

From the Temptation for Purity to the Necessity of Unity: The Anthropological Sciences Put to the Test of Interdisciplinarity

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All scientific disciplines go through periods of self-analysis. However, this analysis often takes place as if inside a closed arena, from which it is difficult to gain a long view. Instead, in hopes of identifying the properties that define it, each discipline examines its own features in a mirror that it holds up to itself.

Yet there are other times, perhaps less frequent, when these self-evaluations are accompanied by an inquiry into the extent to which the discipline's intellectual characteristics are shaped by its relationship to other disciplines.

When the journal *Annales*, in a recent issue,¹ called on historians to engage in introspection, it set at the heart of this inquiry the relationship between the disciplines of history and the social sciences; that is to say it, its focus was on interdisciplinary activity. For the anthropological sciences, this endeavor has at least one initial consequence: that of having to become aware of what the mirror of the other makes of it, and, as a consequence, of engaging in a reflection on its own intrinsic identity.

The invitation to the historian reads something like a diagnosis: "History is involved in a redefinition of its aims and practices . . . but it seems to us that history is not alone in this effort, even if, because less codified than the social sciences, the field of history is the first publicly to air its reflections and uncertainties" (1989, p. 1322).

The aim of this issue of *Annales* contains an undisguised challenge, which can be summarized in the following terms: to define

1. See no. 6, 1989.

how interdisciplinarity – which is “a mode of intercourse among specialized scientific disciplines” – is conceptualized and undertaken. This is the kind of challenge that can’t be turned down. Like a lover, its hold grows ever stronger the more paradoxical its claims.

One example of the paradoxical nature of interdisciplinarity is the amount of research, of a composite character, that has been spontaneously undertaken, sometimes in regard to extremely complex subjects, sometimes in regard to subjects of the most simple make-up. Also, does not the sheer massive number of articles and works, ranging from the fields of sociolinguistics to ethnomusicology, from linguistic anthropology to ethnobotany and ethnomedicine, demonstrate without need for qualification that interdisciplinarity is an accepted necessity? How often, and repeatedly, have researchers – including researchers in disciplines which, although without a two-headed name (anthropology, linguistics, and musicology), are nonetheless, at least to a certain extent, involved in interdisciplinary activity – called for an open, inclusive, and multiple approach to the data?

And yet, as the historians make clear, this natural tendency toward relatedness between disciplines, this exercise of a relationship among neighboring fields, needs to be the focus of a special inquiry. We are no longer talking here about founding new disciplines or “interdisciplines”; nor of assigning borders, defining favorite subjects of discourse, nor even developing fields of research that have integrated all the latest (and best) advice. Rather, we are talking about *observing* the outlines of borders, observing the choice of objects and the progress of research; in short, what is called for is *grasping the interdisciplinary act in action*.

From this point of view, the anxiety of the historians finds its justification, *a contrario*, in the observable lack of interest in carrying out a rigorous evaluation of interdisciplinary practices, and the concomitant (and nearly complete) absence of manuals of a didactic character on how these practices should proceed. This two-fold deficiency (on both the level of evaluation and on the level of analytic tools) is a telling symptom of a field of research that is taking on more responsibility than it is able to handle.

Observation and evaluation? Fine. This is our aim. But, if we bear in mind the extreme diversity of intellectual lenses used by researchers and the increasingly exhaustive focus that these lenses allow, by what means and on what basis can observation and evaluation be carried out? Also, what about multiple studies of the same object, a practice frequently accompanied by compartmentalization

and a reciprocal ignorance of adjacent (and equally multiple) fields of research?

Similarly, what is the retrospective value of analyses, conceived and diffused with such great speed in the vast field of the social sciences (and on the basis of metalanguages specific to each discipline), when this diffusion itself is neither studied nor measured? Yet, quite often, the opening of new areas of research is accompanied by the creation of new metalanguages that are not subject to analysis by any permanent authority, even when this analysis is justified by the need for the accumulation of knowledge to an epistemological end.

In the face of such a stupendous shifting of grounds, such a redistribution of priorities, of methods and constraints that determine the choice of areas of research, where should we focus our analytic gaze?

How far can we go, and how long persevere, in our analytic enterprise, if it is true that the anthropological sciences are engaged in research that pulls simultaneously in two opposite directions? On the one hand, there is the fascination with the model of the exact sciences, transmitted with greater or less accuracy through the use of mathematical and linguistic models; on the other hand, there is the acceptance of almost any approach, justified or not, even if it goes beyond a certain coherence, including the limits defined by the social sciences (which are of course not the same as those of the exact sciences).

