

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

coordinated by
Georges Guille-Escuret

Volume 2

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

Technicity vs Scientificity

Complementarities and Rivalries

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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.

1.1. 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 Chapak and Pierre-Gille de Genne: faced with journalists, Chapak – 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 imbued 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.

1.2. 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.

1.3. 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 [GUL 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.

1.4. Objectives and horizons of this volume

It is significant that this volume finds itself at the start of a series entitled *Interdisciplinarity 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-ethnology 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 mediated 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 unwavering 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 [GUE 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 mycenaecology 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,

particularly concerned are those domains that set forth their technological strength to claim an eminent position as a “guide” or counselor. It is then a subject that this work will not deal with, but which contributes to the justification of its existence in the foreseeable future: would not the verifiable frictions between the world of technical practices and that of scientific practices essentially come from a third, unofficial instance that is used to presuppose the desired universal or natural order and which could, for instance, be called ideology? The distortions would not intrinsically arise from techniques or sciences, but from extrapolations that salvages scraps in sight of a devouring insertion. Without further ado, let us state that there is one hypothesis there to seriously discuss, on which the four authors of this volume in no way wish to promise they share the same view.

Cryptography, a Human Science? Models, Matrices, Tools and Frames of Reference

Decipherment has always, and will continue to, by definition, belong to the field of the *technique* and *exploit*. This certainly seems obvious in the case of modern and, most often, military decipherments, where the effort of cryptanalysts is focused on creating a “key” that can crack a code and where decoding engines are at the heart of the operation. However, the decipherment of languages and writings from antiquity, or archaeological decipherments [BAR 74], poses a different question, for the exploit takes on the character of a scientific discovery in the human and social sciences, where the technique shows the characteristics of a knowledge trick.

On the one hand, decipherment constitutes the only case in this field that can be compared to the breakthroughs made in the natural and physical sciences [POP 99, ROB 02] and whose impact, restricted to the academic community, only seems to differ in relation to the latter [FRI 57]; on the other hand, this unequalled intellectual success which Pope does not hesitate to call “by far the most glamorous achievement of scholarship” [POP 99, p. 9], appears as a hybrid of erudition, analytical thought, precision, method, perspicacity and intuition, and it is a particular example of serendipity [GIN 80]. The art of enigmas figures among the fields of the *métis* [VER 91], and it is the figure of Daedalus [FRO 75] that is best for representing that of the admirable decipherer, or scientist-technician.

3.1. Decipherment between science and technique, discovery and invention

The content of questioning and the fascination inspired by this extraordinary event, but also this ambiguous object, this uncertain notion and this composite procedure, becomes evident once one pores over the existing literature on the subject. It is in following a technical orientation that modern and military decipherments are presented, and yet the biography imposes itself within the history of archaeological cases. The history of decipherers gives evidence for the search for coherence and, in light of the breakthrough made, the path illustrates the need of this same success and the indices that should have led its accomplishment to be foreseen. Decipherment appears then as the field of genius, and the operation seems to be completed because of the cooperation of individual attitudes and predispositions: polyglots, mathematicians, rather inconsistent temperaments and often badly adapted to a conventional education from a very young age, decipherers of ancient languages and scripts are first and foremost men of exception [DOB 59, ROB 02a, ROB 02b, ROB 06, FOX 13] who come to speak with the dead, to give a voice to the rocks [ROB 02a, DOB 61, NOR 75] and to finally understand forgotten languages and disused writings.

Yet the cryptographic term of decipherment has entered use in the sciences of antiquity, but here it actually refers to heteroclitic procedures of knowledge, which range from decipherment itself to linguistic interpretation, translation or reading, according to the characteristics of the writing in question: easily assimilated or not to ciphers (like phonetic writing systems), or to codes (as in logo-pictographic systems and in languages). Thus, the schema modeling situations the decipherer may find himself faced with offers four scenarios:

- 0: known language and writing¹;
- 1: known language, unknown writing;
- 2: unknown language, known writing;
- 3: unknown language and writing.

¹ Case 0 is present in [GEL 75, p. 96] and illustrated by the examples of borrowings and ancient graphic adaptations and also brought back up by [DAN 96], where it is rather assimilated to reading.

Different languages can be represented with similar writing systems just as writings have been borrowed, modified and adapted throughout history. In linguistics and philology, it is the graphic and linguistic characteristics that actually allow the series of cases constituting the history of decipherments [GEL 74, GEL 75, VOE 63, DAN 96, COU 90] to be established, playing the role that technology assumes in modern and military cases. Moreover, these characteristics seem to assume the technical aspect of the procedure which is confused with its object according to whether the intended goal – finding the unknown variable and filling a void – belongs to the domain of language or to that of writing. Put simply, where in modern cases the term defines the operation, in archaeological cases, by decipherment we understand the operation and the result of this process. And if stories about secret codes often relate the most brilliant examples of archaeological decipherments (Champollion and Ventris thus stand along Turing), in the domain of archaeological decipherments, to the contrary, reference to secret codes themselves and to cryptography is rarely detailed.

3.1.1. Cryptographies between war and peace

Let us look more precisely at the cogs of this “machine”. According to cryptanalytic terminology, the term *decipherment* refers to the translation of a message which was made unintelligible and which is found in the form of a secret code, otherwise known as a cipher. The two types of ciphers are defined by the size of the units in question: morpholexical units for codes, minimal phonetic units for ciphers [KAH 67]. In the case of archaeology, this term² is understood as the adjustment between language and writing: for phonetic writing systems, it gives rise to an unequivocal reading of signs and a stabilized understanding of texts through the substitution of the original written signs with the phonetic values that can be attributed to them within the language used by the Ancients so as to be read and understood by the Moderns. To realize this adjustment and this substitution (or rather, to escape

² In French, the terms used are “*déchiffrement*” and “*déchiffreur*”, whereas in English, it is also possible to find “*decipherers*” and “*decoders*” alongside “*decipherment*”: we will come back to the implications of this regime of the cryptanalytical analogy.

it), creators and crackers of codes, or ciphers, face off on the field of signs, their properties and skills required for their use.

These characteristics explain why military metaphors are well adapted to the description of these two kinds of processes: not only do secret codes arise from conditions of war, but the situation itself that they give rise to in and through the signs depends on a particular confrontation: a "graphic confrontation" where the goal and the end can be understood through the tactic and strategy employed.

However, one difference must be highlighted. In the case of military cryptography, the language and writing of the original message (or *plain text*) are those of legitimate users: the code adds a further semiotic level to communication, which from then on becomes technically secret. To the contrary, on the side of the Ancients, it is a matter of normal but displaced communication. It is a particular kind of operation that takes place over time and that can moreover be compared to the interpretation of messages from space: from the cryptologic point of view, both of them constitute "paradecipherments" [KAH 67]. This difference emphasizes the relation that the operation has with the terms of communication (Ancients, language, writing system, written texts) and which the previously mentioned model could not take into account. In archaeology, where the ancient text constitutes the message, the channel of expression and the context of communication, the secret comes rather from what is "not known" [CAR 17]: historic, archaeological and linguistic research, as both a point of departure and a means for the decoder, as well as the condition *sine non qua* for the adjustment of the decipherment and for the reduction of the distance [DAV 65, p. 10] between Ancients and Moderns, takes the place of the modern technical unveiling.

It is then on a complex terrain that the "algorithm" of ancient languages and writings systems worms its way in, revealing a double issue in which the linguistic and graphic skills of the Ancients and the means of accessing them by the Moderns both come into play simultaneously. Language and writing as a technical and semiotic action, but also as an act of communication, are precisely the objects to be defined. A particular relation between types or model of objects and their particularization within the actualization of the "knowledge-system" lies at the heart of every decipherment. This "system" is what ancient texts attest to and what research aims to reconstruct.

If the systems of signs that are languages aim for meaning, writings distinguish themselves in that they actually make up systems of signs whose function aims to represent another system of signs. The role that research focused on Antiquity plays in this process then invites emphasis on its ambivalent, even equivocal, nature. The artifactual nature of writing and the profoundly human nature of language somewhat blur the tracks: would not it be an opposition between science and interpretation that would best be discussed even before referring to the distinction between science and technique? For if the knowledge implied – and generated – through the decipherment of ancient languages and writings fundamentally defines the particularity of this process, the fact remains that the technical aspect of the operation is still unresolved: though not coinciding with "cutting-edge technology", it is still necessary.

This is due to different aspects. If we consider decipherment to be a *discovery*, we must appreciate its fundamentally "literary" dimension (thus more dependent on interpretation) faced with more strictly scientific discoveries [BOT 59, p. 10]. Furthermore, on the level of the *procedure* itself, it seems that a "scrupulous technique" only prepares decipherment and the exploitation of its result. Finally, in considering the *event*, we find one of the largest human "inventions" [BOT 59]. Between science and technique, between discovery and invention, and between result and procedure, archaeological decipherment finds itself "stuck", in a way. Incidentally, science could only aim for language, the technique being limited to writing, although the *experts* are philologists and linguists [PIQ 13, p. 1]. During decipherment, must we then consider that the technique only concerns the moment of substitution (the decipherment *stricto sensu*), or is it preferable to detect its presence in the research giving rise to said substitution? To respond to these questions, we have to distinguish decipherment both from the standpoint of the intended result and that of the object it focuses on, and thus to consider the acquisition of knowledge from a double perspective.

The interest that decipherment takes in the history of ideas and knowledge is tied to the history of writing, that *ambiguous technique where the matter is language, where the facts are signs and where the tendency is semiosis*. As David remarks in her essay on Egyptian hieroglyphics, "forms of writing point to degrees in the artificial" [DAV 65, p. 12]. And the scientific writing that are ciphers, those of mathematics as well as those of

codes, are part of the same history [HER 08]. If, in the 16th Century, "pure signs" started to develop within "languages" far from natural languages, the "oriental renaissance" [SCH 50] and the discovery in the 18th Century of civilizations until then unknown, as well as the revolution of decipherments "en cascade" [BOT 72], illustrate the singular relation between technique and science that writing has contributed to establish. Between history and structure, the western and graphic matrix of the decipherment started establishing itself in the 19th Century.

We are doubtlessly dealing with a founding event: on the one hand, because new graphic and linguistic facts are offered to knowledge and, on the other hand, because it constitutes an application of the historical and linguistic knowledge acquired in the production of ancient knowledge. In this cleavage, it is above all else a particular arrangement between two modes that the human sciences, even less than others, could not consider independent, for they draw closer to one another as soon as they take into consideration the production of knowledge inherent to this event. As Gurvitch remarks in a note in his essay:

"Given that in modern societies the sciences become more and more dependent on laboratories and other research organizations, public and private, the sociology of scientific knowledge understood as the study of the frames of its organization and its means of diffusion finds therein appreciable points of reference. However, in most cases, this does not allow penetration into the *depth of character of the orientation of this knowledge*" [GUR 66, p. 13].

In this view, the strategy and tactic of decipherment – with the polemic aspect of "exploit" that interests us – cling to the methods of spreading, using and creating knowledge.

The specific fields of knowledge that are enlisted in and by decipherment, those diachronic/human sciences that are history, archaeology, linguistics and philology, must be taken into consideration *per se* and in the singular arrangements that guide the practice thereof. Not only then is it necessary (as Pope suggests in his history of decipherments from the Egyptian

hieroglyphics to Mayan writing) to focus our attention on the techniques and theories involved in the decipherment process, and not exclusively on its result [POP 99, p. 10], but also to detect the technical nature of knowledge in decipherment.

3.1.2. An exemplary case: the decipherment of Linear B

The decipherment of Linear B is exemplary in this regard, constituting the last great case concerning languages and writings from the Mediterranean ancient world: Mycenaean texts revealed a form of Greek five centuries older than that of Homer, significantly pushing back the history of the language's origin [CHA 58], or more generally, the history of western civilization. The profound impact of this discovery can also be connected to its slow reception in history [CAR 06]: Mycenaean studies, which were created as a completely separate field of study on the basis of decipherment, remains an isolated niche, and Linear B is among the fields of peripheral epigraphies [CAR 00].

Decipherment makes all the difference. It is through this that Linear B is known: both "archaeological" and "cryptanalytic", it marked a decisive turning point in the history of this intellectual area. With Champollion, the interpretation of languages and writings from Antiquity had given way to decipherment, but in the case of Linear B, the cryptographic analogy materialized in unprecedented research means. In particular, technique stood front stage, all the while being assimilated into the scientific and objective nature of the procedure. This is why this case is a paradigmatic example in cryptography manuals and in treatises on ancient scripts: archetypes for the decipherments to come.

Its history is recent and, especially compared to other decipherments, perfectly documented by the papers, correspondence and working notes of the scholars who participated in it. The image of Michael Ventris announcing the resolution of the enigma to BBC in 1952 [VEN 52] perfectly incarnates the junction between *métis* and *techné* in the figure of the decoder: this young man, incredibly gifted in languages, "amateur in the best sense of the term" and a "stunning figure", as Chantreine would later define him [CHA 57], succeeded where a generation of scholars had failed, by undertaking an unprecedented analysis in the sciences of Antiquity that, internal and combinatory, was close to military cryptography. In 1954, two

years after this decipherment, Ventris was introduced as the "Prince Charming who woke the sleeping beauty" by archaeologist Björck to the audience of a colloquium in Copenhagen.

The considerable echo in the press at the time illustrates the charm exercised by the performance given. Two anecdotes demonstrate this point: June 25, 1953, the day after a conference given by Ventris on decipherment, *The Times* dedicated an article to him. And as this edition was dedicated to the very recent conquest of Everest, the title of the article concerning Ventris and Linear B was "The Everest of Greek archaeology". On April 9, 1954, the news also showed up on the front page of the *New York Times*, which presented Ventris' "exciting performance" in the following way: Linear B, "that for the last half century and longer has baffled archaeologists and linguists has been finally decoded — by an amateur".

