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INTERDISCIPLINARITY BETWEEN BIOLOGICAL SCIENCES AND SOCIAL SCIENCES SET

Volume 2

Technicity vs Scientificity

Complementarities and Rivalries

Giulia Anichini
Flavia Carraro, Philippe Geslin
and Georges Guille-Escuret

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Technicity vs Scientificity

In memory of Robert Cresswell,
Great researcher and great professor,
Who defended cultural technology
Against winds, trends, and tides.

Interdisciplinarity between Biological Sciences and Social Sciences: Methodology and Theoretical Pitfalls Set

coordinated by
Georges Guille-Escuret

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Introduction

The relationship between technicity and scientificity is usually called to mind without stirring up any problems: the speakers need only refer tacitly to "common sense" for these terms, as a function of the given context, to be seen as clear opposites. They then offer a glimpse into two neighboring, but disjointed sectors. The illusion, however, dissipates like a mirage as soon as it becomes necessary to specify the two positions at hand, for the dialogue gets bogged down in many different ruts, e.g. if one reason is mentioned encouraging thought of the link between the techniques that operate outside science (or before it) and those that are created under its control, or if the need arises to specify the distances between technical and scientific *knowledge*, on the one hand, and between technical and scientific *efficiency*, on the other hand. At the end of the day, they will stumble upon the relationship between technical *innovation* and scientific *discovery*.

Intellectually speaking, these queries seem rather elementary, but in practice, they immediately plunge us into terrifying complications that we tend to diligently skirt around: sociologists and epistemologists included, unfortunately. Few texts deal with both techniques and science without confusing them, and most focus on a brief period of time. In general, though, our sights expand in order to delimit the breadth of phenomena considered, and the rarity of reflections becomes more surprising pertaining to *what techniques do in the sciences and what the sciences do in techniques*.

The authors of this volume will have the chance, a bit later, to ask themselves why. By waiting, they will grant first priority to the substance of

the problem itself – the tensions and ambiguities that trouble the relationship between techniques and science – only to avoid participating in this dubious eschewal; so at this stage, let us be content with a pithy statement: the strange thing about our times is that the technical dimension of knowledge has never been as highly valued, as authoritarian, nor as feared by society (including the scientific community); yet at the same time, it has never been as strongly repressed outside all human explanations, with their transformations, cultures and history. As if there were a repulsiveness growing there as a function of its power, and the fact that man's specificity should be "exonerated" from this, even though it returns to nestle in Mother Nature's ample bosom.

I.1. An indicative anecdote: biometrics versus statistics

An anecdotal illustration will allow us to briefly open up a common source of perplexity beyond the disparity of approaches to come. More than 20 years ago, a disagreement led to malaise in certain circles at French universities: mathematicians, having perfected a new method of analysis, hoped to evaluate its viability, so they asked a biology laboratory for a corpus of data, and the laboratory, all too happy to seize the moment, sent them a set of biometric measurements. Some time later, the scientists were astonished to learn that the mathematicians had published the results of operations performed on the material that had been provided to them. The embarrassment increased due to the fact that the heavy reproaches addressed to the thoughtless actors surprised them: they did not see where they had committed an offense.

We must admit, at least as a precaution, that a detailed investigation would show that the real situation was "a bit more complicated than that" and that hearsay slightly dressed it up as a "textbook case". This would not matter much, as it happens: what holds our attention comes precisely from the commentary poured out in the vicinity, outside the area of concrete risks. The popular opinion diagnosed a misunderstanding: the mathematicians were wrong to consider the biologists as their *technicians*, without taking the time to be sure that they agreed to this. Their disciplinary narcissism then *reversed the roles*.

Except that there was an advanced application of fixed procedures on both sides (which implies, *a priori*, a "technical" activity) at the same time

as research in the process of exploring (an activity presumed to belong to the "scientific" jurisdiction): some had constructed an analysis mechanism with new capacities, while the others had set a range of progressively selected measurements in motion with the aim of evaluating the pertinence of a hypothesis. Each of the two teams definitely had something new to investigate under *their* responsibility concerning the list of names that was thus examined.