The preceding remarks are but a preliminary outline of an intellectual inquiry of the future. For now, however, whether we want it or not, the implications of these observations necessitate several independent inquiries: what connections and what hierarchies lie behind the diversification of approaches, behind the multiple studies of the object, the specialization and compartmentalization, beyond the changes of terrain? What is the source of the implicit, if not explicit, unity of the approaches? Finally, apart from the natural hesitation to speculate on its terms, what constitutes the coherence of the discourse?

As applied to interdisciplinary work, these reflections lead to at least three interdependent inquiries. To begin with, it would be useful to summarize how the border disciplines define themselves. Next, we need to explore their operational concepts, tools that are double-sided because they are derived from a metalanguage that serves as a bridge to the objects. Finally, we must inevitably plunge into the processes of collection and analysis, and this must be done

in a double manner: we must be equally concerned with rules of analysis and with actual analytic practices.

It is these three aspects of evaluation that will be analyzed here. We will offer both hard data and reflection, although in neither case do we pretend to exhaust the matter treated; our aim, rather, is to promote a comprehensive approach. This approach is concerned (and in an interdependent way) with ethnolinguistics, ethnomusicology and the ethnosciences (ethnobotany, ethnozoology, and so forth), as well as with sociolinguistics, psycholinguistics, and musical anthropology.

However, this project cannot be carried out successfully without an explicit assessment of anthropology, linguistics, and musicology; whether or not they are drawn to a certain desire for purity, these disciplines have made use of interdisciplinarity in several periods of their history. And these central disciplines constitute a favorite storehouse of know-how for the adventurers of the border disciplines, when the latter gather together their methodological array.

I. The Border Disciplines: Definitions and Denominations

The field of interdisciplinary studies is vast and productive. Many people work in it, and under various banners. This is because in the play of interdisciplinarity, there is a plethora of available goods: the object, whose intrinsic complexity is accepted in principle, offers much to many. And these many can happily discover more and more sides to the subject (we will put aside the question of newer and newer lenses). In the vast field of anthropology, interdisciplinarity is a given. But what does it give?

With very few exceptions, the lack of any complete map, of any pedagogical guide or user's manual, makes an immediate answer to this question impossible. It is therefore necessary to propose an approach in which, as is so often the case, what is observed is a function of the way the object is looked at.

A discipline is defined: it sets forth its objectives and its purpose, its objects of study and its methods. It also gives itself a name. All disciplines produce a discourse about themselves and, at least as a starting point, it is worthwhile to listen to this discourse.

The border disciplines display a wide array of approaches to self-definition and criteria that they select in order to ground their existence. In itself, there is nothing surprising in this. Is not a discipline conceived precisely in relation to its objectives, to its objects of study, to a conceptual framework and a know-how?

However, as we move from one discipline to another, one phenomenon becomes quite clear: each discipline defines itself less by an articulated group of criteria than by reference – one that changes depending on eras and schools – to criteria believed to be sufficient. The consequence of this type of attitude is that the vision of the object tends to remain rather sketchy, and the scientific activity that results is fundamentally unstable.

At the beginning of the century, the particular interest of ethnomusicology was exotic music, which included primitive and oriental forms (B. Gilman, 1909). Somewhat later, this distant object – in which “primitive” and “learned” music were in fact placed side by side – was to include the music of the peasants of Dalmatia (W. V. Bingham, 1914) and ended by including not only popular music but also dance (W. Rhodes, 1956).

Thus geographical and sociological criteria were explicitly at work in definitions that were accompanied by a process of classification developed along two axes: the opposition between that which was purely musical and something that participated in a different order of things; the distinction, at the heart of the musical universe, between registers whose elucidation does not initially depend upon an analysis of musical facts taken by themselves.

In short, ethnomusicology consciously retained only those characteristics, out of the totality of traits which characterized the objects to which they applied, necessary to define their discipline; as to the rest, they left the totality of definitions and their implementation implicit and non-articulated. The later evolution of the definitions of the discipline followed the same course, as did the definitions given of other border disciplines.

By the middle of the century, the separation of ethnomusicology from musicology was not so much expressed in terms of an opposition between “us” and “them,” defined on the basis of the operation of either a social or geographic distance. Instead, the opposition now implied a temporal criterion – music of before/music of now (G. Chase, 1958) – and also a criterion related to the modes of approach to the sonorous material whether or not tied to a cultural and/or social context (M. Kolinsky, 1957).

The processes undergone by ethnomusicology, and expressed in its definitions, are in part reflected in the names under which it was known. The *vergleichende Musikwissenschaft* was a science of comparative music. Here the musical data were analyzed within the framework of a comparison on the societal level, and this analysis

was carried out in a double context: that of a geographical dispersion – thought to be more or less accidental – and that of evolution, measured by criteria that were fundamentally philosophical in character.