Contrary to other known archaeological decipherments, it is not a bilingual or digraphic text, where the message is conveyed in two languages and/or writings, one known (thus allowing the text to be read and understood), nor the hypothesis of a language that guided the work. It is based on an *internal analysis of the documentation*, handling the language and writing both as unknown variables and relying on statistics. This method was represented in a graphic device: the *Grid*, a two-dimensional arrangement for phonological investigation and phonetic substitution, in which each Mycenaean syllabographic sign was correlated with the vowel and consonant of the syllable it was meant to correspond to. Here is the schematic reproduction:

	V1	V2	V3	...
C1	(C1V1)	(C1V2)	(C1V3)	
C2	(C2V1)	(C2V2)	(C2V3)	
C3	(C3V1)	(C3V2)	(C3V3)	
...				

The archaic form of words in the Greek language was thus recognized by a sort of "chain-reaction" [CHA 58, p. 67] created by the device on the two unknowns, language and writing. This kind of device was already known,

having been used to decipher the Cypriote syllabary, but in the case of Linear B, there were two unknowns and they were handled theoretically.

Yet, if the solution was accepted as a whole from the standpoint of its results, the cryptanalytic method used required additional validation from the academic community. It had to be justified before Antiquity specialists, philologists and archaeologists, for this jeopardized all of the solutions and methods that had been thought of to date. As Sittig wrote in a letter addressed to Ventris (May 22, 1953): "Your demonstrations are cryptographically the most interesting I have yet heard of, and are really fascinating. If you are right, the methods of the archaeology, ethnology, history and philology of the last fifty years are reduced *ad absurdum*".

Since the discovery of the first written tablets in Crete during the excavations led by Sir Arthur Evans in 1900, and until the later confirmation of the decipherment's achievement in 1952–1956, the heterogeneous means employed (both theoretical and technical) seemed permanently connected, just like the scientific, technical and sociological aspects of an exploit judged to be unparalleled.

As we will see, the two-sided content of the decipherment, archaeological and cryptanalytic, particularly illustrates the complex relations between science and technique in the case of an experiment conducted in the human sciences: it allows an understanding of the distribution of these two paths between fields of knowledge that make up research at the base of all archaeological decipherment and their transfer in a common application aimed at creating, producing and exploiting graphic and linguistic data.

More precisely, the border between science and technique that was envisioned about this decipherment does not correspond to the one that can be contemplated once we no longer evaluate this decipherment based on the result obtained and we consider the practices and frames of reference at work throughout its constitution. In this perspective, a singular configuration appears in which, all while being understood distinctly, the decipherment objects and the procedures that surround them show their reciprocal relations *vis-à-vis* the production of knowledge as well as its presentation (justification) and its subsequent elaboration. In particular, above and below the history of decipherment itself, this configuration allows the apprehension of the *rivalry* then the *complementarity* and finally the *superposition* between

science and technique through the different disciplines concerned: in their relations with language, on the one hand, and with writing on the other. If the result that crowned the exploit can be understood and defined as a discovery (that of a language) and as an "invention"³ (that of a writing), this complex network of relations also adds an innovation in the field of knowledge: the foundation of "Mycenaean Studies", which, between philology, history and archaeology prolongs the issues that belonged to decipherers by updating the means that they were equipped with, which the practices of mycenologists testify to today.

The "marvelous" and the "rational" [VID 72, p. 11] parts of the decipherment of Linear B can then be questioned in light of invention as François Sigaut sees it: "the technical invention only resembles a ruse if one is not careful to scrutinize the processes taking place within and behind the result that it produces" [SIG 07, p. 28]. It is this framework that we must now describe⁴.

3.2. The decipherer, the result and the procedure: the impossible solution and the technical compromise

Indeed nearly half a century and a substitution Grid were necessary for Ventris for become the first modern reader of Linear B writing and the Mycenaean Greek language noted down in the accounting documents from the palaces of Chnossos and Pylos. Paradoxically, the result obtained by Ventris was even more astounding since the solution had previously been considered during the life of Sir Arthur Evans, discoverer of Chnossos and Linear B, and since certain characteristics of the texts in Linear B, later shown and confirmed by the decipherment, had been glimpsed and admitted in his [EVA 35].

Here, contrary to the decipherment of the Egyptian hieroglyphics or cuneiform, the conditions for assimilating *inscriptions to texts* and *signs traced on clay to signs of writing* were quickly satisfied.

³ In the archaeological sense of the world.

⁴ My research arises from work done for my doctoral thesis through extensive fieldwork among Mycenologists [CAR 10]. Subsequently in 2014, I was welcomed at the *PASP – Department of Classics* – at the University of Texas in Austin, where most of the archive documents are housed.

The tablets presented groups of signs in a linear writing easily assimilated to written words, isolated signs that were often recognizable and recognized as pictograms. Alongside this were ciphers, whose addition would allow a coherent result to be obtained on a decimal basis. Moreover, the tabular structure of the texts and the presence of ciphers and signs separating words allowed the writing direction to be identified: from left to right. On a statistical basis, the signs that made up the groups of signs were quickly defined as syllabic, and the formal analysis of the texts led to the establishment of these texts' function: accounting entries. Most importantly, Evans had noticed the cases of homomorphism between the signs of Linear B and those of the Cypriote syllabary (in the same geographic area as the Greek). Considering a tablet from Chnossos (tablet KN Ca 895) where a group of two signs precedes a horse ideogram, Evans attempted an initial substitution with the Greek-Cypriote phonetic values: *po* and *lo*. While Ventris' decipherment allows this notation (in Linear B in the form *po-ro*⁵) to be traced back to the Greek word for "horse", *polos*, Evans had ruled out the Greek solution on this same basis. The Cypriote spelling would have actually required the presence of an additional syllabogram with which to note the final consonant of the word with a supporting silent vowel: *po-lo-se*.

From this fact, Evans concluded that neither the two writings nor the two languages could correspond. On the other hand, the hypotheses put forth since the 1920s concerning the grammatical characteristics that it was possible to observe in Linear B texts were accepted, but despite being integrated among the data to be considered, they were never the object of an in-depth analysis.

Evans' thesis, his life's work, maintained that, on the one hand, the arrival of the Greeks occurred earlier than the tablets, and, on the other hand, that Linear B had a close relation to the other writing systems found in Chnossos, Cretan hieroglyphics and Linear A. Concerning the latter, according to Evans, it demonstrated a graphic evolution and the probable sharing of a language, Minoan. The interpretation of the inscriptions and the categories used to describe their signs also reflects his position: it was doubtlessly a simple syllabary, but in which the pictographic signs also functioned as semantic determinatives, as in other Middle-Eastern writing systems. The Greek hypothesis was thus quickly and resolutely discarded based on a double graphic and historic analysis: if the language noted in the

⁵ Due to the coincidence of the liquids in Linear B.

inscribed tablets was not yet known, everything indicated that this language was not Greek.

However, Ventris' success also shows itself to be fascinating when we consider the method he used for decipherment. During the 1940s, Ventris drew parallels with regard to the Cnossian documents and, although initially starting with a hypothesis on the wrong language (Etruscan [VEN 40]), this procedure was developed and perfected throughout his work. The *Mid-Century Report* [VEN 88a] that he wrote relates this particularly clearly and this method hardly differs from the synthesis that he published once the decipherment was completed [VEN 53b]. The succession of stages "to decipher" is stated in the following way: "analysis", "substitution" and "check". The *internal analysis of the documents had to precede substitution (i.e. attributing phonetic values)*. This led Ventris to discard the Etruscan conjecture and to admit that, "after all"⁶, the tablets from Cnossos and Pylos were possibly written in Greek.

The stories about the decipherment of Linear B report these moments in more or less detail. The comparison – a true confrontation if we stick to the register of most of the narrations – between Evans, the "lord of Cnossos", scholar "in charge", and fated decipherer of Linear B, and Ventris, the young architect and expert of Minoan writing in his spare time, illustrates the adventure of this decipherment, even that of the latter figure. The impossibility of accessing the documents and the lack of a complete publication of the tablets by Evans (until then, the only possible expert) finally comes to explain the slowness and difficulty of this decipherment, all while clarifying the breadth of the challenge. Between Evans' "failed decipherment" and Ventris' successful decipherment, we observe in particular how the elements of the problem – the language, its lexicon and its rules, and writing, its signs and its orthography – go beyond the level of simple data to constitute the terms of a relation to be explained.

This question concerns the relation between the procedure and result of decipherment, but also between the inductive and deductive approaches aiming to resolve the problem, for if in the two cases we claim the presence

of an analysis of the documents, the framework of this analysis differs between the two. Although for Evans, historical-linguistic interpretation guides the observation and comparison of the documents, in the case of Ventris, language hypothesis and analysis are dissociated [VEN 88a]. The links between the language, the writing system and the texts lie at the heart of this question, its formulation and its response: they illustrate the divide between the "interpretations" of the writings and languages of Antiquity and their "decipherment".

During the 50 years required for the resolution of the enigma of the Minoan B syllabary, there was the war, another excavation campaign in Pylos, new tablets, the death of Evans (in 1941) and the preparation of a second volume of tablets from Cnossos by Sir John Linton Myres (Evans' successor), as well as the creation of a team of decipherers: along with Ventris, but before John Chadwick, Emmett Bennett and Alice Kober.

As we have said, the solution emerged from within a Grid that entirely guaranteed the decipherment: "mute signs were being forced to speak" [CHA 58, p. 66]... in Greek.

However, if the cryptographic method imposed an *a priori* astounding result, it also contributed to bringing a potential imposture and a real problem to light: that of the relations between interpretation, discovery and invention.

Ventris was immediately concerned with the reception of his decipherment and the foreseeable reactions, as seen in this passage from his letter to Chadwick: "I'm glad we coincided in some of the values which occurred to me after I wrote to Myres, though I suppose a court of law might suppose I'd pre-cooked the material in such a way that the coincidence wasn't conclusive" [CHA 58, p. 70]. The *Work Notes* [VEN 88b] he wrote starting in 1951, where we can follow the different passages of reasoning and the stages that punctuated the arrangement of the Grid, provided the elements to respond. Nevertheless, the adjustments figuring into these very notes also show Ventris' desire to establish coherence in his work writings, and this was perceived⁷. A brief but fierce controversy – the so-called *Great*

⁶ M. Ventris, "Deciphering Europe's Earliest Scripts", speech made in 1952 to the BBC, published July 10 in *The Listener*.

⁷ The notes follow one another, but this succession does not respect the date on which they were written.

Controversy – broke out, targeting not the result of the decipherment, but its nature.

In particular, Professor Beattie organized a seminar against the decipherment at the University of Edinburgh, totally rejecting Ventris' work [BEA 56, BEA 58]. He maintained that Ventris had been aware of the "tripod tablet" or similar tablets: he would have therefore written his last work note and filled the Grid *after* having tried his substitutions in these texts. Two articles in the daily *Glasgow Herald* (May 27 and 29, 1959) drew from this controversy: having explained Ventris' decipherment in detail, the author compared the process with the famous Piltown hoax (a monkey jaw was said to belong to a human skull due to identical patinas). Beattie associated the decipherment of Linear B with a mystification: "another Piltown imbroglio".

If the critics thus appealed to an "idealization" or a "falsification" of the decipherment, that the facts very quickly invalidated, pertinent issues arise: the relation between the Grid and its application, the role of a computational deciphering device in the case of archaeological decipherment and the source of the doubt expressed: why on earth think of a mystification?

Curiously, it was Bennett, an incontestable specialist, who had responded to the announcement of Ventris' decipherment with his "condolences" (letter written to Ventris on July 6, 1952) and who, without actually going into greater detail about the stakes at hand, underlined the interest of these attacks (today, reduced within the field of mycenology, to a dull *odium scholasticum*). According to this expert, no attempt to invalidate the decipherment based on certain words or signs truly invalidates its result, for it is a matter of isolated words, signs, limited segmentations of the language. However, the difficulties of reading and interpreting Mycenaean documents will have been minimized by partisans of Ventris as much as they were hastily exploited by his critics. In contrast, Beattie's criticism is interesting in that it focuses on the Grid: "(...) If Ventris' decipherment is anywhere vulnerable, it should be attacked at the Grid; remove other parts and the rest can stand, remove the Grid and there is no decipherment" [BEN 57, p. 553].

Levin's study of the controversy [LEV 64] analyzes the problem. The question that lies at the heart of the controversy is explained as follows: were

the phonetic values of the signs in Linear B deduced from their position in the Grid, or does their position in the Grid arise from the phonetic value that was assigned to them? Which again questions the function of this device: what does the Grid do, at the end of the day? Does it record or does it create its result?

Beyond the function of the Grid, which we will come back to, this ambiguity can be underlined by the significant fact that this same decipherment is classified (see above) into the case of decipherments where the language and writing are unknown (case 3 from the model) and into the case where the writing is unknown and the language known (case 1), according to the point of view, the moment, or the aspect considered in the process.

The response of Michel Lejeune, eminent mycenologist and supporter of Ventris' decipherment, solves the question in an explicit and unexpected way: "Even if I do not know, or not really, how the locksmith made his key, or even if I suppose that he guessed some of the particularly of the lock, for lack of being able to prove it, what is important in the end is that the key can make the lock work" [LEJ 66, p. 216]. Chased out the door, the "problem" of the decipherment comes back in through the window.

The controversy was resolved, if only by default, but it nevertheless indicated the pertinence of certain questions. Jones thus alludes to "familiar problems" mycenologists would have to face [JON 68, p. 67]. These questions are always topical: despite the mechanical character of the decipherment that was insisted on, the relation between knowledge and technique in the process could be neither linear nor "pure".

The periodization of the decipherment process, punctuated by the "stagnation" period, Evans' time, the "prelude" phase, and the American contribution to the decipherment, then the moment of success [VEN 56], allowing us to observe the competition between two forms of decipherment: between theory and interpretation, on the one hand, and between analysis and decipherment, on the other hand. At the same time, a split arises within the very cryptanalytical process, where analysis is separated from decipherment itself. The justification for decipherment and, finally, the way in which the decipherment has been integrated into the history of Mycenaean

studies reveals a sort of bricolage: a technical compromise where the new decipherment is inserted into the frame of reference of previous decipherments and where the cryptographic aspect, stemming from outside, has short-circuited the most specifically philological and archaeological aspects of the matter, with a transposition on another level of solving the problem and a relegation of the "rest" into a preparatory phase. Philologists and archaeologists get involved just before and right after, while in the middle, a technician and his tool are at work: the line between science and technique is safe.