If the biologists had initially published their conclusions and the mathematicians had subsequently added an analysis of the performances offered by their "tool", no discontent would have broken out and no final offer would have been "tainted". By assuming the priority, however, the modelers monopolized a part of the interpretation dependent on the specialists: in short, a zoologist reading the results of the test would have had no problem, because of his own education, deducing the "biological lesson" of the work. What is worse, with minimal information regarding the reason the researchers took the measurements, the mathematicians could have easily delivered the global issue of research in an explicit manner, without the abuse drawing attention, for all in all, this was logical. As such, the competence before the data processing evaporated, and along with it, the vacillation over the choices that would decide the selection of promising measures, with long-discussed corrections and intuitive additions. The corpus thus showed itself to be an asset and no longer a product.

This misfortune assumed a particularly troubling character because on both sides, it confronts technicians who are also researchers, each "field" clearly seeing the technicians "opposite them", but not the researchers: biometrics does not intrinsically constitute a science (the systematic nature of living beings, or more exactly, its zoological "branch"). A discipline thus uses procedures it set by itself for itself, parallel to others taken from more or less distant domains that reserve the liability for their use (in the case of taxonomy, these loans go from chemistry to mathematics, passing through molecular biology).

Nevertheless, by going around this isolated contradiction, it would be easy to point out a multitude of situations revealing disparate and often

irreconcilable disjunctions between the world of techniques and that of science, or, in a way that cannot be deduced from the previous, between the world of technicians and that of scientists, for the way in which reason perceives the borders between types of knowledge does not dictate the way in which society creates professional categories, which stops neither reason nor society from going through unconfessed oscillations between the two kinds of tension.

Let us consider two successive Nobel Prize laureates in physics, Georges Charpak and Pierre-Gille de Genne: faced with journalists, Charpak – whose well-known achievement was to overcome the practical obstacles standing in the way of one kind of experimentation – showed his willingness to be a "technician", in contrast to his "theoretician" colleague. Beyond modesty imbibed with a bit of pride, this surely did not mean that he considered himself below the title of researcher. Furthermore, let us think of the relatively recent term "technoscience", a moving or untenable idea, yet maintained by the irresistible intuition of the growing power granted to disciplines that rely on heavy, expensive equipment, such as the imperious field of genetics silencing ecology, which is nevertheless filled with syntheses. Finally, let us mention the conglomerate of "engineering sciences", which *supposedly* share a resolution to leave theory behind. Consequently, they would not know how to act in the category of "applied research", which maintains a close, if irregular, relationship with "fundamental" science, yet an underlying instability subverts them: a community created through the absence of a rule (no theory) will only contain exceptions, but by force, theories are conceived clandestinely, "under the table". This enumeration could infinitely continue to persistently augment the sensation of an impenetrable fog.

I.2. Erasing the borders with Bruno Latour…

From the peak of its philosophical Olympus, classic epistemology has ignored techniques, reflecting only on the essence of experimentation. Sociology, on the other hand, wanting to be more down to earth, observes the position of technicians, much more rarely that of techniques, which would then tend to turn to the side of comparative sociology, except that social anthropology (another name for said domain), shockingly dominated

for more than half a century now by its passion for signs and symbols, does not care much for it: it asks only to get rid of it.

Some will rise up against this image, denouncing a view that is as summary as it is partial, and it will be safely presumed that an objection on the part of the majority will call the works that have been developed for more 20 years around and under the authority of Bruno Latour as a witness. This foreseeable argument certainly deserves a response that does not limit itself to the prerequisite of disapproval (or approval) directed at the metaphysical orientation sustained by this researcher. It is in *We Have Never Been Modern* [LAT 91] that he ends by clearly declaring himself to be a philosopher, averaging an assumed, if not demanded, continuity with his analyses as a sociologist or anthropologist. From that moment on, a discussion could not fail to insist on the incompatibility of the scientific and philosophical missions. However, 2 years earlier, another essay, *Science in Action* [LAT 89], unfolded entirely on this side of the dilemma, and the cutting remarks addressed at epistemology had still only affected it indirectly:

> "It will now be clear why, from the start of this book, I did not introduce any distinction between a so-called 'scientific' fact and a 'technical' object or artifact. This division, while traditional and convenient, is artificial, because it arbitrarily cuts across the different ways of forming alliances in order to resist a controversy. The problem posed to those who construct a 'fact' is the same as that confronted by those constructing an 'object': others must be convinced, their behavior controlled, sufficient means gathered in a given location, and a way to release the statement or object in time and space" [LAT 89, p. 213].