The rules of ethnomusicology (J. Kunst, 1950) and musical anthropology (A. Merriam, 1964), which were fashioned somewhat later, flaunt the connections between musical and non-musical elements. Although in theory widely favoring a synchronic approach, in practice the researchers' method, as concerns the relationship between data of different orders and modalities of analysis, was similar to many nonsystematized approaches that had been employed from the very beginning of large-scale attempts at inter-disciplinarity in the field.

Heterogeneous in its very make-up, the object of inquiry is therefore not initially conceived in regard to number, nature, or the expression of its facets, or in comparison with the methodological continuum for which it becomes the pretext. Both the object and the approach are defined and denominated by bursts of analytic light which, although partial, are then treated as if exclusive.

Did these border disciplines, which were largely created as a reaction against tendencies attributed to linguistics, anthropology, and musicology – for which heterogeneity and blending were supposedly a kind of nightmare – actually find this temptation for purity in the basis disciplines?

To answer such a question, it is not enough merely to analyze definitions and denominations. Rather, one must observe how ethnomusicologists treat musical matter; we must also see what place they reserve for the extra-musical and how they articulate and implement the difference between the musical and non-musical spheres. Beforehand, it will be necessary to understand how the intellectual material is organized; and this can be done only on the basis of an identification of the elements, as seen through the prism on which the two clusters of research are to be defined, that stand between the subject and the object.

But for the moment, let us return to definitions and denominations, this time as regards ethnolinguistics, sociolinguistics and psycholinguistics.

Just like ethnomusicology, the border disciplines that make language the central support of their research existed before they were named. The successive generations of French dialectologists and

ethnographers did not wait for the invention of detailed labels in order to begin gathering and analyzing objects that were eminently linguistic and ethnographic. The currents of thought founded by the theories of W. von Humbolt (1968) and those tied to the names of E. Sapir and B. L. Whorf (B. L. Whorf, 1956, E. Sapir, 1949, 1960; H. Hoijer, 1954) relied on different epistemological contexts; this was done, however, without it leading to ready-made labels and without waiting for the invention of the labels in order to carry out the research.

On the other hand, if the belief that the rapid shifting of criteria of definition was a result – in the case of ethnomusicology – of a pure and simple revision of data at each step of the chronological development of the discipline, then the three border disciplines affiliated with the study of language force us substantially to modify our vision of things.

At the same time that structuralism was at its most productive, an impressive expansion of criteria was used to found ethnolinguistics, sociolinguistics, and psycholinguistics, not to speak of subsidiary branches. Moreover, this frenzied expansion was marked by an equal fervor for purity.

Subdivisions continued to be created at a fast pace: the distinctions between the sociology of language, sociolinguistics, and differential linguistics, for example, and those between linguistic anthropology, ethnography of communication, and ethnolinguistics, revealed a determination to encompass better the infinite variety of objects, to multiply the angle of attack and, sometimes, to dissociate certain approaches that were considered incompatible with the philosophical foundation.

In the facts elucidated as much as in the aims pursued, the objects in which these border disciplines were interested corresponded to a particular area of the verbal, social, or cultural *continuum*, and was accompanied by a more or less well developed method of posing and thinking through the question of the relationship between the verbal, social, and cultural spheres.

Quite often, however, the desire better to encompass an object leads to the use of restrictive methods that in the end contradict the initial interdisciplinary aim. For instance, ethnolinguistics, in the name of a philosophical principle, thus often found itself forced to deny the variety of phenomena encountered in certain exotic lands, and to exclude sociolinguistics altogether from its purview. To take another example, why should the ethnography of communication

and differential linguistics be incompatible in the same place and while engaged in analyzing the same object? In other words, under what conditions have the development of ideas and philosophical choices facilitated – and to what extent thwarted – the progress toward the object? Indeed to what extent are these ideas, in themselves, an obstacle to a full comprehension of the object? Without giving due attention to these last questions, we run the risk of seeing the exclusions and suppressions that have been noted unfold to the detriment of our understanding the object itself, unless this lack of methodological rigor amounts to nothing more than a pure and simple form of self-defense.

A prime example of the extensive development of this tendency can be observed in the criteria that each discipline uses when it attempts to ground its existence. What common measure can we use, for example, to define the nature of the relation between society and its speech (G. Calame-Griaule, 1977) or that of the linguistic message and the totality of circumstances of communication (B. Pottier, 1970); between a linguistics of language based on oral tradition and the study of symbolic communities; between the semantic bent of a certain kind of ethnolinguistics and the interest in establishing a typology of relations that unite language, speech and discourse to other cultural facts; and for the relation of the subject to his or her speech, and even for the uses of speech in general (F. Alvarez-Pereyre, 1981)? Surely, all these various projects have something in common: they are an attempt at expanding the limits of a linguistics or anthropology considered to be too restrictive.