On the other hand, as Ventris asks in his final work note, what is the most incongruous, the Greek writing at Knossos or the "Cretan" writing at Mycenae? An unbearable imaginary and a scholarly tradition laden with history cohabited the interpretations of an initially Minoan and later Mycenaean writing. From there, the mix earned the definitions of the "deciphering power", the ability to read and the skill to understand, at the same time that the result of the decipherment, through "the exploit", exhumed a language already rather well known: Greek! In such a climate, nothing is surprising in that a Grid was responsible for deciphering Linear B and that this work came to represent a completely separate mode of knowledge: "the Ventris theory" [POP 99, p. 178].

3.3. The Grid, tool, instrument and machine, and the regime of proofs and tests

The Grid perfectly illustrates the following method in the decipherment of Linear B and represents Ventris' "key" in that it assumes the "adjustment" of the Greek-Mycenaean language and of the syllabic writing on the tablets.

Since the very beginning, Ventris and Chadwick insisted on the role played by the Grid, which, leaving the smallest possible space for interpretation [VEN 53a, p. 88] and then for the decipherer, allowed a substitution to be sketched before attributing phonetic values to the signs. However, this "before" is problematic with regard to a description of the procedure itself. How, without the help of any bilingual support, did the signs allow one and only one language, the one spoken by the Mycenaeans and written in their accounts, to be accessed? Ventris wrote in his first work

note in 1951: "It is risky to guess what the consonants (or vowels) actually are: but one can predict that when at least half of the signs of the syllabary have been securely fixed on the Grid, it will need only a small number of inspired pieces of linguistic deduction to solve the whole 'simultaneous equation'.

The relation that the Grid's logic has with the "simultaneous equation" of language and writing that had to be resolved, on the one hand, and with the "pieces of inspired linguistic deduction" necessary for this resolution, on the other hand, lie at the heart of the Grid's function in decipherment vis-à-vis the Mycenaean documents and in view of the interpretation task.

The Grid is a table [CAR 12] divided into lines and columns according to the number of consonants (C) and vowels (V) of the language and whose boxes are filled by the Mycenaean signs, placed using text analysis, hypotheses proposed concerning the grammar and structure of the language, and the functioning of the notation system (orthography and spelling rules). This organization, where the Mycenaean syllabograms are distributed according to their phonetic structure without any value being attributed to them, features the spatialization of the equation applied by Ventris.

The first function of this table consists of lining up the Mycenaean signs according to the relations they have with one another in the texts, which are reported by the very coordinates used to name them: $C_{(1, 2, 3, \dots, n)} V_{(1, 2, 3, \dots, n)}$. The advantage of its use is to allow the creation and simultaneous completion of a puzzle in which the Mycenaean signs (C_1V_1 , C_2V_1 , C_1V_2 , etc., according to the possible combinations) find a *phonetic solution* based on their position and by respecting the coherence imposed by the two coordinates, while the phonetic values receive a *graphic solution* through the signs of the repertoire of Linear B. This double movement of the Grid, and this double reading, lies at the heart of its use by Ventris in deciphering Linear B, and it also explains the different "states" under which it appears.

In the first place, three grids appear in the *Work Notes* (1, 15, 17), showing the changes that take place both in the position of the Mycenaean signs in the table's squares and in the phonetic types that make up its coordinates. Uncertain signs are recorded in the margin, with different possible or probable options. Thus, the first Grid includes nine lines and two

columns; in the third, 15 lines correspond to the language's consonants and five columns to the vowels.

Then, an "experimental syllabic Grid" is published in the first publication of the decipherment *Evidence* [VEN 53a] as an illustration of the decipherment achieved, which presents rather a table of correspondences between the original signs and their phonetic transcription. Furthermore, in the successive works [VEN 56, CHA 58], the table of signs is built using phonetic series, while the canonic repertoire presents the order of the Mycenaean signs based on their shape, from the simplest to the most complex. This subject will also be considered below.

Finally, a less known fact, the Grid is mentioned and explained for the first time in a letter addressed to Kober, dated March 26, 1948, thus before the compilation of the famous "Mid-Century Report" and shortly after Kober's publication of her last article. Ventris therein insists on the role of statistics in decipherment ("I think pure statistics will get us a certain way") and presents the new "form" that he is creating to cross-reference the *probable* consonant and vowel values of the signs, which he calls "Grid²". "I am trying gradually to build up the probabilities for phonetic values in a form which I call 'the Grid'". The letter ends with an invitation to exchange grids that Kober may at some point create.

The difference between these grids is considerable. The letter to Kober demonstrates Ventris' use of a graphic means to process statistical data, which could then also be adopted by other researchers. Additionally an undated notebook found in the archives, Kober had traced a true grid in pencil, very similar to Ventris' Grid, with the Mycenaean signs almost all in the right place. The *Work Notes* show the development of the substitution process according to the parameters that are the positions of the signs of Linear B within squares and the hypothetical phonetic values that determine the structure of the table. The Grid from *Evidence*, mentioned above, shows the substitution implemented according to two distinct plans: the written signs and the syllables of the language that these signs represent. Considering these states, it is possible to present the Grid as:

— a table whose structure contains *per se* logical properties functional for decipherment;

— the synthetic graphic layout of a heterogeneous set of Mycenaean data and hypotheses;

— the ordered arrangement of Mycenaean graphic and linguistic data according to their correspondence and in their distinction.

The correlation between these aspects, or these occurrences, emerges in the relationship that the Grid installs between inputs and outputs, as well as in the function that it finally recovers as a cognitive tool [GOO 77, NOR 91]: machine and model of decipherment, for the experimental Grid was also an object of experimentation. A series of operations allows this dimension to be highlighted.

The principal stake was to put the Grid to use with particular syllables: to "breathe sound" into it. In the first place, Ventris proposed considering the sign that appeared most often at the start of groups of signs as a vowel-only syllable based on statistics. He attributed the hypothetical and provisional phonetic value "a" to this sign. Then, using the homomorphism between the signs of Linear B and the signs of the Cypriote syllabary, already observed by Evans, he used the syllables "ti" and "na" for two of these signs. The activation of the Grid was thus realized with these three syllables: "a", "ti" and "na". Using these three values, the first series of substitutions took place within the Grid, thus affecting three signs in the columns ("a" and "i" series) and two signs in the lines ("t-" and "n-" series) that were henceforth found to have a "name" no longer expressed in the language of the Grid and through its coordinates, but made of sounds.

Second, based on the possible parallels with Middle-Eastern accounting documents, Ventris put forth the hypothesis that certain groups of signs represented place names: in various texts written in Linear B, these groups of signs seemed to reiterate, and in the same order, what was known (particularly in Ugaritic) to be toponyms followed by lists of commodities. These toponyms thus served as test benches for the phonetic values considered and the relations that the Grid was meant to map out (for the proper names had the property of being conserved over time and varying little from one language to another). The toponymy of Crete from the first millennium was well known and from the very first try, using the name of the closest port to the palace of Cnossos, *Ammisos*, by counting on a syllabic notation (*a-mi-ni-so*), other place names became legible: Cnossos, in the

form *ko-no-so*, and Tyllisos (*tu-li-so*). The table was filled with new vowel and consonant series, as well as new signs.

Yet if the names of places can guarantee the phonetic values considered, they cannot provide information about the language for the very fact of their phonetic stability from one language to another, and two different languages can have sounds in common. It was then the turn of the groups of signs that could represent the names of the commodities recorded in Mycenaean accounting, e.g. those that were found before numbers and beside ideograms. The *Grid* thus allowed words to be read, particularly the words for "boy", "girl", or even the one for "total", the meanings of which had been suggested for a long time based on their form: *ko-wo*, *ko-wa*, *to-so* and *to-sa*. What is more, Ventris observed that certain groups of signs presented the same radical as the place names and a variation in their final part. From this fact, he was able to deduce the written form of adjectives derived from place names: *a-mi-ni-si-ja*, *ko-no-si-ja* "from Amnisos", "from Chossos" and these elements allowed him to go from place names to words of the language and their grammatical formation.

The recognition of the language that these words were part of completed the process: the written form that the *Grid* had allowed Ventris to reconstruct was extremely close to that of the Greek language known through the alphabetic tradition. Put another way: the words *ko-wo*, *ko-wa* and *to-so* sounded like the Greek, and the parallel with the Greek form *kouros*, *koure tosos* was immediately apparent to Ventris. What a surprising result: his *Work Notes* end on the suspicion that the result is the one common sense had until then allowed to discard, possibly showing the flaws in the procedure followed until that point and in the hypotheses that it had been built on. Had not he rather been "cooking up" (the term he used in a letter to Bennett on June 1, 1951) a Greek decipherment, when he was "just" organizing the recipe?

This series of operations illustrates the functioning of the *Grid* and features the tests that give rhythm to Ventris' work toward his instrument. They also allow a tension to be understood: while perfectly stemming from the cryptographic technique, the stages that punctuate the implementation of the reaction in the *Grid* show the elements of comparison with other decipherments and the difference of framework they can be situated in. The decipherer's interpretation and knowledge are put to the test.

The parallel with the Cypriote writing is based on the consideration of the shape of the signs and, unlike Evans, without either the writing system (with its orthography) or the language intervening in the operation. The toponyms allow the minimal units that are syllables to be processed: on the one hand, in regards to the substitution table they result from and, on the other, before a proper noun, which remains independent of the particular languages all while belonging to the language. Furthermore, the very choice of groups of signs corresponding to place names in the texts means the consideration of the text and the literacy it attests to based on another series of texts, different and foreign to the set in question, but nevertheless comparable. This means trying probable words expected to occur in the texts based on different clues, as is the case in military cryptanalysis. Finally, the recognition of common nouns of the language within the groups of signs is based on the two preceding stages at the same time that it means the comparison, implicit in recognition, of the inscribed form with a particular known language and whose competence is incorporated to the decipherer.

These operations consist of first differentiating writing, language and what is written (as graphic occurrences, notation and texts) in order to isolate and define the field of that which constitutes an experimentation in language and writing. The entities that are taken into consideration each time – signs and phonemes, groups of signs and proper names, written words and words of the language – are linked to a superior unit of analysis that determines its meaning and function in order to assure a continuous progression whose final assembly will allow the *language system* and the *writing system* to be revealed by distinguishing them.

The objects to be processed and tested are no longer the language and the writing system as in the case of Evans' failed decipherment, but rather their constituent units, and it is through this segmentation that the recomposition becomes not only possible, but probable.

In particular, the comparative process effected in a restricted and situated way no longer acts in the medium term between two writings and/or two languages (the Cypriote syllabary and the Greek, for instance). Due to the level at which the framing of the comparison (signs, groups of signs, words) is situated each time, it operates between graphic facts and their

relations extracted from all positive content. In other words, because of the Grid and through it, by verifying hypotheses without them bearing directly on the solution of the decipherment, progress is made in translating linguistic data into graphic data and vice versa until there is complete superposition. During the final test, the consonants and vowels that make up the coordinates of the Grid become the signs of the language itself, even its spelling: to then be compared with the historic, also alphabetic, writing system, in order to reconstruct the lexical sequences and finally the historical state.

The Grid thus carries out a fundamental mediation: it allows for the verification of that which constitutes pieces and stabilizes the "back and forth" necessary for their rearrangement. Playing the role of relays between language and writing, it distributes the key of the decipherment into devices – in this case, sorts of bilinguals, or digraphs – contextual and real, analogical (like the Cypriote signs) or diffracted (as in the case of toponyms and words). Using these keys, Ventris' linguistic deductions reveal and verify themselves, progressively moving through the layers of the language and the writing that henceforth adjust one another after the identification of possible crossing points. The relation between the graphic, textual, historic and linguistic elements varies each time according to the form, level and degree of adjustment using a method that is both comparative and experimental, where the interpretative task is broken down and distributed across these levels: verifiable through precise tests. The decipherment experimentation inscribes the signs of Linear B in the environment from which they can initially acquire phonetic pertinence, then graphic pertinence and, finally, linguistic pertinence. However, this also means the processing and translation of this environment into the signs that become its clues.

If it is the acceleration of the process, increased by the tool, that has been emphasized consistent with cryptanalytical mechanics, it is rather the deceleration it is based on that allows the technique of the knowledge process to be grasped: the Grid works through the translations of downtime that coincide with the activation of heterogeneous analysis unities but that becomes henceforth comparable. This activation is confirmed in the proper and the epistemological sense, for the Grid-machine is quite capable of processing the data and engendering the values based on the "program" its functioning is based on. In this perspective, the Grid is disposed to read and write: like Turing's machine, because of the instructions implicit in its

structure, it goes to the next position by going up a level. The translation of Mycenaean writing into alphabetical writing also means translation from the graphic level to the linguistic level, which, separated beforehand (only the Mycenaean signs in the Grid's boxes are writing, while the values of the coordinates correspond to language), are superimposed again, showing and – finally – making the written Mycenaean language legible as a state and form of the Greek language.

However, if the parameters, the functions and the tests of experimentation become its results, it is necessary to dwell on that that provides proof of the decipherment: the Greek-Mycenaean language and the meaning of the texts.

On the one hand, philology and linguistics come into play. Using the known Ancient Greek language, it was necessary to go back to the Greek-Mycenaean, and finally to translate the two Greeks into one another, thereby illustrating the linguistic change that took place between the two states of the language, as well as the rules that governed its notation in the Mycenaean syllabary.

