates. Breaking this mental barrier, no doubt a highly philosophical one, becomes an urgent matter, because it remains one of the worst obstacles on the path to interdisciplinarity. It is precisely because nothing grants the definite simplicity of a fact that science should relate it to a mode of observation, to a questioning, within a materially defined framework. Many biologists and sociologists have dreamed of the absolute purity of a basic real, involving initial causes, but not inspired by the same reasons. The long neglected need for uninterrupted monitoring of frames of reference necessarily pierces through the commitment towards interdisciplinarity. And the concern about philosophical shortcuts immediately emerges.

During the *Man the Hunter* symposium, which shook the vision of the hunter-gatherer societies [LEE 68], a participant pointed out that it did not matter that the "primitive" lived in alimentary opulence for 10 months of the year if they suffered from famine during the rest of the time. Anthropologists retained the argument because, as a consequence, they regularly studied "off-seasons" or seasonal phases during which the group had to overcome a temporary but alarming lack of resources. From there, research focused on the dangers of bad years. Finally, archaeologists pondered the consequences of a series of disastrous years [HAL 89]. This sequence shows a temporary extension of the concomitant fact of a gradation of variables acquired by the

problem. Accidental lack, off-seasons repeated every year, and an alteration of means of subsistence over a decade had an impact on the social implying qualitative and not only quantitative differences. For information to flow between these three facts, it is important for each of them to be linked to "its" own framework.

Conversely, the framework gives access to consistency and to the content behind the fact, since it fixes the oscillation between the observed and the hypothetical. This is because scientific research carries the attested fact to various expanded frameworks, in order to check which of them "agrees" to acknowledge it. Let us go back to the mind-blowing calculation of the Papuan village of 60 inhabitants who received 35% of their protein needs from the fact of eating one man per week. Cannibalism in New Guinea brings together a substantial body of facts recorded in various places at different times, in which a society has vindicated the consumption of other human beings. Shocking or not, the question about the nutritional value of such absorption is admissible. Ironically, the first ones to have approached the topic up front wanted to spread its insignificance in *American Anthropologist*: according to them, at a rate of one prey per week, a community of 60 people could only have acquired 15% of the desirable volume of proteins [GAR 70]. But a reply arose in *Human Ecology*, challenging such an assessment: the group considered might have actually obtained between 35% and 40% of the desirable amount [DOR 74]. And the debate continued within the context of the strictly nourishing aspect, far below the basic reflections of ethnography, according to which this biological variable might come upon rather embarrassing parameters. Cannibalism covers a wide range of biological, psychological and social aspects, involving more or less large perimeters. Let us be polite and momentarily admit to an alimentary rationality at the scale of the village, along a year: strictly speaking, an explanation of anthropophagy in a Melanesian horticulturist population could only follow if, once set against socio-ecological frames of reference, the motivation confirmed its effectiveness when confronted with other influences. No need to leave the life sciences to stumble upon a crippling denial: within a few years, the nutritional factor led to a cataclysmic phenomenon affecting population dynamics, a disaster which only the generalization of (pacific) funeral endocannibalism and the suicidal denial of surrounding villages to adopt this "adaptive strategy" could *slightly* slow down.

We rarely come across such spectacular discrepancies, and here we can profit from the magnifying glass effect generated by an ideologically heavy topic. Nevertheless, more subtle skids often merely limit themselves to better conceal similar exchanges during the transitions from one frame of reference to another: the trick is always to *tacitly* paralyze the formulation of a question – as well as its answer – far from initially acquired relevance. The expedient works all the better as soon as we let this initial framework in the blur, by betting on the complicity of "common sense".

METHODOLOGICAL ASSERTION 1.23.– A scientific question should be related to a fact which can clearly be delimited in space and in time, or to an association of facts which can be conceived according to a common delimitation. The frame of reference outlines the physical contours surrounding the relationship between the question and the analyzed fact. However, its edges are not static: the question and the fact mutually define each other during research, while the adjustment of the problem's framework progresses, as a result.