However, posed in this way, the preceding question is misleading if not actually incomprehensible. In its very materiality, the question is symptomatic of a lack of intellectual foundation; it necessarily calls for explanations that necessitate passing beyond the situation it seeks to describe. *This is because the question reveals that criteria of different nature are placed side by side, as if they were located on the same conceptual plane, or as if they were literally equivalent; as if, as regards the object of scientific scrutiny, they were not located at different stages of an integrated approach to a particular object. And yet, it is by this means that the interdisciplinary discourse develops when it attempts to reflect upon itself; and this activity, of course, is hazardous to the object, hazardous to the entire intellectual discourse. Interdisciplinary activity thus simultaneously manifests a natural attachment for a certain methodological purity and a remarkable confusion about what interdisciplinary activity actually is. Does this not put the interdisciplinary project in peril?*

This analysis is equally applicable to the methodological tools to which ethnolinguistics, sociolinguistics, and psycholinguistics alike lay claim, when these tools are invoked in the definitions. The existence of a multiplicity of options and tools is then asserted, which again leads the author to this brutal observation: *because the constitutional complexity of the object is the central justification for interdisciplinary activity, it logically follows that interdisciplinary activity should pose and think through the question of unity (or at the very least of coherence) and, at the same time, the question of the kind of continuum on the basis of which interdisciplinary activity develops.* And yet, whether we are talking about psycholinguistics, ethnolinguistics, sociolinguistics, or ethnomusicology, we note the very same absence of expression and hierarchization of the variety of methodological options – none of which are perhaps in themselves inadmissible – that is to be found in the declaration of criteria of existence or the constitutive facets of the objects of study.

The same impression is left by an evaluation of the definitions and denominations proposed for the other ethno-sciences (J. Millot, 1968; A. G. Haudricort, 1969; S. Bahuchet, 1989). Just like the linguistic sciences, these sciences – ethnobotany, ethnozoology, ethnominalogy – consistently promote the idea that relation is the constitutive element of their subject and, consequently, assert the logical need for an intellectual program that such a fundamental choice demands.

In the meandering course of definitions and denominations, “interdisciplinarity” has become a catch-all that sometimes seems to supplant scientific activity itself. It does so by depriving the object precisely of what interdisciplinarity asserts in its regard, namely, the object’s constitutional complexity. At the same time, if interdisciplinarity is, or should be, “a mode of intercourse among specialized scientific practices,” then the approaches advocated by the border disciplines – as revealed in their definitions and denominations – can only lead us to believe that these specialized disciplines are based on a rather unstable substructure, in relation both to the object of study and to the intellectual procedures used to analyze it.

It is far from our desire to deny the possibility of interdisciplinary activity. Nor it is our aim to reject the idea of specialized sub-disciplines. *This is because what is at stake in interdisciplinarity is simultaneously, and correctly so:*

- the necessary questioning of the temptation of purity – which is felt to be philosophically untenable;
- the double labor through which it becomes possible to give due weight

both to the object's constitutive complexity and to its coherence;

– the question of the type of unity or coherence that runs through the entire scientific discourse (beyond the inevitable transitory or intermediate specialties), since this discourse places the multiplicity and complexity of the object at the center of its attention.

This vast program, suggested by the invitation to the discipline of history to reflect upon its relationship to the social sciences, must also be implemented by anthropology, and without shrinking. However, as we conclude this first stage of our analysis, it is incumbent upon us to observe that it is equally vital for the border disciplines to enter into a rigorous reflection on the methods of their theoretical and practical itineraries; for if they do not, they run the risk of seeing the object of their attention vanish before the sincere gaze they claim to focus on it.

II. The Operational Concepts: A Case Study

To foster this inquiry, and apart from the definitions and denominations that a discipline ascribes to itself, it is now time to try to clarify another, extremely intricate aspect of the identity of a discipline: namely, the moment which, preceding or largely conditioning the way the data is analyzed, coincides with the phase when the object is approached. This phase is composed of a total intellectual "meal," whose ingredients are simultaneously multiple and heterogeneous, and more or less explicit but also fundamentally operational.

In this context, several complementary inquiries are required. One of them is comprised of the analysis of the metalanguage that a border discipline consciously employs. What are the specific characteristics of this metalanguage? Do other border disciplines share this same metalanguage? Is the metalanguage itself derived from the basic disciplines? Two recent inquiries show the benefit to be gained from such inquiries. One of them concerns ethnolinguistics, in particular the notion of the semantic field (G. Drettas, 1981a); and the other, sociolinguistics, specifically the idea of diglossia (G. Drettas, 1981b).