On the other hand, the proofs depended on the texts and particularly on the ideographic signs that illustrated the meaning of the words being read, according to what is called Mycenaean "double writing". In fact, the decipherment was definitively confirmed when new texts were read by people not involved in Ventris' analysis: through the phonetic substitution outlined, they could not only read, but also understand them. As in the sensational case of the "tripod tablet", PY Ta 641, found on the Pylos site, in which American archaeologist Blegen could read the names and description of different tripods, recognizable in the ideograms in the text. However, another piece of evidence must draw our attention: the discovery, in 1957, of the central fragment of one tablet from Pylos (PY Ta 709) of which only the first and last parts had been published. The announcement of this text was entitled "The missing link"⁸.

The previously known parts of the tablet having been studied "separately" beforehand and published in 1955, the reading and translation of the central fragment thus had to be consistent with these conclusions. The ideograms showed that it was a list of cooking utensils and the different

⁸ Mabel Lang presented her announcement on the fragment (at the *TAPA MEETING* in Washington) under this title in 1958 [PAL 61].

interpretations agreed on the reference to a "household". The new fragment allowed word *e-ka-ra* "household" to be read and the consistency of the translation to be established. The concordance between the meaning of the adjectives, all relative to the particularities of Mycenaean households and the grammatical coherence with regard to the names and the adjectives was therefore successfully put to the test of the archaeological context: it did indeed have to do with those households whose decorative characteristics had previously been described by Evans.

The Greek reading of the central fragment of the tablet meant empirically reconstructing a textual, linguistic and archaeological puzzle that left no doubts. The "missing link" clearly illustrates the regime of proofs for the decipherment, but also echoes the tests of the experimentation presented above and the activation process of the Grid. The conformity of meaning between the messages and the ideographic part of the tablets also makes the ideograms sorts of bilinguals or, better yet, digraphs within the text; the attribution of the Cypriote value to the homomorphic sign of Linear B ties the artifact of the written signs to the phonetics of the language just as the coupling of tablet fragments reconciles the support with the text; place names, which have a particular status within languages, work as sorts of ideograms, linguistic signs subject to very little change, allowing a reconstruction of the language from a phonetic standpoint, but whose implications on the grammatical level are quick to appear, as the derived adjectives found by Ventris show; finally, the application of phonetic values to new texts and the reading by third persons confirm the exactitude of the substitution, for a phonetic writing always involves a unique reading.

More explicitly, the proofs of the decipherment allow the relation of experimentation of the Grid with the analysis of the documents to be understood: the Grid is the outcome and, at the same time, the shape of the reasoning, and it connects the event of the decipherment, as well as the solution found, to the process whose procedure was the result.

3.4. An applied and interdisciplinary internal and collective analysis

The analysis of the documentation entered with the Grid allows a long knowledge process to be followed. In this, as the archives testify, the

protagonists take turns and coordinate their actions over time, while dissimilar skills, knowledge and methods are used and converge in the form of a collective project. Outside the cumulative progression of knowledge that, because of several contributions, finally led to progress, let us instead see here a disposition specific to this decipherment of historic, sociological and anthropological dimensions established since the 1940s.

Starting with the discovery of the first tablets with inscriptions, there were many scholars coming from all four corners of the world to tackle the Minoan enigma. Some set themselves apart: Ventris' collaboration with Chadwick, philologist and linguist, who accompanied him in the reconstruction of the Greek-Mycenaean lexicon from the first months following the announcement of the decipherment, was essential and recognized as such. The situation is quite different for American scholars Emmett L. Bennett and Alice Kober, who played a leading role in the internal analysis (Ventris was in close regular contact with them)⁹.

It is particularly in the "preludes" [VEN 56] and the "decor" [CHA 58] of the decipherment, properly said, that they were most often located: the installation of the "foundations" [PAL 11, p. 44] of the decipherment to come. If, after the decipherment, Ventris and Chadwick had to "defend" it [AUR 08], the most recent presentations [POP 11], which accept it unanimously, insist more on the details and the actors. The way the decipherment itself is foreseen, with the succession of phases, has hardly changed. The directly exploitable results produced throughout the analysis stay a priority: to Bennett, we owe the first study on Mycenaean scribes, which built the foundation of Mycenaean paleography and epigraphy, and the classification of the signs and documents that is still in use today; Kober is known, rather, for her "triplets", that is, the three grammatical cases of the Minoan language that she identified to reveal, just before her death in 1950, the phonetic relations between consonants and vowels within Linear B syllabograms (a foundation for the framework of the Grid). Bennett is said to be the "Linnaeus of Mycenology" [POP 89, p. 25], while Kober's role, more directly linked to the decipherment itself, is compared to Rosalind Franklin's role in discovering DNA [FOX 13].

⁹ Let us note that the first work based on the study of archives did not appear until 2013, in a book written by a journalist for the general public [FOX 13].

More than anything else, these extraordinary contributions have aroused, explicitly or not, a question on *the completion* of decipherment. This is particularly striking in the case of Kober. Thus, a question seems to have haunted mycenology since then: would Kober have succeeded if her illness had not sent her to an early grave and stopped her work? And if Bennett's work has only drawn attention in rare cases [DUH 89, POP 89, POP 11], the question also arises with regard to him: why is it that this expert, who had privileged access to the documentation and who served in the American army as a cryptanalyst for Japanese messages during the war, did not come closer to the solution? These questions become epistemologically pertinent between the history of decipherment and the stories of decipherers, particularly with regard to the relations that the decipherers had with one another. They are interesting on more than one level to study the relations between decipherment, knowledge and technique, even their equivocal positions throughout the deciphering process.

Remarkably, before the unique exploit that decipherment represents, the performance revolves around a competition between scholars, for there could only be one decipherer for one decipherment. Pope, for example, writes that Ventris' practice, all while following in Kober's footsteps, was "much more audacious" and "much less assured" [POP 11, p. 7]. Then, concerning the method, the collaboration established between the decipherers allows not only the role played by each one, but also the efficiency of different kinds of knowledge employed in the task that they undertook to be established. Finally, concerning the objects implied in decipherment – the writing and the language – it is possible to observe that one is affiliated to the technique (as an object) and the other to scientific knowledge (as a goal).

A split between analysis and substitution thus insinuates the cryptanalytical process itself within the decipherment, and taking into account the collective character of the operation on Linear B allows its modalities to be highlighted. Indeed, the participation of several individuals in studying the Mycenaean documents does not necessarily coincide with the joint work established for practical means. The creation of what Ventris and Chadwick called a "team of decipherers" [VEN 56, p. 16] draws our attention.

Evans' death and the excavations at Pylos mark a definitive turning point. Sir John Myres quickly needed assistants for a complex and delicate task: the publication of the second volume of *Scripta Minoa*, which was to include all the inscriptions that Evans had never published in his lifetime. These assistants were Michael Ventris and Alice Kober.

The first, because of his 1940 article, had developed an ongoing, privileged relationship with Myres until he left the work for personal reasons and intellectual disagreements [ROB 02b]. In particular, the competence and dedication with which Ventris recopied the signs of Linear B over several months with Myres for Oxford University Press earned him Kober's derisive judgment, according to whom he "would have had no trouble getting a job as scribe for King Minos" (letter to Myres on July 8, 1948). As for Kober, linguist and professor at Brooklyn College in New York, she had started studying the Minoan B syllabary in 1928 [HAH 50], dedicating the time that teaching left her to this task. In 1946, Kober, having already published some important discoveries, received a fellowship awarded by the Guggenheim Foundation, which gave her the chance to fully dedicate herself to her passion. She then wrote to Myres to obtain permission to study the material from Knossos: after two trips to Oxford, in 1947 and 1948, where she copied the tablets and met Ventris, aware of Myres' lack of preparation and of the risk of mistakes likely to threaten later analyses, she offered to edit the texts.

At the same time in the United States, after the excavations led by Blegen at Pylos, advances were being made in editing the continental material¹⁰ in Linear B. The publication was entrusted to Emmett Bennett who, since his thesis with Blegen at the University of Cincinnati (1947), had been working on the tablets and their signs. It is concerning the comparison of the Pylos and Knossos tablets that Kober and Bennett started working together, through an initially "top secret" cooperation plan (letter from Kober to Bennett on August 8, 1948), then with Blegen and Myres' authorization to cross-reference their studies of the tablets. Their correspondence, until Kober's death, reveals the particularities of their respective analyses as well as the commonly employed techniques, the convergences and divergences, concerning, for instance, the classification of the documents and signs where

¹⁰ Pylos is in Peloponnese.

their points of view were irreconcilable [BEN 53]. Ventris was once again taking part in the plan. Blegen allowed him to access the documents from the American mission and Bennett became his primary interlocutor, the first to know about the reading made possible by the Grid (letter written on June 18, 1952).

The corpora of inscriptions did not appear until 1951–1952: throughout the years of preparation, these editions define the decipherment playing field. In particular, concerning signs, their variants and the context of their usage in the texts, this allows a simultaneous understanding of the emergence of knowledge that concerns the language, or writing and the specific mode of knowledge that the decipherment progressively came to represent.

The two processes – editing and decipherment – are closely linked, because access to the documentation was a fundamental prerequisite for all analysis (before publication, only the archaeologists who led the excavations can truly access the documents), but also because the analysis of the documentation, necessary for editing, coincided with the establishment and definition of the objects to be deciphered: the signs, the system that they made up and the system that they represented. The decipherment process finds its context here, and the decipherers' work finds its *raison d'être* and its means. The collective was formed concurrently with the instituted teams.

The correspondence between the decipherers ensures this collective's ground. Much more explicitly than in the texts published or circulated and the conferences held, the decipherers' letters provide clarification of their *savoir-faire* and scholarly literacy made up of notes, outlines, diagrams and calculations, along with the formulation of the questions, concerns, problems and priorities that kept them going.

Editing foresaw lists and classifications of the inscriptions and their signs, as well as of the tablets as archaeological vestiges. The undertaken analysis of the documents takes place within the necessary referencing [LAT 95] and within the establishment of the tools that are lists of signs, classifications of documents and index (*encyclopedias* of the signs and their use in the inscriptions). The scientific and technical approaches cross in editing that features the collective problem of access to the

documents just as much as the means of attaining the shared goal: deciphering.

The publication of the material demanded examination of the Mycenaean documentation and a stabilization of it for the entire scholarly community. A very concrete problem: no sign charts had been found in the excavations, and if Evans had published Linear B's repertoire from the beginning, the idiosyncrasies that it presented concerning the shape of the signs and their variants and their function as phonetic or "pictographic" signs revealed the need for a detailed study that defines the graphic items. Failing that, a point of no return would quickly be reached. There were actually several lists of signs, which risked promoting confusion and uncertain references. The creation of one standard list left hope for a foundation for later eventual corrections or changes [KOB 48, p. 93]. With the Pylos documents, this need became urgent, Linear B quite unexpectedly coming out of the Cretan area. In other words, it was necessary to decide if a single writing system, thus a single language, would be referred to for the two sites. Thus, analyzing the signs traced on clay from originals, their copies and available photographic reproductions, comparing their shape and situating them in the graphic environment they were part of through the texts for the two corpora, Pylos and Cnossos.

The decipherers' papers show that they were all in the process of looking at the signs, cross-referencing the texts and making calculations using a panoply of graphic devices to count, arrange and compare the signs and texts. So as to isolate the variants and the scribes to identify the frequency of the signs in the groups and in the texts according to their presence, relative – at the start, in the middle, or at the end of a group – and absolute, to calculate their occurrences, and finally, to establish a graphic repertoire, the decipherers drew diagrams, charts, files (Kober filled 186,000), using multiple colors on pages with various formats, on pieces of paper, sheets stuck to one another, or notebooks.

The difficulty of the task shed light on the similarity to military cryptography, even, more generally, with the nascent field of computer science and the application of statistical methods. IBM proposed its services to Professor Blegen (letter written to Bennett on January 31, 1950), and Bennett discussed advantages and disadvantages of such a strategy with

Kober, who used punched cards from the beginning of her work (her letters talk about this in 1945), prepared using all sorts of cardstock – postcards, birthday cards – carefully punched by her mother and brother at home in the evenings, which allowed her to quickly identify the signs through the texts of the inscriptions. Just as Ventris invites Kober to use the Grid (see above), Kober repeatedly highlights the utility of these cards to Bennett: their fabrication requires a lot of time, she explains, but once in place, they save hours of work because of the quick comparison they make possible.

Internal analysis, combinatory and contextual, was thus a necessarily collective and indispensable stage, and statistics became a common method within the group, from then on restricted and highly specialized. Henceforth, the experts on the Linear B Minoan syllabary were Kober, Bennett and Ventris.

Nevertheless, these same calculations that point to a shared instrument also reveal the difference in the decipherers' goals and research objects. Counting also necessitates rules that must be agreed on. The exchanges between Kober, Bennett and Ventris concerning the problem of the list of signs and the index to be compiled are very precise and they bring to light the dissimilar and frequently incompatible options: to consider the groups of signs that are featured on the tablets as words is not at all tantamount to considering them names; counting based on the frequency of the signs or counting based on their occurrences does not always lead to the same results; finally, the perspective changes if we consider the traits physically constituting the shape of the signs rather than concentrating on their function. The lists, schemata, ciphers and questions multiply as the different decipherers' points of view and their scholarly discussions are considered. If Ventris always admitted the competence and legitimacy of the two other decipherers, remaining in the background concerning their scientific work, he did not fail in his letters to note the doubtful points as an "outsider" and technician. An example of this is the case of the frequency calculations, where he remarks to Bennett that the occurrences of the signs leads to an unpredictability factor in the calculation, because in a series of tablets, an identical group of signs can be repeated, referring either to the name of a person or to a vocabulary word. From this "point of detail", the resulting frequency of the signs will deliver the result of an administrative accident more so than the distribution of the syllables of the language (letter written

to Bennett on April 20, 1949). Kober demonstrates a similar position, however, in the remarks in her correspondence with Bennett about the need to establish the lists of words that can provide stability to the signs and coherence to the calculations of their frequency¹¹.