METHODOLOGICAL ASSERTION 1.24.– When a question reaches a reliable result within an explicit frame of reference, the transport of this result to another frame, or the change of scale as regards its application (expansion, or reduction), require a joint transport of the question. As soon as the number, the quality and intensity of variables are exposed to changes, the double alteration of the questioned fact and the question concerning the fact become the object of a priority inquiry. It is actually the permanent respect for these constraints which characterizes the scientific method and objectively differentiates it from any other mode of thinking.

Considered as a part of the daily work of a discipline, this may probably sound like a platitude, since methodological empiricism strives to grant the *technical* control of such precautions. Instability arises when the exploratory dimension of research goes beyond the previously defined limits: conjectures and theories appear. Scientific activity is "different" in its resolve to build frameworks, not in virtue of the protection that these offer it, but by the essence of the question preceding the existence of the *grasped*[51] fact.

51 Depending on whether it is "grasped" or not, the statement avoids relativism, or not. The "materialistic bet" assumes that reality precedes the question, but facts stand out from the interrogations that model them. They are not classified by themselves.

The attraction of "serendipity", including the technocratic vogue we mentioned in the introduction, quickly subsides in this field: its success actually depends on an ability to record the surprise factor within a frame of reference, something which a lot of comments on the topic often underestimate. The unexpected does not merge with the unhoped-for, and "exemplary" accidents do not quite happen randomly, when we closely pay attention to their context: enthusiasm would be more justified if physicist Henri Becquerel had incidentally found penicillin, or if biologist Alexander Fleming had unexpectedly stumbled upon radioactivity. These kinds of serendipity emerge in contexts and favorable moments in which unanimous convictions help glimpse some breaches.

1.4.3. *Scientific clarity and the impurity of scientists*

It is necessary to open a parenthesis on some deviations it would be dangerous to completely ignore, even if they are here secondary in the short term. The sociologist looks at science via hesitant scientists, while epistemologists study the habitual ways of scientists by using a polarized compass, relying on the ultimate state of science. While one forgets that projects become refined throughout the centuries, the other loses interest in the contradictions which stimulate the servants of such an aspiration. But making scientific activity noticeable does not mean simplifying recognition, or the position of the actors of science. And the other way around. In this respect, a reflection which gives precedence to the enigma of the hiatus with philosophy finds refuge in a methodology in which, whether they want it or not, the impurities of scientists cleanse science by friction: we thus come across serendipity once again, which fights for a place among these historical frictions more than it informs epistemology *stricto sensu*.

It is possible to place the technical, scientific and philosophical approaches in perspective, but not to distribute them among individuals or groups, who would serve one of them without ever listening to the other two [GUI 91]. It is then important to admit the existence of three coexisting instances, but which need to avoid imbroglios: the cleavages of the method, the combinations of practice, and institutional dissociations.

The rifts of method necessarily monopolize our will to shed light on the sine qua non conditions of a scientific stereoscopy beyond the metaphysical ferule. Summing up, technique repeats and refines operations within the fixed

frames of reference. Science works on the frames of reference until they reach the posited question, after which technique becomes responsible for them. And philosophy evolves beyond frames of reference. This tripartition which can be deciphered by synchronic observation does not stem from a logical trend, however: it is formed as a result of a complex story. Societies as centralized as ancient Egypt or imperial China accumulated considerable rational knowledge by contenting themselves with the duality between an engineer and an organizing authority (priest or mandarin). Western science took a decisive advantage only in the 16th Century, when it restored a major role to the critical dimension of science foreshadowed by ancient Greece, but especially by regularly confronting it with the universe of technique, about which Greek thinkers worried little. The technique/ideology/science triptych does not represent "natural" or tendential progress: it emerges in nations which necessarily resign themselves to the lasting cohabitation of antagonistic cosmogonies, which in turn allows for long-term controversies within rational analysis [GUI 17]. The prerogatives of the "materialistic bet" are involved once again within a frame of reference, and the points for an impartial method which we are trying to build as important stages, shared by different sciences, do not claim "going beyond" history.