Where does the aforementioned division show itself to be more "artificial" than in that complete negation under the pretext that, in the aim chosen by the author as a function of his own goals, the distinction appears ineffective? By witnessing the two moments where Latour admits to the detection of a difference close to a "common sense" whose interruption here, as support, is disconcerting:

> "That said, despite the impossibility of distinguishing science from technique, it remains possible, throughout the recruitment

> process and the inspection of their behavior, to detect two moments that will allow the reader to remain near common sense by maintaining a certain distinction between 'science' and 'technique.' The first moment is that of recruiting new allies – it manifests itself most often in laboratories, in scientific and technical literature, through animated discussions; the second moment is that where one succeeds in bringing together all the means implemented in an inseparable whole – it manifests itself particularly in motors, machines, and pieces of equipment. It is the only distinction that must be maintained between 'science' and 'techniques' if we want to follow researchers and engineers when they create their subtle and supple alliances" [LAT 89, p. 214].

In the end, a sociology that is so little detached from philosophy that it respects common sense (as opposed to science) has decided to observe sciences and techniques that mix together to form complete confusion, for only an *arbitrator* of unknown origin believed it best to differentiate them. The process leads to a banishment of the nature/culture relationship, as well as a coronation of hybrids, and seasoned with the dissolution of science into belief: in fact, techniques included? Latour's thought would seem allergic to all divisions, if the whole building did not rise up from an intransigent opposition between the study of science "being done" and that of science "already done": the first is not added to the second; it dispels it. Many severe judgments by this author concerning it, sometimes refuted with disdain in virulent notes, indicate that this demarcation is not close to falling, though he alone really decided to make it impermeable because of a philosopher's intuitive liberty and not as a virtue of sociology's methodic progression.

Welcome to the analyses of the *science being done*! May they come into regular dialogue with "the others", enrich them, expand them and if necessary, challenge them. Oscillation would be necessary, even if it means reporting exclusion until it is evident on its own. In waiting, let us remark that Latour's "unifying" option, while he intended it to serve as the test of controversies, takes back all means of commenting on the disagreement between statisticians and biologists cited above, because the techniques sensed by the parties involved, just like the technicians' adjoining roles, do not appeal to "machines and pieces of equipment". Technology has always been

interested in processes that do not incorporate the intervention of particular material tools: is it wrong, and if so, why? What will an anthropology of knowledge do with this share of unannounced discredited techniques, and how will Latour's recommended inspection of the science being done treat them? A silent evacuation has taken place that certainly does not affect a single detail.

I.3. Or confronting forms of knowledge with Georges Gurvitch?

The imposing, colorful figure of Georges Gurvitch dominated French sociology until his death in 1965, but the wake of May '68 necessarily tossed the incarnation of an Antediluvian Sorbonne into the trash. Some months after his death, however, an essay written by the professor was published, *The Social Frameworks of Knowledge* [GUR 66], whose prolegomena revealed an intact vitality *a posteriori*. These recently helped one of us [GUI 14] unravel the knowledge on ecology unmindfully contained therein. Yet at that time, their heuristic value was enhanced by an unforeseen statement: the initial directions handed down by Gurvitch in his utter disgrace are in diametric opposition to Latour's resolutions, and so a methodological alternative follows. So much the better, for this would help these contrary intuitions strengthen one another through mutual clarification. Because of an abundance of unused ideas, the old master thus receives a welcome "rejuvenation" and comes back as his triumphant junior's challenger. Let us give a very quick overview of the salient points that primarily concern us in this disregarded work.

First of all, the sociology of knowledge and epistemology cannot result from one another, but they must "loyally" collaborate, permanently face-to-face, keeping a close watch on one another, all while providing mutual stimulation. Whether or not this is a vain wish, the position begs for the aforementioned oscillation between "science already done" and "science being done". It also demands that the two disciplines get engaged for themselves; if not, dialogue will not exist.

Next, Gurvitch believes that his sociological construction must find its origin in the distinction between various *knowledge genres* forming an adjustable hierarchical system in societies. The domination of one "genre" at

a given time actually increases its ability to penetrate and influence others. The classification that he begins in this domain would surely be worthy of long discussions. Let us not forget that he separates the technical, political, philosophical and scientific genres, as well as "common sense knowledge": taking an interest in the power relationships between these instances is tantamount to inviting history into the comparison of sociology and epistemology. In other words, the problem does not nullify the search for sociological mechanisms acting within one science, but rather it predicts that said mechanisms must themselves undergo historical transformations. This is in direct opposition to a widely spread fantasy in the social sciences: finding rules that push history to the margins of the structure or the system, until it can be assimilated like a mass of heterogeneous "contingencies".