As Kober writes to Bennett on July 3, 1948, "The day will come where we have all the evidence and then a lot of these difficulties will disappear. In a way this period is not too bad, because we are formulating the questions that must be answered and so will know exactly what to look for when we get the chance".

At the heart of these calculations, which translated Linear B and its interpretation into a question of ciphers, lies the key to the frames of reference that forged the cryptographic method and the Grid and that run across their implementation.

Another disparity is worth underlining, namely the role of the collective in the decipherers' work. In the case of Kober and Bennett, editing the texts and collaborating were a necessary step for the analysis of the documentation the excavations had unearthed, because of which they could finally create an adequate repertoire of signs on which to base the decipherment. On the contrary, on Ventris' side, the collective made up a goal in itself that he pursued all throughout his work, in accordance with the methods that he had learned in architecture (where, through a "working group", projects are discussed and put in place by heterogeneous and complementary competencies).

The "Mid-Century Report", mentioned above, perfectly summarizes this. A 21-question survey was sent to scholars in several countries with the aim to pool the knowledge acquired since Evans' discovery through a sort of "informal exchange" [VEN 88a, p. 32]. The responses, which Ventris translated, formed the report with tables that synthesized the different positions and characteristics of the problem: the elements of text interpretation, the linguistic and historical context in which they were written and used, problematic points and plausible hypotheses. The opinions provided were discordant and the comments sent attest to the fact that the questionnaire instilled a genuine spirit of exchange.

¹¹ The letter written on June 7, 1948, for instance.

All of Ventris' activities must be observed through this prism. In this way, he wrote his work notes both as a report meant to be distributed and a sort of rough draft for his own personal use. In his first note, he wrote as an introduction: "Please make use of these notes or discard them as you like, and do not bother to acknowledge their receipt unless you feel discussion is worthwhile. It would be helpful if we could have similar informal notes circulated by different researchers from time to time, since some progress may often be made by comparing notes on difficulties and 'hunches', which are not definite enough to be presented in published work". In his correspondence, he also communicated his ideas, hypotheses and remarks, each time engaging open discussion; Bennett (whom Ventris often wrote several times in the same day) defined this as an unbridled, "terrible flow of ideas" (letter written to Ventris on December 12, 1950).

Moreover, the regime and variable content of the exchanges show not only distinct positions concerning the analysis of the tablets, but also concerning the content of the collaboration.

The decipherers' different status allows for an explanation of the tone and position in the letters that Bennett, Kober and Ventris exchanged: the first, a scholar and archaeologist; the second, a linguist, educator without a renowned position within the academy, and a woman; the last, an architect and young amateur. From this point of view, only Bennett was *a priori* a potential decipherer, the two others finding themselves at the periphery of the field and community. Kober's response to a letter from Bennett emphasizes this point: "I don't know if you were serious in talking of us as rivals. I was not. Rivalry has no place in true scholarship. We are co-workers. I will be glad to help you in any way possible. The important thing is the solution of the problem, not who solves it" (letter written to Bennett on February 12, 1949). The slogan Ventris chose for "his" collective echoes this: "Let our motto be: on the way to a decipherment by 1950!" (letter written to Bennett on August 17, 1949). However, Kober did not show the slightest availability for a direct collaboration with Ventris and she even refused to respond to his questionnaire, severely contesting his presuppositions [VEN 88a, p. 37].

The collective of decipherers allows the recognition of a thread linking the editing to the decipherment, and consequently, internal and

statistical analysis to experimentation, all while underlining the protagonists' distinct methods and positions. It is on this path that we will go beyond the series and explanatory framework "decipherment/event/result" and instead follow the frames of knowledge at work and the relations they have.

3.5. The patterns and mechanics of the documents

Kober's name is tied to the most linguistic aspect of the analysis of the Mycenaean documentation, to the grammar of the inscriptions and finally, to the phonological traits of the Linear B syllabograms in which the primitive form of the Grid has been seen [DOB 59, p. 259]. However, it is using the "science of graphics"¹² that she founded and "the mechanics of the material", which she speaks about in her correspondence (and which she revealed), that we will consider her "decipherment". Her results can also be found there, showing the relation between technique and science, which they came from and announce, in a new light.

The articles published between 1944 and 1949, the classification outlined for *Scripta Minoa II* (posthumous publication in 1952), and the unpublished conference manuscripts, on the one hand, the notes and letters, on the other hand, present her methods and results; their cross-study makes the central axis of her work, her design, clear: a description of the written language of the tablets that allows "the pattern" to be found by deduction.

During the 20 years dedicated to the Minoan writing, Kober touched on every aspect of the problem. She trained herself to copy the Linear B signs as quickly as possible, so as to make the writing more natural, and prepared statistical files for each sign, just as she compiled the lists of identifiable words in the texts. She examined the textual environment of the signs and counted the frequency of their appearance in the inscriptions. She took part in archaeological excavations in Mexico and studied a considerable number of ancient languages, such as Lydian, Lycian, Carian, Persian, Sanskrit, Akkadian, Sumerian, Basque and Chinese. Finally, aware of the novelty of the process undertaken, not only in the field of Cretan studies, but of writing in general [KOB 45, p. 144], she made it a scientific and logical matter by taking an interest in mathematics, physics and chemistry. According to Kober, the use of these disciplines, seemingly so far removed from

¹² This is the term used by Kober during a conference, not published and held at the Yale Linguistic Club on May 3, 1948: "Form without meaning".

decipherment, lay in the need to rigorously develop hypotheses using the facts that the inscriptions provide and to formulate the right questions. She notably refers to Euclid's postulational method, which states that a set of postulates is necessary to establish coherent series of hypotheses (whose truth can be deduced logically without being demonstrable). These elements are part of a new method where science and technique constitute the concomitant effects of the reality examined.

Kober often alludes to the great decipherers Champollion and Grotefend and to the fact that, in the case of Minoan B writing, the same clues could not be exploited: most of the successful decipherments done in the past were based on lucky guesses arising from information about the language, the values of the signs, or the meanings of the texts in question, and according to Kober, it was too often thought that this same "technique"¹³ would remain useful in cases where this information does not exist. Furthermore, as she underlines at a conference in 1948, "If we are dealing with a syllabary of the Cypriote type, we can say in advance that the signs will fall into a pattern that can be set up on a grid, so that all the signs in a vertical line will have the same vowel, and all signs in a horizontal line the same consonant"¹⁴. More precisely, she insists, when trying to decipher documents written in an unknown language and an unknown writing system, the first step consists of determining the facts found spontaneously upon examining the available documents, the second in discovering the deductions that can plausibly be made from these facts. And the distinction between facts and theory underpins all of her work:

"The basic distinction between fact and theory is clear enough: a fact is a reality, an actuality, something that exists; a theory states that something might be, or could be, or should be. [...] In dealing with the past we are concerned, not with something that exists, but with something that has existed. Our facts are limited to those things for the past which still exist; everything else is theory, which may range all the way from practical certainty to utter impossibility, depending on its relationship to known facts" [KOB 48, p. 82].

¹³ A. Kober, "Form without meaning", conference held at the Yale Linguistic Club on May 3, 1948.
¹⁴ [KOB 48].

Using these axioms, Kober studied the texts as objects of both language and writing, taking an interest "only", as she said to Myres (October 29, 1948), in "what the Minoans wrote".

The questions posed by the author are the following: what are the different signs used in the writing from the documents? What is the system of the ciphers? If ideograms are used, do they give any indication of the texts' content? What are the categories that can be used to classify the documents? Can words be distinguished in the texts? These questions, some of which had in part been answered by Evans since the discovery of the documents, are for the first time systematically formulated and they aim to build the research methodological framework. Using these data, it was possible, according to Kober, to move on to another level of analysis with the aim to theoretically understand the language.

Based on the known Cnossian documents, Kober established corpora of tablets with the same ideogram (first that of the chariot, then that of the woman) and, supposing that these texts all referred to this ideogram and thus dealt with the same subject, she concentrated her attention on groups of signs in order to see if the observable variations in their form allowed the particularities of the language's structure to be identified. In fact, according to Kober, if similar words appear in sentences with the same subject through variations that allow their common skeleton (two or three signs) to be recognized, then the visible variation taking place concerns inflection.

Her assumption was the following: the simplest way for a simple syllabary to note down a language consists of combining consonant value and vowel value in signs, which gives rise, in an inflected language, to a change on the final sign of each declined word. Certain signs would then be repeated in given positions in sign groups-written words as prefixes, suffixes, or infixes: *regardless of whether these variations remain obscure, as soon as they show up regularly and provided that enough material is available to conduct an adequately intense analysis* [KOB 45, pp. 143-144].

The same groups of signs were repeated from one tablet to another, sometimes in the same order and presenting the same final signs: these elements supported the hypothesis of an inflected language containing grammatical suffixes [KOB 45, p. 147]. And furthermore, as Kober writes in the conclusion of her study, the notation constraints specific to a syllabary and the fact that hypotheses on the language cannot be advanced *explain the*

gap the analyst must face between the occurrences that can be studied and the possibly corresponding theoretical forms [KOB 45, p. 150].

The way in which Kober illustrates her procedure draws attention. If Latin was written in a syllabary, like the Cypriote, and neither the language nor the writing system was known, the fact of finding two sentences where one contained the word *fecit* and the other *fecerunt* would provide no means of concluding that these two words are connected. Only the first sign, *fe*, would allow a parallel, and it would be necessary to discover that the signs *ci* and *ce* share the same consonant. Once this is established, it would still be necessary to establish the link between *t* and *rum* (they constitute inflectional variations), notably through the analysis of the regularity of their "alternation" in a rather large set of inscriptions. Kober's conclusion is striking: "The present state of our knowledge limits us to the observation of obvious alternations or additions in suffixal, prefixal or infixal signs; their significance, for the most part, escapes us" [KOB 45, p. 150].

Yet through the observation and analysis of variations probably owing to gender in the groups of signs in the tablets with the ideogram of the woman [KOB 46], Kober considers broader lists of words from all the known tablets. The variations thus allow her to discover the known grammatical declension known under the name of "*Alice Kober's triplets*". However, the explanation of *this variation, formal from an inscription standpoint and a morphological standpoint from the written language* also allows a supplementary step to take place and the graphic units that make up the groups of signs to be analyzed.

In fact, if the parallel theoretically established between groups of written signs and words of the language allows the behavior of the latter to be described at the level of grammar, grammar allows a theoretical parallel to be established between the final signs of these groups and the syllables that they represent.

Once again, the parallels with two known ancient languages are involved in illustrating the matter: Latin and Akkadian, which can in no way be considered to decipher Linear B. The Latin declension for the singular, for instance, demonstrates the following variations: 1) nominative: *ser-vu-s*; 2) accusative: *ser-vu-m*; 3) dative/ablative: *ser-vo*, and these illustrate the

particularities of an inflectional language concerning lexical morphology, made up of a radical and endings that change according to grammatical case. If the same endings are applied to an Akkadian word, like *sā-dā-nu*, for example, according to the syllabic division particular to cuneiform writing and to the standard transcription, this word presents the following form for each of the three cases: 1) nominative plural: *sā-dā-nu*; 2) oblique plural: *sā-dā-ni*; 3) nominative singular: *sā-du*. "If this interpretation is correct, we have in our hands a means for finding out how some of the signs of the Linear Class B script are related to one another" [KOB 46, p. 276].

Once again, and more explicitly than in the case of Latin and Cypriote, logic guides the examples that illustrate the linguistic "mechanism" of the tablets and provide a "means" of attaining it. Latin serves as a language while Akkadian, where the application of the Latin declension is only an artifice, takes the place of Linear B writing and, in its alphabetically transcribed form, makes visible what the Mycenaean syllabograms may hide.

If, as Evans believed, the B syllabary was a simple syllabary (exclusively recording open syllables according to the form CV or V), the ending signs that vary in the demonstrated case of declension should necessarily present modulations similar to those of the Akkadian model. Here, according to Kober, the third syllable constitutes a linking syllable between the radical and the ending; the consonant of this syllable can then be considered part of the radical, while the vowel is part of the ending. Kober applied the same morphological rule to Linear B, where only the vowel changes according to the variations specific to the declension and not the consonant. As the language has a visible morphology that is understandable in the formal variations of the inscriptions, also the phonemes, invisible within the syllabic sign, can become visible once their distinctive traits are identified through the relations they have to one another. The graphic units of the alphabet allow this translation in the Akkadian case, and the same technique can be used in the case of Linear B, despite all knowledge of the language.

The syllabograms are thus translated into "phonetic patterns" (Figure 10 in [KOB 48, p. 101]): *phonetic models* that Kober presented in a table (a sort of reduced grid) and that, while being distinct from the phonetic values themselves, allowed them to be characterized at a higher logical level as *types*.

This step is based on the three previous ones, and if structure (inflection), grammar (declension) and phonetics (phonetic patterns) do not confirm one another, the whole demonstration comes crashing down [KOB 48, p. 99]. In the opposite case, however, progression is ensured.

The results thereby produced constitute, as we know, the fundamental clues for identifying the Greek-Mycenaean language, but the method implemented led to another kind of result, certainly not negligible.

Firstly, Kober extends to all the signs of Linear B the treatment reserved to the groups of signs, presumed to be the only carriers of linguistic information. In doing this, meaning rejoins the criteria for identifying a language in Kober's process.

Second, based on what can be considered as the subject of the texts, she takes into account the words that appear alongside the ideograms, supposing that they have a semantic and contextual connection.

Finally, it is proven that, in a phonetic writing system, the words consist of common signs that are necessarily repeated, and Kober organizes the possibility for a connection between the statistical study of the writing system and that of the language.

Above all else, the criteria of comparability underlying Kober's analysis using ideograms produce a particular framework for the problem. The analysis goes through every layer of the written language in the documents, from the corpus of tablets to the groups of signs that make up their text, then to the signs that make up the groups. Each time, the sequences considered constitute distinct objects that correspond to a different adjustment of the analysis unit allowing them to be grasped, as well as the translations: the forms in the texts become the key to the language's semantics and morphology, then to its grammar and, finally, to its phonetics.