Another approach involves the attempt to situate the border disciplines or to describe the explicit coexistence of two or more basic disciplines within the context of the history of ideas. In this regard, one can cite work concerning the relationship between ethnolinguistics and the ethnosciences (J. Molino, 1981), between anthropology and history (J. P. Dozon, 1989), or anthropology and ethnomusicology (F. Alvarez-Pereyre, 1988).

By combining the two types of evaluation that we have just enumerated, it should be possible, ultimately, to specify the amount of autonomy and the specific characteristics of each of the border disciplines, and also to draw up an inventory of the conditions under which bridges between the basic disciplines can be constructed.

With the same aim in mind, it would seem to be useful to engage in at least a third type of inquiry, one that makes the operational concepts themselves a subject of reflection. Here we will try not so much to encircle the metalanguage as if it were in a test tube, but rather to observe the various stratagems applied by researchers as they attempt to approach the subject (and which precede the stage of the analysis of the object itself). As an example, we have chosen to take up the notion of "orality," since it, more than any other concept, directly uses the totality of the border disciplines involved as well as the basic disciplines themselves.

Orality in its Objects

What is concealed by the idea of orality, a notion as omnipresent as it is ill-defined? Under the rubric of orality, researchers from the human and social sciences treat objects of a bewildering variety – objects that are immediately consigned to a group of notions (orality, the oral sphere, oral tradition) whose borders are notoriously fluid.

The typical object of study – although not the exclusive object, far from it – is, quite often, oral literature and its canonical genres (tales, myths, riddles, legends, proverbs, sung fables, and other forms). But where does oral literature begin and end? And what criteria are used to justify the insertion, into the field of oral literature, of genres that in other fields are considered to be basic or secondary?

When the question of the status and definition of genres is raised, a debate over the borders of oral literature inevitably follows. Have we not recently seen how "ordinary speech" has managed to win over domains that once belonged solely to the totality of linguistic creations conceived precisely as distinct (formally, stylistically, or sociologically) from the plane of daily language? Yet, and quite interestingly, it was by appealing to the extension of the borders of what could be legitimately treated as written literature that the rupture of the traditional range of oral literature was justified (D. Rey-Hulman, 1987, p. 7).

The oral object is composite, and its criteria of definition are variable and shifting. This is true not only of oral literature. Magical incantations, because tied to ritual acts, have been displaced from the field of oral literature by some researchers. This was done in the name of an adjacent theory, according to which the genres of oral literature – although determined sociologically in some cases – cannot legitimately be tied to functions of a central or explicit nature. For others, only the texts of the incantations themselves can be assimilated to oral literature, since they are verbal matter. But then what are we to do with the rituals to which the incantations are so closely tied, and without which the incantations lose their purpose? If these rituals cannot be considered part of oral literature, do they not at least partake of orality?

Before taking up this important debate (which extends to many objects of anthropology), we should point out that the actual range of oral literature finds itself confronted with another transformation when it is discovered that an oral repertory, passed on by word of mouth, is partially based on written liturgical texts (S. Critescu, 1984). Is this oral memory still part of the domain of orality? For some researchers, it is enough for orality that these repertoires retain their verbal forms of transmission and apprenticeship (as well as enunciation), even if an archeology of the actual statements reveals a written element within the oral act. For others, the mixing of codes and the identification of sources to some extent invalidates the application of the label "orality."

Might one find a more solid terrain by abandoning areas in which orality and written language overlap? Even supposing that there are areas of strict orality, that is areas in which the linguistic material can be assumed to be "pure" because not hybrid, certain thorny questions that arose concerning oral literature continue to exist in the non-written setting: can tales, myths, and proverbs exist in the same conceptual framework as ordinary speech? Are magical incantations – as purely oral as they may be – part of oral literature to the extent that they are text, although something else (but what?) for ritual and the totality of other, always oral, contexts? By extension, when genealogies and epics become an object and means of political or social anthropology, or of kinship studies (A. Deluz, 1970; M. Izard, 1970), are the resulting texts still within the compass of oral literature, and under what conditions?

Let us summarize. In the limited context of oral literature, a series of consequences results from the introduction of the notion of orali-

ty; first, a kind of code (oral, verbal) is established; then follows speculation about a typology of relevant linguistic products (from ordinary speech to the canonic genres); next, ideas about the nature of the relationship between linguistic products and social environment, and between the text, its usages and its contexts; finally, places of predilection for orality are established.

If these are the criteria by which locales are to be designated as civilizations with an oral tradition, then at least two further questions arise: what is the status of orality elsewhere? Also, how are we to understand the meaning of orality here, and what are the ties between text and speech to other elements (such as society, culture, rituals, techniques, and other factors) of civilizations with an oral tradition? Let us try to take these questions head on.