The combinations of practice can be explained by the fact that no individual, even if professionally labeled as "technician", "scientist" or "philosopher", can infinitely cogitate by following one of the three components, while ignoring the other two. A science engenders, or recovers, techniques which, when sharpened, sooner or later collide with the anomalies buried in a support which might have believed to be leached of all mystery. As for the uncompromising commanders who proudly display themselves as "formidable rationalists", arguing that their knowledge stops at deductions and does not tolerate speculation, it is them who a few minutes later, reveal themselves as naive philosophers. Let us recall the "Sokal case", which began by a scientifically instructive, salutary hoax: a text which skillfully "mocked" the delusional uses of the "hard sciences" by the postmodern trend, was published without a hitch in Social Text, a highly popular journal in 1996. Alas, the physicist who had produced this magnificent kick, Alan Sokal, soon flanked by his colleague Jean Bricmont, proved unable to analyze the event created, and had to call sociologists and epistemologists to the rescue, despite the fact that some of them experienced malevolent pleasure in helping the offenders. An essay was the result, *Fashionable Nonsense: Postmodern Intellectuals' Abuse of Science* [SOK 99], in which the physicists bogged down in advantageous poses, mumbling a crude and deficient philosophy: from that followed a worrying symmetry of charlatanry [GUI 98c]

aggressively opposing the falsely innovative postmodernists to authentically backward latecomers. At the price, once again, of the abandonment of the multidisciplinary debate which is still required in the present.

A joke has been circulating recently, among the students of preparatory classes to the *Grandes Écoles*, higher university establishments in France. Confronted with the law stating that, beyond 1, all odd numbers are prime numbers, the fable elicited the following reactions:

– *the mathematician:* "3, 5 and 7 are prime numbers, but not 9. So the law is false";

– *the physicist:* "3, 5 and 7 are primes. 9 is not, but since 11 and 13 are, it is probably an experimental error. Basically, it works";

– *the biologist:* "3, 5, 7, 9, 11 and 13 are primes. The law is valid";

– *the computer scientist:* "3 is a prime, 5 is a prime, 7 is a prime, 9 is not a prime, 9 is not a prime, 9 is not prime, 9 is not prime...".

This humorous feature (about which we can easily guess the disciplinary source) secondarily endorses a truthful statement under the caricature: as the fantasized physicist would say, "basically, it works". It is then an embryo of the philosophy of science, based on hierarchy, which the students are assimilating and will imprint lasting thought reflexes in them[52]. The fact remains that a scientific questioning cannot claim to be untouched by any ideological presumption concerning the arrangement of the world: even when it intends to prove the error of a thesis by the absurd and must ultimately capitulate to its validity[53], the "materialistic bet" tests an exogenous idea.

Finally, institutional dissociations fully belong to sociology and history. Steven Shapin has shown the tension between "invisible" technicians and scientists during the 17th Century, long before the formalization of attached categories [SHA 89]. Nevertheless, the moment when the rebellion against

52 By the way, compiling a corpus of jokes on various nations regarding this sort of humor promises a beautiful ethnographic thesis, in which semiological comparison could be related to contexts and traditions. The sociologist was not invited in the fable: we expect his answer to have insisted on the fact that the prime number is still a belief.

53 Some have shamelessly recorded these "shocking" findings among the cases of serendipity, which would be equivalent to compiling all the denials from nature to our prejudices, in this category.

this deletion took place corresponds to a rise of technical power in the face of science considered as too meticulous, an ascension which can be evidenced by the sharp spread of the term "technoscience". The relative positions of the technician and the scientist vary widely between disciplines, with phases of mutation and internal turmoil [ANI 17]. Moreover, society issues either severe or praising opinions about one and other, which fluctuate depending on the circumstances (findings, alarms, accidents, etc.). The growing opportunism of "electronic" or "post-Gutenberg" philosophy, diligently amplifies and formalizes these confidence or enthusiasm waves.

Let us shorten the discussion, the essential trait having emerged by itself. Regarding the three proposed "plans", complex relationships can be established between art, science and philosophy, at the same time interconnected and immeasurable, due to an obvious fact: each area summons its own frames of reference, impervious to each other. Thus, the space of the method is not insensitive to changes in point of view in the mind of the researcher, but cannot deduce any profit before having translated the fact to its field of research. Our pause, motivated by the concern to avoid the all too frequent confusion between these three types of inspection, curiously leads to the consecration of a double major function of *frames of reference*: disqualifying the comparisons which are based on intuitive or analogous oscillations; and guiding those who are progressing step by step.