For the first time, the two classes of objects involved, the language and writing system, are defined in a parallel and complementary way by a transfer to a higher level in relation to the documents. This level being typological, it provides no key to the reading itself, but organizes and projects the adjustment between language and writing through and in theory, below all hypotheses. The type of language recorded by Linear B is thus

contemplated through an *assimilation of forms* and a *translation of plans*: the groups of signs, immediately treated as "words", finally becomes this through the demonstration. Kober thus constructed a *theoretical language*: if no bilingual text was available in the case of Linear B, her work consisted of establishing one. Hence, we must consider the particular status of the languages in the matter.

The languages and writing systems considered by Kober – among them, as we just saw, Latin and Akkadian, but also Cypriote – did not constitute hypotheses or options "for deciphering", but rather the means to describe the language of the tablets. Thus, it was possible for her not to choose a language, but to reveal the function of a theoretical language that, parallel to the written one, would reveal its salient grammatical and phonetic characteristics, working as a theoretical bilingual: a sort of *decontextualized Rosetta Stone*.

Nevertheless, the logical steps taken by Kober and the "facts" that are the written texts deserve great attention. They mean both the stabilization of the sequences examined and their interlocking. All the documents then represent the actualization of the Mycenaean language and, though they cannot coincide with this from the point of view of the ancient Mycenaeans, they necessarily represent it from the perspective of the modern analyst. The documents feature the *totalization of the possible linguistic occurrences*. Kober's outline of classification of the Chossos tablets [KOB 52] is thus the constitution of her field of study.

Unlike that of Bennett, always in use and in which the tablets are classified as archaeological vestiges, Kober's classification, which will never be considered in mycenology, is drawn up from a linguistic standpoint and classifies the tablets as texts. Based on the arrangement of the sign groups and ideograms, Kober establishes the framework for all her demonstrations and simultaneously engages the different levels of the text and of what it is meant to record in a unique system. Two different collections of objects and two distinct levels of a single collection of phenomena are treated there at the same time, and, in the same way, the nature, function and use of the language and writing among and by the Mycenaeans below the archaeological constraint of their understanding [CAR 15b].

The pattern of the script can henceforth be understood through the coincidence between writing system and language, and this coincidence only

will allow the demonstration to be carried out *after* the linguistic facts are proven. If, for her comparisons, Kober distinguishes languages from writing systems, the facts force her, on a theoretical level and from a logical standpoint, to treat them as a single thing: the language can only exist in the writing of the texts that transcribe it.

The *singular mechanics* of what the Minoans wrote is both logically constructed and empirically illustrated as *communication within* the tablets in Linear B.

The scientific references in Kober's articles are missing and no trace can be found in the archive documents, while her process could not be clearer. Two details, two coincidences, draw out attention: Roman Jakobson was a Guggenheim Fellow in the same year as Kober, 1946, and Leonhard Bloomfield wrote this surprising statement to Kober in 1944: "the very notion of your problem scares me".

In Kober's "science of graphics", the eminently philological question of Linear B and its decipherment become an *application of American distributional linguistics and a demonstration before the time of information and communication theory*. The "mechanics" of the inscriptions Kober works on corresponds to that of the hierarchical relations of the language in synchrony, but the linguist's means of description on the field are, in the case of Linear B, applied to an ancient, graphic and written discourse. This aspect gives Kober's translations a particular connotation, for they depend on the previous and implicit transformation of the language's domain into a range of signs, and finally, on the construction of a *code*. Furthermore, in the demonstrations and description outlined by Kober, we observe *the encoder's point of view*, for it is in the encoder's eyes that meaning constitutes the point of departure, while sounds are at the other end of the communication channel [JAK 71, p. 575].

The language unveiled by Kober is theoretical, not because the phonetic values are not given, nor through the emergence of inflection, grammatical declension and the behavior of the phonemes within the Mycenaean syllabograms, but because it refers to a comprehensive model in which the material and semiotic nature of the tablets' communication are all considered together and indicate a theoretical competence *ranging from the language to the written speech*.

The urgency for lists of "words" and Kober's interest in statistical calculations are clear, as are the divergences from her co-decipherers. Let us cite the question that Jakobson addresses to linguistics and communication theory in a conference communication from 1961:

"The amount of grammatical information which is potentially contained in the paradigms of a given language (statistics of the code) must be further confronted with a similar amount in the tokens, in the actual occurrences of the various grammatical forms within a corpus of messages. Any attempt to ignore this duality and to confine linguistic analysis and calculation only to the code or only to the corpus impoverishes the research. The crucial question of the relationship between the patterning of the constituents of the verbal code and their relative frequency both in the code and in its use cannot be passed over" [JAK 71, p. 578].

According to Kober, "figuring out the statistics simply involves learning a certain amount about the mathematics of probability and working hundreds of hours with a slide-rule"¹⁵. But the real question is that of the value of the data collected and of the type of statistics. Otherwise said, the problem is that of the question through which one counts. Logic and mathematics go along with the linguist's work at the same time that they account for the *differential* implied in the distance between Ancients and Moderns that the writing and script convey.

Let us then briefly return to the instrument of decipherment. Kober's patterns are not the same thing as Ventris' Grid and while they can be represented in the same manner, they could not be assimilated to it. The continuity between the two forms of tables has been foreseen on the historic and functional level [POP 99, POP 08, CAR 10]; however, the assumptions of its use diverge and, by virtue of this very divergence, seem to complete one another. Where patterns are the arrival point for Kober's demonstration and they stand for its conclusion, the Grid is the point of departure for Ventris' experiment: *when* the language and its characteristics, reduced and translated into their minimal components in the form of C and V, are

¹⁵ A. Kober, "Form without meaning", conference held at the Yale Linguistic Club on May 3, 1948.

mapped, the paper machine can then assume the mechanics of the documents, indeed going along the same path in the opposite direction. Despite the fact that Kober's work has often been neglected or misunderstood, the public perhaps was not completely missing.

3.6. Scribes' hands and autopsy of the tablets

Unlike Kober, who worked on copies of the tablets, Bennett examined the originals of the documents in a pioneering study on the signs traced and written by the scribes on the clay by starting what he defined himself as long "paper work" [BEN 57, p. 555].

The systematic paleographic and epigraphic study focused first and foremost on the Pylos material, then on that from Cnossos, and consisted of filtering and sieving the Mycenaean doodles. This is an inscription and referencing work that concerns both writing as a graphic notation system and script as a set of the occurrences of the artifacts that are the signs. According to Bennett, it is a matter of copying, checking, listing, extracting and composing signs again and again.

No publication accounts for this work, which is mentioned here and there in mycenology articles. Bennett references it in the introductions to his essentially technical publications, the corpus of inscriptions he edited and the index he established. This work on signs only constitutes a preliminary step to the treatment and philological analysis of the texts: the condition necessary for the study and a highly specialized technical means rather than a target. As such, it is a matter of expertise rather than scientific research *per se*. The training of the epigraphists and paleographers continues to echo this, if only as a reminder that no treaty could take the place of the practical and concrete relationship with the material. According to Bennett, it is a matter of "skill" acquired without any conscious effort and that can be drawn closer to that underlying the recognition of the personal handwriting of our pen pals: the eyes and mind use objective criteria to proceed; these same criteria are difficult to objectify. In reference to our writing practices, Bennett compares, for example, the different shape the numbers 1 and 7 take in Europe and the United States, leading to confusion [BEN 67, pp. 475-476]. Through this tacit knowledge, the facsimiles of the texts and signs are formed.

Starting from the list of signs and changes that can be observed throughout the publications, we can finally clarify what is born to remain implicit and what generally remains implied: the back kitchen of decipherment.

We have seen that the problem of the list of Linear B signs remains until 1952 concerning the Cnossos tablets, for which no other source was available but that of Evans. Moreover, Evans himself points out that the copy of the documents that he had drawn needed revision from the originals, but that the full documentation would have been published (too) quickly, also due to the fonts that Oxford University Press was already preparing. They were casted, but never satisfactorily: these are recurrent problems concerning the reproduction of signs, except that at that time, another issue arose: for what characters was it better to cast types and matrices? With mediocre results: most of the time, the types made available showed themselves to be variants of a single sign and the successive discoveries only increased the number of signs to be considered, all the while jeopardizing the categories proposed by Evans.

For the Pylos tablets, on the contrary, Bennett worked from the very start with photos taken before the war. Then, for the 1951 publication, through the original documents that he made facsimiles of. He furthermore underlines that the facsimile only shows the essential elements of the document, to be supplemented by photographs, drawings and descriptions of the tablets [BEN 51, p. IX]: this passage concerning the "transcription" of texts that were not yet understood nor read helps assess the importance of the documents that the tablets constitute as clay sources. *To copy the signs, and even to "see" them, they must be discerned.*

This requires what is called an "autopsy": at the same time, reading, recognizing and processing the tablets and plotting the writing. No reproduction of the tablets being feasible, contrary to other ancient inscriptions, on other, more manageable supports, it is with a toothbrush and a magnifying glass, trying to see what photos and the drawings could not show, prudently manipulating the clay tablets and the fragments, that it was possible to make visible the written marks that would have otherwise remained hidden in the clay that envelops them, and that, together, form the text [BEN 53, p. IV].

This expertise focuses on both the writing as a conventional system of signs and on the text as a set of traces left by the scribes. However, the relationship between the material characteristics of the documents and the texts that take shape there is not self-evident: seeing the written signs and knowing how to verify and reproduce their shape does not mean distinguishing a writing system or rewriting it. Thus, Bennett's work provides access to a paleographic definition and a functional definition of writing, with the chance to observe their nascent reciprocal relation.

The thousand tablets found at Pylos included the longest and most complete texts known in Linear B; these texts, written by more than 30 scribes, feature various handwritings. This means that in light of these, once the tablets are cleaned and the graffiti made visible on their surface, all the written Mycenaean marks cannot be determined as a repertoire, for each sign appears in as many different shapes as the number of scribes: selection and sorting criteria must therefore separate the handwriting from the writing system.

If 20 scribes write the same sign, there will be more variations than if one scribe writes a sign 20 times. The provenance of the material to be analyzed from a single site helps define the graphic items and the conditions for their production on an archaeological basis. In light of this, Bennett focuses on the lines that form the shape of each written sign and its material appearance.

The method consists of considering the simplest forms or the most evident similarities and in comparing their variations: the added or subtracted traits, for instance, but also the modifications of the outline. Scribes also serve as a reference: when one is recognized, the uniformity of the signs is revealed and no longer leaves doubt concerning their identification. For this reason, Bennett explains, it is better to risk a false distinction between the variants of the same sign than to assimilate similar shapes of different signs by confusing them [BEN 53, p. VIII]. Thus, he established series of written signs to produce only one repertoire using what he calls their "history" [BEN 67, p. 481]. Nevertheless, certain signs do not provide information and seem to be devoid of "history": the analysis then stops before signs whose shape is very simple and lacking in traits, while other signs do not show any history due to their rarity or a unique origin (the same scribe).

The line, curves, depth of the mark, size and proportions of the signs on the tablets allow a scribe to be identified through the *ductus*, that is, the shape that the gesture takes in the clay, as compared to others: these details constitute a signature – the word "hand" is also used – and it is through the reduction of hands and the synthesis of gestures that the signs emerge. The scribes' hands thus compose the real and archaeological occurrences of Linear B writing: through them, we obtain *the* sign of the repertoire, the theoretical and never perfect model that the Mycenaeans wrote, in both a modular and modifiable way.

Another aspect leads to a form of reference for the autopsy: the graphic units, which guarantee the coherence between the signs and scribes on the level of the writing system through the nature or function that can be recognized in them.

According to the occurrence of the signs, isolated or in groups, according to the available space on the tablet and to its format, and according to the text line being used, the shape of a sign changes: the scribe's movement follows these constraints and adheres to these writing conditions, surrounding in the *ductus* of each sign the graphic environment it belongs to. As we know from Kober's work, we find the reference to the set of signs constituting a group as a necessary criterion in establishing morphological variations: two or three signs must be identical for two sign groups to be compared and analyzed. Furthermore, in her study of the "chariot tablets", Kober also insists on the differences of traits between certain signs of the "words" and emphasizes that her predecessors, not having paid attention to the variants, had naturally added or subtracted details in their copies of the inscriptions [KOB 45, p. 146].

The "history" of the signs unfolds in an environment, and, alongside the scribes' hands, the text must also be considered as a set of graphic sequences insofar as it allows the connection of the graphic units that constitute it with the functions they have vis-à-vis the inscribed message: these elements are the basis for the organization of the repertoire through the scribes' graphic uses.

Between the scribes and their texts, a number and form are attributed to the signs of Linear B: such as Bennett's repertoire of the Mycenaean writings, where the series of syllabographic signs is made up of their shape (from the

simplest to the most complex). Another aspect of his classificatory work must then be highlighted, concerning the technical principle at work.

The reduction of the scribes' act of writing, wherein lies the origin of the sign list, also leads to a translation of the archaeological reality of the documents. The calculations of the frequency and occurrences of the signs are the basis for establishing the list, but only as they take shape in the *index*, which Bennett prepared throughout his "decipherment" and which was only later published, in 1953 [BEN 53]. This is a particular volume, eminently epigraphic, which inventories the signs starting from the groups that they form and from the signs that precede, or follow, them, and whose goal is to situate the signs as they are used in order to clarify their relations.

The hands-on approach of expertise requires "grips" and those are found both in the text and the writing system. This emphasizes the transition from the shape to the function of the signs, then the transition from the scribe's gesture to his competence in the language and writing. Bennett establishes three categories:

- signs used in groups of signs;
- ideographic signs;
- signs for numbers and punctuation signs.

If the first can only be the object of a true decipherment in that they constitute phonetic signs and that the third created an immediate and easily verifiable recognition, the distinction between syllabic and ideographic signs was not evident. For Evans, with his evolutionary theory of writing, the function of the signs is given by their shape; for Bennett, on the contrary, the function of the signs comes from the textual environment to which they belong and is a synthesis of their shapes: *the results of a gesture and the actualization of the ability to write texts*.