Learning to play a musical instrument in a conservatory or a music school is based, in France as much, for example, as in Indonesia, on the use, to varying degrees, of musical scores and/or other methods. For the music of the past, but also for more contemporary repertoires, instrumental technique and phrasing too can be learned with the help of various treatises. Along these lines, when a student is taught by a master, an entire body of directives and comments which, although not consigned in writing but offered verbally, constitute a large part of the instruction; these oral instructions pertain to the correct playing of the instrument, and can even include the teaching of the musical theories that underlie the scores and treatises themselves.

There are parallels to this in many other trades and crafts. The transmission of such knowledge does rely in part on technical manuals, but it also relies on oral transmission. Undeniably, therefore, the total body of written and oral elements (not only verbal, but non-verbal as well) constitutes a non-negligible part of actual technical knowledge.

What, then, can be said about the significance of apprenticeships and technical knowledge when writing is lacking, or when writing plays no role in an entire sphere of technology? Apart from the material itself (primary materials and tools), there is, at the very least, a collection of verbalizations tied to the processes of fabrication and usage, even in cases where apprenticeship itself cannot be literally likened to an oral technical manual.

We therefore find ourselves as far from the canonical genres of oral literature as from so-called ordinary speech. However, the question of technology or of certain other objects scrutinized by eth-

nomusicology brings us back to a subject which, as it relates to studies of kinship, magical incantations or epics, and genealogies, is equally of interest to political and social anthropology; to wit, the existence of an oral sphere – manifested in various forms – which, at the very least, corresponds to linguistic products whose nature, in other respects, is quite variable.

Now another question arises: beyond this purely verbal orality, are the other constitutive elements of the object assimilable to facts of orality? “Yes” will answer those who assert that what we call writing civilizations are societies “dominated” by writing, even in cases in which the oral sphere is quite alive; and they will consider those societies in which there is no writing – hence societies lacking writing – as civilizations of orality. But in this case either we are taking a part for the whole, or, more simply, we haven’t yet determined what, outside of the verbal – and with or without writing – constitutes the essential ingredient of a human group (its technology, relations of kinship, social organization, and/or other factors) and what makes it possible to identify these factors.²

At this point many a researcher, overwhelmed by the vertigo caused by the observation of objects that vanish as soon as he tries to catalogue them, is tempted to reach for that final life raft which the study of ordinary speech, the oral object *par excellence*, seems to offer.

Yet oral language, which is a desired object because used daily, and a reassuring object because fundamentally verbal, in fact often finds its own existence challenged or compromised: sometimes it is channeled into the mold of written language, when this mold becomes the sole possible means of communication (H. Frei, 1928) – but, at that point, in what sense can one continue to speak of an oral sphere?; sometimes reduced to newspaper excerpts or to models taken from grammar textbooks, disguised as spoken language (C. Fuchs, J. Milner, 1979); or quite simply drained of all life in the arrogant blindness of sociolinguistics (H. Bonnard, 1982).

If such is the situation for a language extensively treated by gen-

2. The present situation would appear to be definitively untenable when one bears in mind the fact that, during the Semitic era, there were entire civilizations which, from the beginning of their existences, instituted – at the very heart of their ethnic identity and their social and cultural foundations – a necessary relationship between writing and orality, giving each of these concepts theoretical and practical definitions that cannot be reduced to verbal and linguistic materialities, no matter how complexly conceived.

erations of philologists, grammarians, and linguists of all persuasions, then what hope is there of finding real consolation in lands where there is neither writing nor newspapers, in exclusively oral languages that interest those who are called – and often in quite a restricted sense – field linguists?

In a certain sense, the situation of the field linguist can be described as both original and irreducible. For example, the linguist who is interested in one of the numerous African languages should in fact be interested in the status of the language itself. However, generally he will not be able to find written creations that spring from the group he proposes to study. Nor, in general, will he know anything about the language he wants to describe. And he can only gain marginal help from the knowledge of geographically neighboring languages or ways of speaking.

Yet, even when he can rely on previous knowledge of the speech of a single linguistic group, or avail himself of grammars or lexicons devised in the past by a missionary or colonial administrator, the linguist is nonetheless faced with two kinds of questions: the first concerns the type of linguistic product the researcher intends to analyze as he attempts to develop a description of the language; and the other is the type of intellectual tools to be employed.³

What oral material will in fact be retained and highlighted? Will the linguist have recourse to an approach that emphasizes a linguistics of the sentence or of discourse? In either case, will there be an initial postulation of a type of canonic model of either the sentence or discourse; or will we only proceed to the description after extensive scavenging of the forms of possible phrases or discourse, without limiting ourselves to an *a priori* hypothesis in regard to a formal central type?