At risk of insisting heavily, it is important for the reader to admit that the demarcation criterion of the frame of reference in no way represents a theoretical gadget, but a device traditionally employed by all sciences, and which nowadays only asks to be stipulated as such. This automation, subconsciously implemented by each discipline on its own behalf, cannot, indeed, stay silent when these engage in sustainable cooperation aiming at common research of important magnitude. Without the explicit recognition of its generality at the core of the scientific world, interdisciplinarity is condemned to specific undertakings, which may have little future.

METHODOLOGICAL ASSERTION 1.25.– The planning of long-term ambitious interdisciplinary projects mainly depends on the coordination in the design of common frames of reference: they should be likely to receive useful information for every participant in order to favor assemblage. The scientific areas never learned to "manufacture" together these receptacles for the questioning, despite the fact that this know-how conditions the mutual understanding of disciplines and, consequently, their cooperation.

Thus summarized, the constraint seems, however, almost trivial. The impression dissipates with the reminder of the necessary dynamics for the use of these "containers". When a biologist considers adaptive advantage, he quickly projects his revelation comprising an annual cycle to a calculation regarding its diffusion in the genome of a population covering a million years. This hardly raises any objection if the question is to retrace the evolution which actually took place beyond past accidents, but it will be quite different for a prospective conjecture calculating their exceedence. Other sciences will have to assert the importance of the variables which they observe on their side, during much smaller time periods. We expect that the frames of reference will cooperate more with the interdisciplinary dialogue, by first recapitulating the past, and then looking on to future scenarios. However, applied research will quickly require that the knowledge retrospectively acquired be transferred to future estimates. From a broader point of view, the totality of the research regarding socio-ecological interactions meets a formidable wall, since the justification for a nature/culture relationship essentially stems from a systematic discrepancy between the transformation rhythms of one and the other.

METHODOLOGICAL ASSERTION 1.26. The decisive obstacle that will be faced by the management of several areas for a common frame of reference lies in a preservation of their agreement during the progress of the questioning and, therefore, throughout the modification of the framework itself: a methodology for interdisciplinarity should focus on this neuralgic point.

For the sake of awareness and in order to avoid any suspicion of a misunderstanding, this operational and progressive *relativity* is in no way related to any kind of *relativism*.

1.5. "Reframed" comparison

The comparison is not placed, as many may imagine, on an axis going from the "soft" analog to the "hard" experimental. It opposes these two practices on different levels:

– metaphors and metonymies augur similarities, or dualities, which unfold to infinity. The scientific comparison begins to acquire its characteristics by stopping everything from the moment that the analogy

stumbles upon a denial[54]. According to philosophers, the exception can be compared to an unfortunate incident, about which we say that it confirms the rule only to look good. Gaston Bachelard said in a soft tone: "philosophy has a science that is peculiar to itself, the science of generality" [BAC 02, p. 64][55]. For scientists, the anomaly is providential since it offers possibilities for controlling the limits of competence surrounding the stated law: the exception, in sum, reveals a question based on a difference, which allows the development of frames of reference to enable a comparison. We never compare the frames themselves, but the manifestations of a parameter, or a relationship between variables, through a suite of frameworks. An external view would tend to deplore the needy cautions of comparative analysis against the bright audacities of analogy.

– an experiment produces simplified frames of reference, in view of accepting change only due to *one* factor. It thus facilitates specific demonstrations concerning basic phenomena which will then have to be inserted into an unpurified reality. The haunting lies in the suspicion of "hidden variables". The sciences of observation use the results of these *manufactured frames*, nonetheless admitting the predominant place of *natural* frames "in the last instance". In the wide range of life sciences, the rivalry between theories stemming from experimentation, and those initiated by observation necessarily depends on the social context (through an admiration of technique, for example) and not on a "noble" epistemological root.