The categories are also defined in relation to the visible organization such that the signs are distinguished, on the one hand, as material elements of the text – including the space that separates them – and, on the other hand, as functional elements of the text. An example can illustrate this idea. In the table of ideographic signs (that appear isolated on the tablets), we recognize, most often in relation with the numeric signs, the sign groups *ko-wa* and *ko-wa* [BEN 47]. In these two sign couples, three signs are associated with

distinct phonetic values (respectively, *ko*, *wo* and *wa*) that will allow the words "boy" and "girl" to be read. Bennett considered them as ideographic and isolated signs due to the accessible logic of the occurrences in the texts. Thus, the signs that form these groups appear both in the category of ideographic signs and that of the syllabic signs used in groups. It is then the texts and their organization, comprehensible in and through graphic space and through the written signs, that operate in the definition of the typological categories of signs and in the definition of the formal characteristics specific to their arrangement [CAR 15a].

Bennett's work allows us to observe how the signs and scribes are distinguished and become independent but complementary objects within the analysis: on the one hand, the scribe assigns properties to signs and characterizes their shape through a particular *ductus*; on the other hand, signs allow characteristics to be attributed to the scribes, who only exist with and at the same time as the written signs. It would be impossible, however, to conduct a systematic analysis on these two fronts so that certain elements of the text, considered to be outside the system, are thus regarded simply as a way of writing rather than Linear B itself. The elements that the autopsy has allowed to make clear and that are "preliminary" to the Linear B writing *per se* and to be deciphered are thus gathered in the Mycenaean signs inscribed in the boxes of the Grid. Here, they are gradually distributed by Ventris, following his hypotheses and changes, by means of a pen and through his precise and singular hand, which necessarily references Bennett's hand and those of the Mycenaean scribes: which in mycenaean is called *modern and regularized Mycenaean writing*.

However, if one and only one sign occupies one and only one box, the list as well as the Grid presuppose that they will never be considered isolated objects: after suppressing the scribes' ways of writing and the archaeological characteristics of the tablets as background noise, the text – as the context and environment of the signs that makes them emerge – remains the shared object between the ancient Mycenaean and the modern decipherers. The Grid is not just a theoretical-phonetic device for organizing the signs in relation to the language, but also the result and possibility of the signs' arrangement within the text, vis-à-vis the scribes who traced them, as well as the attentive eye of the analyst, and his hand that follows it.

3.7. Technomycenology

The differentiation between the language and the writing lies at the heart of operations. During the decipherment, only their possible and probable virtual and factual "junctions" exist. These initially connected terms can only be considered independent objects after the fact. It is exactly this relation that the Great Controversy focused on, and it is through the same relation that attempts have been made to provide concrete responses: to two unknown variables, an unknown and a half, plus a Grid. If a distant glance makes science and technique discernable, their proximity blurs the boundary and the difficulties of the analysis increase once we get closer to it. *How can we describe the overlap between the process of substitution and translation and the simultaneity of the emergence of language and writing? And how can we account for the coincidence in which they take shape?*

The observation of the decipherers at work allows us to understand that substitution takes place through the deconstruction, segmentation and extraction of the writing system from the script, on the one hand, and through construction, recomposition and inscription of the linguistic elements on the other. From clay to shapes, to sounds, with a brush or a pen, through letters, holes or tables, through the eyes, calculations, or gestures, the practices and the materials that characterize this process bring to the foreground a similarity of tools and a common goal at the same time as a difference in approaches and, finally, objects: beyond the language and writing system, it is the signs and groups of signs, the words and sentences, the accounting records from the palaces and the texts, the Mycenaean scribes and speakers that were the objects to be dealt with. These elements reflect the interdisciplinary and applied character of the collective of decipherers and show that internal analysis was *necessarily* combinatory and *conjuncturally* cryptanalytic.

Facts and theory, hypotheses and technique are distributed on the terms of the relation to be grasped, discovered and invented. In this "back and forth", we can observe the relationship that is woven between the decipherment of Linear B and cryptanalysis. More precisely, it unveils the relationship between archaeology, philology and the Grid, and it shows how techniques and sciences are engaged even within the gears of the machinery implemented as well as the past and present discourse among mycenologists.

3.7.1. Cuts of pertinence and relays, frames of reference and hierarchies

The cryptographic frame of reference within which decipherment has been presented, on the one hand, and the distinction between the preparatory phase and decipherment itself, on the other hand, reveals a peculiar epistemological purification: wishing to respond beside the point, the defenders of the decipherment and its historians finished by making the question even more pertinent.

By closely considering the work of the decipherers, we can understand that the presentation of the decipherment and its "rest" illustrates the manifest *hierarchy* established since its completion. It is first and foremost designed around disciplines and objects; in the second place, vis-à-vis the cryptanalytic framework itself and the method implemented by Ventris.

The concerned *fields of competence* – archaeology and linguistics – and their respective *responsibilities* concerning the inscriptions, signs and their reproduction, on the one hand, and their analysis, on the other hand, had been very clearly highlighted by Kober in 1941, with regard to the *faux pas* made, the traps of a hasty interpretation and the reciprocal lack of understanding concerning the *research questions* and the *concrete problems* posed by the object of study: "The blame must be equally shared by archaeologists and linguists, who too often work independently, and without understanding one another's problems"¹⁶. The success that crowned the effort has certainly reduced the urgency of this question, nevertheless familiar to all mycenologists: access to the language, the aim of operations, is seen as a true knowledge exploit, the discovery and heritage of decipherment within knowledge, while the writing constitutes its invention on the field of a sort of textual excavation. This distribution will henceforth concern the whole process, notably affecting the object-goal and analysis-substitution series in the explanation given of it.

The founding role that the decipherment had in the birth of *Mycenaean studies as an autonomous and multidisciplinary field of knowledge* focused on philology and the study of the Greek-Mycenaean language, and also at

16 A. Kober, "Some comments on a Minoan inscription (Linear B class)", presented at the meeting of the Archaeological Institute of America, December 31, 1941 (unpublished manuscript conference).

the same time that the study of writing went into the background, must not be underestimated. The first Mycenological colloquium, in Gif-sur-Yvette (Paris) in September 1956 [LEJ 56] where a cohort of linguists, presided over by Michel Lejeune, convened and where only Bennett represented the archaeologists' side, attests to a clear division of the tasks between the scientific and technical domains with a view to study the material, philologically reconstruct the language and educate a new generation of mycenologists.

Yet neither the stages of the analysis, the disciplinary affiliation of the results obtained nor the compartmentalization of the issues raised, respectively, in editing or phonetic substitution could, in isolation, account for the orientations of knowledge at play and the relations made. If, for Kober, Bennet and Ventris, these distinctions formed the *sine qua non* condition of advancing the work, this is because their work also consisted of testing their impermeability in order to cross the borders into a *hand-to-hand relationship* with the documentation and a *knowledge-to-knowledge relationship* that implied them. The multiplicity of the objects lends meaning to the segmentation of the decipherers' work and in parallel accounts for the problem that the thereby implemented interdisciplinarity poses.

What framework, what discipline, what decipherer is the question's master? Who assumes the technical support role? And what connections can be identified between modes of knowledge and types of data? While at first it may seem easy to outline a response, for instance in following the language-writing and discovery-invention series, we are quickly disillusioned as we progress in the study. The composition of the probable arrangements is far from being stable and the decipherers borrow one another's questions, as well as their tools and work objects. The discussions in their correspondence take turns highlighting the adjustment of the positions and the explanation of the respective questions, the search for a solution for the divergences, the attempt to convince one another, insisting on the incongruities of the standpoints just as much as the chosen techniques and frameworks.

If Kober's work frightened Bloomfield, Bennett's contradicts Gelb, for whom epigraphy and paleography are branches of linguistics only in the alphabetic tradition [GEL 63]. Both support one another mutually in their

analysis and in their interrogations without the technical mode and the scientific mode being recognizable in the analysis of the writing and the language, respectively. As for Ventris, it was not just with a Grid that he opened the lock, but with several keys. Finally, Chadwick's collaboration with Ventris, once substitution had taken place, joins this matter: the philologist scientifically sanctioned the decipherment in the field of ancient languages at the same time that he played a properly technical role in Ventris' discovery and concerning the recognized Greek language. Among the decipherers, *the philological transparency becomes opaque, the archaeological hardness flexible, the cryptanalytical mechanics questionable.*

We can thus identify *relays* through which technique and science are alternated and distributed. The units of analysis and the graphic and linguistic units considered show the cuts made on the documentation and on the problem to be resolved, and according to the frame considered, they create a *different pertinence*. The signs and syllables, groups of signs and the words or nouns, the lists and sentences, the marks and the hands of the scribes, the tablets and the inscriptions all distribute the same Mycenaean reality on different levels, which could not avoid *reciprocal interferences* and *irreducible convergences*. The text, product of the Mycenaean scribes, is the object for distribution between what belongs to the language and what belongs to the writing. What is more, writing enables the passage from the archaeological frame to the philological frame and allows the signs established to be questioned on the basis of their shape and function. Finally, the language strikes the final blow: the script, on the one hand, and the notation, on the other. Linguistics and philology, archaeology and history take their place as the objects are put in order.

Indeed, these same relays can also be found in the current practices of mycenologists who, since Gif, have been updating the questions that were formerly those of the decipherers and reorganizing them. Although the decipherment has inaugurated a new season of Aegean archaeology, and the scribes have never been an end *per se* [PAL 11], the patron spirit of mycenology, the so-called "*esprit de Gif*", actualizes both the foundation and the myth of Mycenological necessary and specific interdisciplinarity, by reminding of Ventris' approach, at the occasion of every colloquium, which takes place every 5 years since 1956. However, the relationship between technique and science still seems to be strangely polarized. The reading of the corpus of tablets and inscriptions with the help of 3D technology and

new digital reproduction and diffusion devices brings to the forefront the traces and the supports of the scribes' ancient "making", whose noise (or interference) was removed during the decipherment since it did not directly concern the language and its history. Bennett's expertise finally managed, outside the decipherment of Linear B but within Mycenaean history, to be the frame for new scientific issues between archaeology, history and agency. Would it herald a Mycenology 2.0?

To that extent, the term "archaeological decipherment" acquires a double pertinence. When paired with its cryptanalytical twin, it would only seem to refer to the nature of the code and the message to "be discovered"; to the contrary, it bears the technical dimension of the knowledge and scientific dimension of the procedure. What is more, it accounts for the very equivocity to which the two modes, technical and scientific, can give rise. In fact, as soon as the work on the language and writing is considered an essential assumption for the decipherment, to not be confused with the decipherment itself, the Grid can take care of the event. It establishes a coincidence between the process, the substitution and the steps of its implementation, which could henceforth constitute only gears of the mechanism: in its cogs, the specificities of the objects to be known are lost just as much as the particularities of the modes of knowledge that are engaged.

The Grid guides thought by featuring a *formal mechanical synthesis* and an *efficacious synthetic formalization*: its goal was to collect and distribute a cloud of scattered knowledge; through its spatial organization, it kept this knowledge in position all while guiding the efficacy of the decipherer's questions. The history of this controversial or misunderstood instrument is that of the different frames of reference that alternate, overlap, fall back on one another to finally converge toward the solution: one language for one writing system and one script.

If it arranges and systematizes the information into inputs and outputs, supporting their combined emergence, the Grid also reflects the exhaustion [DEL 92] of possibilities as they were unveiled by the analysis and embodied by the collective. In particular, once attention is paid to the frames of reference and the relays of knowledge installed, the knowledge process gives way to a *sieving* [FRI 02, KOC 13] that invests all the documentation

and at the same time all the knowledge as "ready-to-use" resources or potential "grips" [BES 95]. Grid-machine, Grid-table and Grid-sieve: at the same time that it carried out the decipherment, it also established the decipherers' place and the story to be told: that of a technician taken for a scholar, a scholar taken for a technician and an expert-scholar who then played the role of safeguard. Stakes of the collective, between science and technique.

3.7.2. *Prince Charming and the scribe of Minos*

To be "scientific", the decipherment had to be "technical". The figure of Prince Charming who, with a magic kiss (or a grid), wakes the sleeping beauty perfectly represents the situation: Björck's presentation at the 1954 congress was right. However, the Grid, which seems to have pushed the decipherer to the periphery of the process while making its work into an exploit outside the historic sciences, only partially accounts for the "technologization" of the decipherment. If the established hierarchy maybe gave the prince a horse, it also cut the cryptanalytical explanation from its frame. A flying carpet would have then been needed... and another story. Yet it is all there and concerning the process and orientation of knowledge, a prince would not suffice. Let us say it in a different tone than the one Kober used to define Ventris' work at Oxford: it is much like a scribe that Ventris deciphered Linear B.

In fact, the Grid was able to work as a deciphering instrument only because of a known, spelled out, and thus recognizable language; from then on, to consider a completely ready-made substitution, even to suspect the construction of a template using that same language, there is only one small step. Ventris himself, as well as Kober, were perfectly aware of this, and they both emphasized this "trap". But there was no need to look far for a response for the critics. The applied, collective and interdisciplinary work of the decipherers provides the elements necessary to explain the epistemic foundations of the *cryptanalytical reason* of the Linear B decipherment. If Ventris succeeded where a generation of scholars had failed, it is because of an *unprecedented and unimaginable alliance between archaeology and philology*. This could generate the frame within which a cryptanalysis was possible in an archaeological decipherment.

The operations involved, a known language and writing, and a secret code constitute the terms of the modern situation, while the gap and distance are the condition of the decipherments of ancient languages. The Grid assembles the data elaborated by the collective and represents them: the scribes, the speakers, the signs and the documents are in its boxes at the same time as the calculations that served to process them. The substitution carried out by the Grid was possible because the equivalence between sign and phonetic value was supported beforehand by internal and statistical analysis where the nature, function and use of signs were inscribed and translated into calculations, then letters. Except that, if this is evident in cryptanalysis and in the decipherments from Moderns to Moderns, this is not the case for Antiquity, where the distance "hardens" in the objects to be known.