Concerning the entry on technical knowledge, declared irreducible to all others, it transports us light years away from current schools of thought, Latour leading the charge:

> "However, it would be wrong to limit technical knowledge to the sole knowledge of the manipulation of matter and, all the more so, to identify it with technology. On the one hand, technical knowledge is both explicit, insofar as it is transmitted, and implicit, insofar as it is connected to practice, skill. On the other hand, technical knowledge has an incomparably broader domain than the manipulation of matter. *It is the knowledge of all efficient manipulations, artificial and subordinate, but which has a tendency to free itself and to value itself as such – precise, transmissible, and innovative manipulations, whose knowledge is inspired by the desire to dominate the worlds of nature, humans, and society in order to produce, destroy, protect, organize, plan, communicate, and spread*" [GUR 66, p. 29].

Let us freely admit that, in the preceding passage, each word, each proposition and each articulation offers countless comments, and let us admit that we would need a dozen volumes like this one to (provisionally) exhaust the subject. Except that this is not at all a criticism; on the contrary, the important thing at this stage is not to adopt or refuse to adopt Gurvitch's convictions, but to take into consideration every section of reality that he underlines and that more brilliant or sophisticated discussions have eliminated, their light masking hollows.

Do not the manipulations that free themselves and value themselves as such, when they go far beyond the manipulation of matter, have something to tell us about the friction between statisticians' models and biologists' measurements? The following pages do not at all arise from a revived Gurvitchian school of thought, and the four authors will herein react differently to the previous statement. We will at least agree on a posthumous gratitude addressed to the underestimated master: he embraced the whole range of a research space that his successors, on the other hand, have tried to restrict by prematurely specializing their inquiries in a way that brings to mind the actions of a chainsaw.

At the end of the day, the denial of borders and distinctions that mobilizes Latour reduces the diversity of the reality to be interrogated, while the classifications and selections produced by Gurvitch, despite their completely provisional value, encourage us to look more broadly. Paradoxically, we would have to admit that the technical contains much more than one manipulation of matter to realize that technicians and scientists are distinguished both by their practices and their knowledge. And we must deny this extension to favor confusion.

I.4. Objectives and horizons of this volume

It is significant that this volume finds itself at the start of a series entitled *Interdisciplinarities around the social*. Our introductory anecdote once again sheds light: the technique/science relationship takes on greater depth and increases its visibility when it is considered on the basis of cooperation between disciplines attached to distinct targets and having distant origins.

Here and there, this volume finds one of its sources in a discussion that took place in a colloquium organized by Philippe Geslin, to which Georges Guille-Escuret had been invited. Taking advantage of their longstanding friendship, he decided to start his speech by teasing his host with an infamous provocation: "What does your interdisciplinarity have that mine doesn't?" Geslin was actually broadening his experience over an impressive range of areas and debates, while his "accuser" was rarely sought.

Yet by trying to soften that initial pleasantry, the dissenter shied away from an irritating parameter: the two researchers effectively had a double

education at university – ethnology and eco-ethology for Georges Guille-Escuret, ethnology and ergonomics for Philippe Geslin – and they both favor cultural technology avoided by institutions. However, with this disparity determining that one of the paths contains a permanent technical dimension that the other lacks, Guille-Escuret opposed contradictory theories that only sought to ignore one another, waiting in vain for their adversary to respond to their arguments. Or in parallel, determining that socioecological research opportunities arise, despite the disturbing power acquired by a moralistic environmentalism. Geslin, despite the looming difficulties that left him out of mediatized debates, built up various cases of instructive collaborations, teamwork on top of everything: another volume in this series will discuss this [GES 16]. Beyond the mastery of technical competence, a multidisciplinary education passively exposes itself to the *real* moods of decision instances governing society, at a distance from declarations of intention.