Preferences and hierarchies which are often attached to these disjunctions do everything possible to misrepresent their content. First, a scientist determined to "kill the philosopher" inside of him, by banning the couple metaphor/metonymy, would globally prohibit the recourse to hypothesis, which brings us back to "combinations of practice" as described above. Secondly, the widely proclaimed superiority of the experimental protects itself from epistemologists by a pretty staggering intellectual hodgepodge, with illusory contrasts masking the main complementarities. First, we have

54 The first sociology thesis defended in France consecrated an admirer of Spencer, Alfred Espinas [ESP 78]. Almost a similar deduction: while Spencer's analogy flew over the pitfalls, Espinas stopped in the face of adversity. This is why Gabriel Tarde and Émile Durkheim always spared him in their condemnation of the dominant "social Darwinism".

55 Let us concede that rustic brevity here leads us to behave as a philosopher would: our generalization quickly sweeps a few exceptions. Was Bachelard not a bit of a philosopher himself?

to see that frames declared as "natural" do not conceive themselves as any less *artificial* than the others: the qualification relates to the fact that the work selecting the studied criteria is carried out in direct connection to perceptible nature, and not by means of a schematic reconstitution, that is to say, an *image*. Natural frames certainly benefit from the information offered by manufactured frames, but do not the latter depend on "on-spot" corroborations by observation? Sooner or later, the legitimacy of the "reduced model" does not require the assistance of any prefabricated support: neither higher nor lower, this completes the analysis by own means. Technique, which boasts experimentation and denigrates the theoretical whirlpool in which observation bathes, likes to forget that experiences are channeled through theoretical options, upstream of falsifiability.

In 1978, in an entomology class, two teachers reported recent work in which biologists had collected packages of cells from the larvae of insects known as "imaginal"[56], responsible for producing the head, the legs, or the wings of the adult, after metamorphosis. They were transplanted into other larvae of the same species, and then, again removed before the decisive transformation, and introduced again in similar organizations, this operation being repeated many times: 200, 300, 400 "transfers" or more. After this the researchers let the metamorphosis unfold, in order to see what became of these cells in transit on the unfortunate individual responsible for allowing this supplement to express itself. It appeared that these "packages" long retained their original destination, but that beyond a certain amount of transfers, they developed legs instead of wings, etc. And, if we doubled or even tripled the number of transfers, those who "yielded" Y organs, instead of X, resulted in Z organs. It was thus possible to determine different pathways following stages of alteration, from each initial vocation. Recently disembarked from the social sciences and stunned by the story, we asked the teachers (who were in their turn disconcerted) whether the authors of this protocol had thought to take up psychoanalysis! Experienced zoologists felt a strong legitimate admiration for the revolutionary outcome of this study, admiration which somehow shadowed the consideration for the eccentric inspiration of these operations. On our side, the neophyte first felt dumbfounded by the sophistication of the program and the sequence of the verified *expectations* one after the other: this experiment seemed prepared to welcome all the obtained surprises, from the first to the last. Who would have thought to criticize the scientists had they

56 Imago designates the terminal stage of the accomplished individual.

given up on pursuing the examination after 200 transfers, while the imaginal cells still held their course?

METHODOLOGICAL ASSERTION 1.27.– Comparisons are observation algorithms. There are different categories, some fitted out for experimental investigations, others for projects concerning open environments. All of them carry a theoretical burden in accordance with the chosen approach, which affects the recognition of a relevant framework.

The allusion to the algorithm, the fruit of mathematics, conveys no analogy. These procedures extend to all logic[57], and from there, to the complete scientific series. In systematics, for example, a determination key leading to the identification of species in a zoological family matches this exercise. Above all, the connotation emphasizes the absurdity of a general staging of the phases, which would transcend the relationship between tools and targets. Once again, science becomes concrete with its eyes fixed on a phenomenon to be reached, and not by the means for approaching it. Otherwise, its deprives its work from the dimension of responsibility to be assumed in front of the society which sponsors its activities, in order to focus on the quest for intellectual exploits. Does not the "hardness" of a discipline first measure the temptation to work exclusively for the glory of the human mind?