As to the description itself: will the linguist limit himself or herself, without examination or evaluation, to the use of linguistic tools derived from intellectual history, and used and refined in regard to languages whose formal structures – everything would lead us to believe – are different from the language in question? Or, instead, will the question of constructive models of linguistic systems be posed in order to go beyond the existing tools if these prove unable to account for the specificity of a particular language?

However, even in this case, having set off in search of oral materi-

3. Need we emphasize that these same questions concern those who want to describe, for example, a Bulgarian dialect, spoken French, or the language of the Sames tribe?

als and targeted for analysis only those that can be called verbal (and for which, in many cases, only an oral code exists), we will nevertheless find that our grasp of the object remains a problematic one – as much in regard to the language's contour and borders as to the analytic tools used to define the object.

Our remarks concerning the contents of the object are not new; the question of the analytic tools, however, is now posed for the first time. It is a question that inevitably arises when one tries to encompass the objects of orality, and not only in regard to linguistics. Is not the history of ethnomusicology haunted by the question of what tools are appropriate for transcription and analysis? And doesn't this concern in fact run through ethnomusicology's entire inquiry into its objects and limits (A. Merriam, 1964; B. Nettl, 1964; S. Arom, 1985)?

As regards orality, what can we conclude after our inventory – an admittedly incomplete one – of its objects of research? From the point of view of its objects, orality has two definitions. The first is a restrictive one, limiting the scope of orality to the spheres of the verbal and linguistic, each of which is itself divided into a code and its products. The other is extensive: here orality encompasses the verbal and linguistic spheres – also divided into a code and its products – and then spreads into other aspects of culture (jests; rituals; musical theories and techniques; technology; social and political organization; and other factors), whether or not they are tied to speech.

The two definitions just enumerated are, strictly speaking, but an indication of tendencies and means. To turn them into hard and fast definitions would only lead us back to the demand for purity that has seemingly so often arisen as if simultaneously with the demarcation of the objects.

Yet this temptation remains perceptible on three planes. First, in relation to the actual matter of the object: this matter, inscribed in typologies whose borders fluctuate, can, depending on the case, be either disjoined or linked, such as can be done with the canonic genres of oral literature and ordinary speech; or with the minimal expression that constitutes either the phrase or discourse; finally, with the plane of phonological structuration and structural data of phrastic and transphrastic intonation.

It is in the approach to geographical identification that the temptation for purity is most obvious: according to this approach, there are locales of predilection for orality (civilizations based on oral tradition), and this hypothesis is used in order to establish, in certain

cases, the idea of the imperviousness of codes (written and oral). Finally, this temptation for purity affects the types of relations that are supposed to exist – or, quite often, not to exist, or not brought to term – between data of one order (the linguistic order itself in the varieties of its products, incantatory texts, technological terminology, oral instructions relating to musical phrasing, ordinary speech and words from dictionaries) and data of another order (magical rituals; analytic tools and equipment; manufacturing processes and techniques in the narrow sense; treatises on organ playing or musical theories; and others).

Yet, simultaneously, many factors run counter to the temptation for purity. For one, in many places, the existence of pure orality is a fiction. Still, there has not yet been any real exploration of the infinite number of examples of the co-existence and overlap of orality and written language. Secondly, the ties between data of different orders are simply too strong to allow for a convincing hypothesis of purity, that is the non-articulation of different orders. By adopting a more realistic view, we are in fact led to reflect on these ties and articulations; yet this subject, which could hardly be more obvious or necessary, has rarely been systematically explored. Finally, our brief inventory of objects has allowed us to touch on questions concerning the analytic tools employed by the researcher. Methodological questions remain open. These questions transcend not only the borders between objects but those between separate disciplines; they also go beyond the exclusion of objects as regards geographical identification, and the types of definition – narrow or wide – of orality.

Definitions and Theories

Orality, because heterogeneous in its choice of objects (and hence heterogeneous in relation to these objects), is defined by many different criteria. These various criteria are then applied to an undisciplined scientific discourse which, when analyzed, allows us at last to comprehend certain theoretical tendencies that are, however, not consciously employed by the researchers themselves.

Will an exploration of theories connected with orality, and an attempt to establish certain definitions of it, only exacerbate this state of affairs? Or will it instead permit us to identify several landmarks of a more stable and, above all, more orderly nature?

These theories and definitions in fact constitute a complex field, which is itself subdivided into more particularized topics that are compartmentalized under the names of oral traditions, orality, and

the oral sphere. The notion of the oral sphere is strongly tied to the existence of linguistic codes and the conditions of their definition and exploration. The concept of oral tradition is identified in two ways: the first is in relation to investigations of an historical character; the second is concerned with matters of cultural and social identity. Finally, the term orality stands at the heart of inquiries into the way areas of knowledge are formed.