"Common sense", which Latour previously evoked to anyone who would listen and in which Gurvitch underlines a genre in its own right, implies the Epinal's image of stubborn, pragmatic and (still) narrow-minded technicians before fickle, day-dreaming and (possibly) visionary scientists. The epistemologist may despise this view, but the sociologist must state that each of these qualifications is a double-edged sword: positive, on the one hand, negative, on the other hand. Judgment involves terms suggesting the flaws in qualities and inversely: as "narrow-minded" expresses the will to maintain limitations as opposed to others who discard them, and "stubborn" expresses the permanent concern for the result. In these conditions, common sense could – once is not custom – surround a truth to dig into: interdisciplinarity requires portion of technicians' authority.

Hypothesis: the success of an interdisciplinary effort incorporating the treatment of social facts rarely depends on the prerequisite constituted by a shared feeling of political or economic urgency, one a task to accomplish, a mark to reach or a solution to discover. It plays out even less from the necessity felt concerning the resolution of a theoretical discord entertained by sciences jealous over their autonomy. In contrast, interdisciplinarity often profits, in other areas, from technical imperatives that preside over its elaboration in the context of "finalized" research. In other words, *if there were only one field where sciences and techniques did not mix to the point of*

becoming undetectable, it would certainly be that of applied research. Does not ignorance in this regard result from an unavowed rejection of the range of applications among the regular and necessary stimulations exercised on theoretical practice?

Having come this far, the reader will understand that this thin volume does not truly hope to sort out this network of questions once and for all. Our goal is not modest, however, consisting of uncovering its central importance and showing some of its ramifications so as to shed light on the hidden or skewed stakes that condition the planning of a wide array of scientific projects located at different levels, which are currently thought of by means of an impasse on these mistakes or contradictions. Even by placing ourselves at this level, we do not imagine that we finish the cartography of difficulties. Instead, we will have achieved our goal if it clearly seems that an enormous site of reflection has been left abandoned, despite an ever-growing urgency, and that this oversight is not at all trifling.

The authors of what is then proposed, in the literal sense of the term, as an "essay", base their arguments on very different personal experiences, all the while sharing the influence of anthropology, cultural technology and non-philosophical epistemology, in that which resides among the sciences and not above them. Thus, Guille-Escuret pleads for an "internal epistemology", working on interdisciplinary methods [GUI 97], while Giulia Anichini, on the other hand, suggests a "bottom-up epistemology" [ANI 15] founded on an analysis of "science being done" in Latour's sense, but these aspirations converge, at least in their first steps. As for Geslin and Flavia Carraro, they are supporters of the "oscillation" between the zones of knowledge, which at times brings them closer to the "dialectical empiricism" that Gurvitch wanted to make his trademark.

Chapter 1 wraps a diachronic anthropology around a genetic epistemology that would stop conferring the central role to psychology: it outlines the reconstruction of a sociogenesis of knowledge, or more precisely, of its dissociation, through temporary frameworks that are reduced as biological evolution loses its monopoly vis-à-vis the growing autonomy of social relationships and history imposing its own tempo. Nothing more than a rough outline, however, that can, for its own benefit, lay claim to the argument that generally serves to implicitly devalue an effort of rationality:

it is “worth existing”. And, so that this quality can be recognized, the question will immediately be posed to explain the absence of competition that it must face, for if society only crystallizes “contingencies” in technical systems and scientific constructions, it would surely not be “a waste of time” to establish it once and for all.

Chapters 2 and 3 are more complementary than we had thought at the start, in the sense that they reflect one another in unexpected ways.

Giulia Anichini shows us the complex interaction between informatics and brain cartography with the help of an ethnographical survey conducted with neuroscience researchers. She is particularly interested in the use of MRI images in constructing anatomical knowledge. In the age of neuroinformatics, researchers exploit images from large databases in hopes of increasing the reliability of statistical knowledge and “solidifying” the conditions for knowledge production by reducing the “weight” of hypotheses in favor of a data-driven science. In the same way, the automation of image processing seems to support objectivity where the engagement of the scientific is relegated to the use of computer programs. In practice, the exploration of image data banks is framed by theory, without which the interpretation of results would be difficult or even stop. The observation of the scientific task also emphasizes that the ideal of image processing automation is nuanced by the manual intervention of a researcher who corrects the software’s errors by making use of his knowledge of anatomy. Conversely, other examples will witness the influence of techniques in the demarcation and classification of “natural” phenomena. In the process of selecting MRI images that will constitute a database meant to represent so-called “control” or “healthy” subjects, the definition of the “normal” brain is oriented by technical consideration, for the images must be selected to respond, among other things, to the demands posed by statistical analysis and computer programs.