The terms of the situation at stake allow us to grasp the knowledge orientations, matrices and modes, as well as the frames of reference, through the *dimensions implied* in the operations and that concern the research objects as well as the decipherers' positions. In this perspective, the substitution gears become the means of *synchronization*, and the distinction between the conception and application of the Grid – especially between Kober and Ventris, and leaving Bennett outside the decipherment yet at the middle of the analysis – brings the issue of *learning* and *appropriation* to light.

For Bennett, scribes and writing correspond despite the variations found in the shapes of the signs. Coming as close to the tablets as he can, the archaeologist, epigraphist and paleographer remains far from the actual agents of writing, those others who are the Ancients, whose uses he copies and whose technical competence he standardizes. The language system, on the other hand, allows a coincidence between the modern agents and the ancient ones, and this is based on the previously established assimilation between language and writing. It is on this assimilation that Kober founds her phonetic patterns and that the use of language attested in and by the texts can be translated into a theoretical language: neither abstract, nor hypothetical, but above the particular language of the Mycenaean. Finally, the distinction between language, writing and text for Ventris, as his experimentation shows, bring out his participation in the procedure: beyond the shape of the signs, the elements of the graphic system and the structure of the language, the administrative purposes and functions of the texts must be taken into consideration. Once the substitution has been realized and the

Greek language recognized, the decipherer Ventris will then need Chadwick, scholar or technician, who will protract this same distinction principle but through another correspondence, this one to the alphabetic tradition. From this moment on, we have *two languages, a scholarly writing system and a Mycenaean script*.

From the nature of the data produced and made available by the analysis, we can now turn our attention toward their function. The mechanics of the language and the autopsy of the tablets constitute the possibility to transform the domains of the ancient language and writing into models and parameters involved and make it effectively possible to grasp the language and writing of the ancient Mycenaean as a *code* and as a *cipher that the machine, then, can decrypt*. Kober's description allows us to go beyond interpretation by establishing the encoder's point of view: from there, the *decoder Ventris* is perfectly capable of following the path that goes from sounds to meaning, up to the proofs that confirmed his reading. On the other hand, the signs of Linear B, according to the model established by Bennett, allowed us to go beyond the text and the script and constitute the Linear B the *decipherer Ventris* will tackle. In this perspective, the list that catalogues the signs, the index that situates their use and the corpus of known inscriptions as language in use are not just instruments for editing, but the very basis from which to start counting and thus decrypting. In fact, the frequencies do not only depend on the identified units, but also on their position and on the distinction with the occurrences in the texts. The statistics and archaeological and philological "technical tools" point out two facets of the procedure, particularly when only the first seem to belong to the cryptanalysis, the second rather show the empirical correspondence for it: they are *the archaeological and philological form of the cryptanalytical situation*.

These elements have a major implication in the very definition of the decipherment. Ventris' cryptanalysis certainly is not based on an empathic or pragmatic internalization of the situation, as would be the case of a decipherment from Moderns to Moderns, but surely on a *computed internalization* thus assembled in the Grid for the communication context, the scribes' competencies and the functioning of the language are contained and represented in the columns, the lines and the boxes of the table.

A real device in the hands of the decipherer, the Grid contains and assumes the functioning as well as the ancient use of writing and language, and the synthesis that it performs pre-empt the historic scenarios in which

the scribes, ancient speakers of the language, stood in the limelight as the point of departure for all hypotheses. The Grid thus becomes the instrument of a *simulation* as a knowledge process halfway between science and technique and the form of a project and a program at the same time.

The interdisciplinary process, applied and dialogic, which takes place with group working and where the collective is concretely installed within the decipherment, and the Grid illustrate one another. By allowing "back and forth" with the documentation, the Grid institutes methodic circularity in which the combinations of knowledge can be exhausted. It is particularly because of this circularity that a synchronization is effected in parallel to the emergence and superposition of the language and writing: with the structure of the table, the letters of the alphabet, two techniques and two convergent applications that short-circuit the gap between Ancients and Moderns and show a form of appropriation that allows the distance to be overcome.

In this aspect of Ventris' work, we also find the particular meaning of the simulation implemented. Just as a child does not learn language and then speech, but both simultaneously, Ventris, unlike the other decipherers, learns the writing of the Mycenaeans.

The ongoing appropriation and learning are not revealed through a theory, but in its object. Ventris dissociated writing and language, not *a priori*, by separating the theory from the analysis and the thesis to be demonstrated, but during his effort, and finally, "on the job". The internal analysis conducted on the documents and the internal analysis within the Grid correspond and allow the responses the decipherer could not find on his own in his ordered questions to be sought from the outside, through comparison and analogy. More than a theory, it is a specific technique that we find then, with, more particularly, the link between two distinct techniques: that of the technician who is aiming for a goal with pre-established operations, and that of the "skilled person", who "makes" from the inside, like Ingold's artisan [ING 13].

Ventris' *technè*, just as much as his *métis*, spread across the humanities and prove to be a matter of knowledge and know-how. Scientificity and technicity are thus repositioned by the Grid itself, mixed with regard to the sciences of Antiquity and their objects *vis-à-vis* decipherment operations, and finally one taken for the other: *what has been presented as being more scientific was in fact the more experimental, and where there was a desire to*

see a theory, the technique arises again not as a procedure that is applied and which pins to reality, but as a competence at work in the flow of deciphering like in that of the ancient flow of writing.

What place, then, is left for the *intuition* or *art* of the decipherer's genius? If Ventris first situates those aspects outside his meticulous practice, always wanting to reduce and filter them, the question of their relationship with the simulation, on the one hand, and with the experimentation, on the other, still remains. It certainly concerns the "problem" of the interpretation and the comparative approach by which his predecessors had tried to decipher Linear B; however, it also heralds the work of the mycenologists, readers and writers of Linear B [CAR 10] who will succeed him, by projecting the science/technique relationship of the decipherment toward a broader horizon whose sociological and epistemological implications concerning the frames of knowledge engaged or initiated cannot be ignored.

As Clarisse Herrenschnidt writes, simulation is older than computer science and while not specific to it, it becomes "one of the most surprising modes of knowledge without experimentation, for designing things, and for educating individuals that computer science has produced" [HER 08, p. 389]. The comparison between the Grid and Turing's machine could not be restricted to an analogy. The humanist and historic area of the decipherment of the Bronze Age Mycenaean writing and language questions the relationship between science and technique and their rivalry in what is most modern and current about it, particularly in the way that these two modes are implemented.

In his essay, Pope observes that in the hands of a priest from the Middle Ages, the Rosetta Stone could have been just as useful as the photograph of a motor car in the hands of a skillful Roman engineer [POP 99, p. 10]. What would it be like with the complete alphabet and a table in the hands of a "scribe of Minos" teleported into the 1950s?

Conclusion

Confrontations with Confusions: Stakes to be Re-established

A sneaky agitator will be tempted, upon reading the contributions assembled in this work, to suggest an *a priori* concise, but immediately ambiguous conclusion: *everything is fine so long as technicians know how to play their role*. The proposition offers itself up to two diametrically opposed interpretations. One advocates the *limitation of ambitions* by the watchful authority that scientists exercise from above: technicians must not cross the boundary of their duties in any case, the scientists that they "assist" having the responsibility of monitoring the border. And every sociologist knows that this "snubbed" vision underlies a great deal of established research, even when its expression is tempered by a luxury of precautions. The other translation considers that, for technicians, "playing" their role means, above all else, protecting a responsibility in a field of acquired knowledge by means of surveillance that they, in large part, must exercise: the *refusal of abusive intrusions*, inseparable, in the long term, from an invalidation of extrapolations. And all epistemology should recognize that the degree of attention obtained by this "defense" mobilization conditions, in the medium term, the proper execution of the activities undertaken by a discipline.

Without the anchor, or "raw material", of stable technicity (even if only methodological), science loses the means of distinguishing itself from philosophy. However, the confusion is not unanimously perceived as a threat, quite far from it, because it tends to exempt certain "soft" sciences (concerning their intentions, if not concerning their aptitudes) from a practical *responsibility* in their field before society: their *raison d'être* would be limited to decorating "general culture", nothing more. Technicity has, in

effect, this inconvenience of inferring knowledge well enough proven to be applicable: an encumbering ability to act follows. Inversely, denying all continuity between technical updating and scientific inquiry is tantamount to dismissing the eventuality of a dialectic between technicity and scientificity: thus, to move towards a representation of science as a belief steeped in know-how, but otherwise, "neither better nor worse" than another. The demand for "responsibility" would thus express an awful temptation by that belief: an imperious, even imperial aspiration. A paradox appears by bringing about the resurgence of the hiatus between the technician and the scientist, in which the former just could not cut himself off from all responsibility: he must accomplish a certain task, and not doing so would risk the failure of the research task weighing down on only his shoulders. The incompetence of a scientist is discussed circumspectly, and that of the technician is proven with severity.

This is because it is rather easy to define a state of technical knowledge (the artisan, athlete, accountant, etc.) or a state of cosmogonic/ideological knowledge so long as science does not rely on it. However, once it joins the protagonists, technical and ideological knowledge can no longer be understood in a static existence. Technique, ideology and science, then, are each discerned in relation to the others in the variable dynamics that must be understood each time through their particular contexts. Without ever forgetting, above all else, to distinguish the *professional categories* (technician/engineer, researcher, etc.) from the *functions exercised* (the researcher basing himself on the technical procedure, the technician "going beyond" his field of activity, etc.). Only a permanent confrontation will lead, for example, to the discernment of the combination realized by a geneticist between his scientific identity and the economic weight of cutting-edge biological techniques when he tries to impose extrapolations that secretly arise from the ideological knowledge. The same is true in neuroscience.

As for the disciplines attached to dissecting the conditions in which "rationalist" knowledge is constituted – the epistemology and sociology of science at the top of the list – they will not develop efficient cooperation, or any dialogue, so long as the technique/science relationship remains buried in opaque earth. Let us think, for instance, of those recent publications in physics that respond to *one* question, but that are signed by *thousands* of contributors: should we resign ourselves to reducing these extensive

collaborations to a hazardous magma where the chain of operations would do without analysis?

A hypothesis emerges that we will happily whisper as an eventual "path", pending more precise reflection: *moving but perpetually reiterated tension between social hierarchies and epistemological complementarities could bring the technique/science relationship in its whole to life.*

This volume pursued one primary goal: to show that *the relationship between technicity and scientificity, as soon as it is questioned instead of being skirted around, shows itself to be omnipresent in the sciences, extremely diversified among them, and very adjustable throughout the phases of a research task.* The authors obviously do not assume that they have presented a panorama of the question, but the "quadrilateral" of our standpoints covers a rather broad range and delivers a sufficient multitude of textbook cases to demonstrate the expanse of problems long since left in suspense by highly dominant issues, resolved to "skip over" this old obscurity.

An enormous field of study has undergone – and still endures – prolonged abandonment that gives rise to perplexity. More than ever, Bergson's *Homo faber* shows an underestimated dimension of Man. Throughout the past century, the dominant jousts at the subject of the singularities of our species have fervently pitted biological determinations against mental emancipations, as if it were absolutely necessary to provide a priority for the competitions between the organic and the symbolic at the "heart" of Man, at risk of – or to? – relegate the intricacies of the practical relations between men and their material environments (including other men) as incidental. Genuine ideological squinting leads us to believe that evoking the intrinsic role of technical acts is as indecorous between science and philosophy as incongruous between genes and signs. Besides, are not these two hostilities closely tied?

The aberration of this chronic amputation should come to light when we stop looking at *one* science to consider the diversity of fields of scientific knowledge, with the difficulties that they face in exchanging information and ideas. Each time, the presence of defined technical support conditions the success of dialogue, just as its evaporation forbids it. Roughly outlined here, the opposition of two textbook cases summarizes the issue.

On the one hand, archaeology. Multiple specialties accumulate on an excavation site: palynology, zoology, paleoecology, taphonomy, technology, etc. Each incorporates scientific investigation that belongs specifically to it, but first reaches the results from the work site in the form of technical contributions that a responsible party, chosen in advance, would have to gather. The synthesis does not necessarily take place peacefully, and disagreements sometimes emerge: due to contradictory data (concerning dating, for example) or for more personal reasons, deontological or otherwise, through which the nature of the conflicts rarely leads to confusion, even when professional friction is added to a point of interpretation, and there is no doubt that interdisciplinarity has been considerably improved in this field over the past 50 years, because of both the individual progress of the technicities engaged and their practical combinations.

The social sciences and ecology, opposite one another over the same period of time, have proclaimed the urgency of interdisciplinarity that brings them together. The expression of this pious wish has not moved an iota when we realize that most of the great confrontations enticed here and there have encouraged representations and conceptions in the absence of foundations or technical support. This is in no way surprising if we realize that these sciences of interactions and interdependencies recover more or less intrepidly from outside technicities, but by creating very little by themselves. That said, we will notice that they have constantly avoided interacting at the level of the elementary techniques that their identity depends on and that they nevertheless share: the methods of observing, indexing and comparing. Purely theoretical interdisciplinarity too often turns into a fiasco for us not to pose the question of a technical flaw.

The four contributions found in this volume, beyond the disparity of their objects and approaches, have all shied away from this point: a proven technicity not only modifies the discussion with one science; it also transforms the dialogue between the sciences. Without including that it sometimes has to intervene from a distance between technicians and scientists who had not always seen it coming. The difficult and moving distinction between technical *achievements* and scientific *advances* then seems to assign a condition *sine qua non* to the exercise of interdisciplinarity that this brings face to face with continuous or, on the contrary, distant fields. The sciences recognize one another, even perceiving one another, because of the common points of reference conferred by technicities

developed by one of the areas of knowledge faced, by several of them, or sometimes by others.

In this sense, it is not trivial for a specifically "technical" frustration on the fortunes and misfortunes of interdisciplinarity to have arisen as the fountainhead of this work.

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