Let us go back for a moment to Zonabend's investigation of "her" atomic power station. The salient fact – contrary perceptions between irradiation and contamination – invites a comparison. Does this interpretation only emerge at a plant specialized in waste reprocessing, or is it also applicable to the sites devoted to energy production? Are there emerging parallel visions in Russian, Anglo-Saxon and Japanese factories? From the technical point of view, as soon as the phenomenon is found in one place, the radiation (if we dare say) of confrontations regarding a range of similar situations which might differ from the source on an essential variable would not raise dissuasive difficulties: an inquiry in half a dozen places would lead to net results, and we would know if the track deserves to be followed, or whether

57 At the dawn of the 20th Century, Louis Couturat pleaded as well for an "algorithmic logic", seeking to build a universal, scientific, or philosophical language [CMH 10]. Incidentally, the term algorithm, derived from the name of the Persian mathematician al-Khwarizmi, does not conceal any "directive" in its etymology.

the essential information has been acquired. Without a doubt, the practical and theoretical reasons for acquiring such information are abundant.

Above, the entomologists had obtained the technical and human means to ensure the larval cell trajectory for months or years, so as to evaluate the fidelity of their determinations: an expensive comparison between successive stages. The institutional recognition of the stakes was not evident, not more than the receipt of essential subsidies. Here, a disturbing question about the "misadjusted" representations expressed by workers in a plant dedicated to nuclear energy: the suggested comparison would not mobilize fortunes, and would progressively move on. It only stumbles on indifference and slumbers in limbo.

An absurd approximation? Not quite. *Every science, experimental or not, endures the obligation of having to choose, from the resources allocated to it, among comparisons to be made in the short, medium and long term, and this, among an infinite number of possibilities.* However, the management of this project concerns the political bodies of the discipline and the surrounding society, leaving methodology aside, since by itself, it cannot justify any preference: prospection goes out from the frame of reference, and the fight shifts to assumptions. From this perspective, epistemology stemming from a "demarcation criterion" which describes the experience by suffocating comparison, and which advocates for the superiority of a group of sciences, is it not encouraging a kind of ideological *lobbying*? Between the algorithms of the laboratory and those of the "natural environment", there is no continuity: observations are reproducible, and others reunite all the parameters of a preconceived experience, and statistical treatment are applied to multiple media. On the other hand, a gap has widened between the planning of comparisons in manufactured worlds and in the natural world, with a growing paradox: despite the fact that the former constitute reduced models, the anticipation of comparisons to be carried out substantially increases, while in the immensity of the second, the dissemination of inspirations rules, even the improvisation of attempts. It took a far-sighted and orderly community to move packets of cells of larvae (on larvae), to a laboratory for many months. Opposite to this, the results reached on the nuclear industrial site will only trigger a differential inquiry if a worry of some magnitude eventually strikes the attention of academic institutions.

Since the end of the 20th Century, Western societies have been asking ecology and the social sciences to work together to illuminate the interactions

between societies and environments, so as to rationalize rebalancing, conservation, transition and improvement programs. But the efforts of researchers have dispersed in all directions and split up to infinity, as a mosaic ruled by the benevolence of chance. Axes, syntheses, centralization and gatherings no longer make ecology or sociology dream. Too many disappointments, or disillusionment, undermine the time of "post"? Even so, the fact remains that this voluntary collapse leads to the continuous, even exponential increase, of the urgency of the task.

To our knowledge, no authority currently ponders this yet trivial remark, according to which a wide-scoped interdisciplinarity requests progressive guidelines and directions. We suffer from a serious deficiency, maintained for too long by the ideological obsessions of classical epistemology on absolute truth, to the detriment of progressive truths: the sciences of observation are thus condemned to identifying their algorithms by themselves, because interdisciplinarity essentially involves coordinating methods so that several domains can produce and refine comparisons together. Because so-called "fundamental" research above all shuns its responsibilities when it opposes a force of inertia to the resolution of a problem on which depends the future success of applied research. Indifference is sometimes as fierce as enmity: it is enough for it to peacefully camp on a dominant position.