We could not imagine a universe further from that previously described than the one that Chapter 3 dives into: the decipherment of ancient Mycenaean writing, so-called “linear b”, by a minuscule community of decoders. Flavia Carraro developed a “double anthropology” in which ancient and modern practices are reflected back-to-back [CAR 10]. The implications of this ethnographic and reflexive approach are found in the

anthropology of techniques and sciences. History and the practices of actors implied in the decoding of Linear b studied here will allow the relationship that the concerned areas of knowledge – philology, archaeology and cryptanalysis – have vis-à-vis the science/technique relationship to be put into perspective. The models, resources, tools and frames of reference at work in this extraordinary exploit of knowledge and technique will be understood through the process it consisted of. Posing the question of the relationship between technique and science on this terrain then necessarily arises from the shifting of the definition of technique at the same time as the consideration of the orientation of knowledge put to work and created between the multidisciplinary domain of Mycenaean studies and the knowledge system at the heart of which language and writing come together.

The initial contrast with the observation realized by Anichini (which maintains a foreignness in relation to the discussions that she analyzes) lies in the fact that Carraro finds herself caught up in a history she is relating all while epistemologically peeling away phases, clearings and swings that the relationships between the main protagonists make visible. With this difficulty that she brings for the reader to follow in the midst of all these ricochets, discrepancies, misunderstandings and reunions: let us know, then, that this journey requires a rare form of concentration and constant effort.

Also anthropologists, of course, Anichini and Carraro favor different allies: for Anichini, it is sociology that defines the relationship between engineers and researchers, the former playing the role of a "safeguard", even in the activity that it designates as "bricolage", with no irony intended. Meanwhile, it is a meticulous historical recap that leads Carraro to another "face" of epistemology, with technician-scholar relationships stemming from paths blazed by university institutions. The figure of Michael Ventris illustrates this: a technician who assists renowned researchers, but who, as an *inventor*, is received with natural, authentic deference. The technician discovers, the others are in charge of understanding, but strangely enough, all of this happens in a human science and the technician proves himself to be the leader.

If a reader whose head is filled with the classical vision that hierarchizes the series of scientific disciplines from the "hard" to the "soft" reads these two chapters one after the other, he/she runs a high risk of getting the

disconcerting impression that neurology sometimes masks the temptations of a soft science, while mycenaeology can aspire to the title of a surprisingly hard science. In all likelihood, however, the disparity between Anichini and Carraro's problems does not cause this sensation by itself: in the background of brain cartography reigns *experimentation* assisted by informatics, whereas with mycaenologists, the scepter and crown can sometimes be seen returned to the *logic* that validates – or not – experimental success. Let us, nevertheless, presume that these two researchers will only admit the intuitive content of this overview with the caveat that it is "a bit more complicated than that", taking the risk of startling the reader.

Finally, Chapter 4 will come back to the one among us who has long diligently visited engineers in their practice of anthropology: a rare bird, even an endangered species. Seizing the opportunity for this attempt to put things into some sort of perspective, Philippe Geslin, *through his experience with experimentation*, brings out the guidelines that promise the best points of reference, those for anthropology and, inseparably, for the broadening of applied research. This time, as expected, the complementarity seems to emerge over the horizon compared to Chapter 1. With a salient point that the author does not emphasize and that he most likely no longer sees due to a totally assimilated necessity: nowhere does the collaboration with engineers in the mentioned cases authorize the infiltration of a predominance and a preconceived authority so long as the sharing of responsibilities and the service of a common goal, set at the start, reject this event.

Between techniques and science, between technicians and scientists, we regularly see mistakes, tensions and contradictions. What about solidarity and interdependence? We have seen that their strength drove certain researchers to ordain a pure and simple fusion of these categories, and that they henceforth consider them to be destined to a single fate. Except that this decree given from far away has nothing to do with a practical guarantee of quality, which, represented most closely by Geslin in his contribution, agrees, to the contrary, with a clear distinction of the activities in a common project.

When hierarchies are expressed by "playing" techniques against science, the issue rarely – sometimes never – concerns one discipline in isolation. Quite the contrary, the affirmations refer, at least implicitly, to competence and efficiency relationships established between several disciplines, with several forms of organization for scientific research on top of it. Thus,