As autonomous as they might appear to be, these separate topics are in fact linked in many ways: to begin with, there is no cut and dried separation between codes and knowledge [*savoir*], between knowledge and identity, history, culture, and society. Also, the very same keys, the same intellectual guides and tools, are often employed to mark out more than one of these topics. Finally, and surreptitiously, these topics, and the ideas with which they are labeled, fundamentally have the same things at stake, and are haunted by the same intellectual and philosophical demons.

1. THE ORAL SPHERE

As a code, the oral sphere is defined in one of two fundamentally different ways, depending on whether the studies are motivated by a desire to establish its intrinsic qualities or are instead willing to let themselves misrepresent the nature of the material itself.

There are many ways in which the oral sphere is misrepresented, a variety of methods employed in order to avoid facing the material head-on. The role played by certain sociolinguistic factors is decisive here. Thus, in France, the fact that an interest in dialects did not arise until the eighteenth century can be directly attributed to linguistic planning (G. Tuaillon, 1976). But does the historical development of the discipline, the particular configuration of its becoming, oblige the linguist automatically to forsake the demands of description, to "forget" entire aspects of the reality of language?

B. Malmberg's implicitly negative answer to this question can be seen when he states that "the linguists realized too late that only in the dialects were the oldest forms, words and phonetic habits preserved, features that had disappeared altogether from the national language" (1966, p. 83). But this answer relegates oral language to a function of conservatory for fossilized materials, suppressing any discussion of the oral and/or written modalities of the dialects, on the one hand, and of the national language, on the other.

This example, as secondary as it may be, nevertheless demon-

strates the urgent need for a complete review of the paths that have been followed again and again in order to avoid the oral sphere, or, rather, to avoid grasping it on its own terms. It was as if, as soon as it was conceived of and then made into a project, this oral sphere could only be grasped through the intermediary of substitutes.⁴

It is true that to grasp is partially to mutilate, and that all representation is transfiguration: but is not the linguist's job to describe what he observes by depicting it? If this is so, then there is no escape: the attempt to formulate this transfiguration becomes inevitable.⁵

This imperative is not limited merely to the area of transcription. The clarification of the specific nature of the oral sphere leads, on the one hand, to the elucidation of a linguistic material that, quantitatively at least, is quite large; on the other hand, it leads to the necessary activity of accounting for the intrinsic nature of material that can neither be assimilated to any other, nor reduced to any code or speech; this, in the end, will allow a more accurate perception of the diverse elements that constitute the totality of the act of speech and, more generally, the linguistic act itself, as much in its codes as in its products and their processes. And these obligations prompt a new requirement: that of a real intellectual and methodological investment, one that will be able to avoid all the temptations to misrepresent linguistic reality.

2. ORAL TRADITIONS

The study of oral traditions has proven useful in the work of both historians and anthropologists. However, this work has been understood in conflicting ways by those who have made use of it. Some specialists assert that only oral testimony directly and explicitly linked to the historicity of the group under study can be considered legitimate (D. P. Henige, 1974). For others, like J. Vansina (1961), the idea that there is but a single legitimate link to the historicity of a group – which prompts the exclusion of “oral art” – constitutes a harmful constraint to the objective of reconstituting a real history, an objective that can only be realized by a profound exploration of identities.

4. It was precisely this concern that motivated and brought about the joint labor of C. Blanche-Benveniste and C. Jeanjean (1987).

5. See, for example, the reflection that this question provokes from D. François (1974) and V. Labrie (1982), and from their colleagues in the *Groupe aixois de recherche en syntaxe*. Their work appears in the review *Recherches sur le français parlé*, published in Aix-en-Provence.

Indeed, it has been shown, in a variety of locales, that the recourse to linguistic terminologies, to songs and proverbs, to texts of oral literature and to botanical expressions, yields impressive amounts of information (J. Vansina, 1978; A. Bensa and J. C. Rivierre, 1988; R. Letouzey, 1976; J. E. Mbot, 1975; D. Rey-Hulman, 1977). This more liberal attitude in regard to sources is accompanied, in all cases, by theoretical and methodological inquiries that equally influence the choice of tools and the approaches of the historian, the linguist, and the anthropologist. Such choices run exactly counter to the arguments that have led many an historical anthropologist to reject texts issuing from the oral tradition (under the pretext that these arguments are made *pro domo*), in preference to written sources that, however, are neither more neutral nor objective, although they are admittedly more exterior (J. P. Dozon, 1989).

Equally subject to question are the romantic notions pertaining to oral tradition and, more generally, to folklore; these objects of study have often been conceived of as pure, intangible and durable, free from all hybridization and transmitted, in the European milieu, right to the heart of the illiterate strata of society. We are also far from the clichés concerning “exotic peoples,” peoples who are exclusively oral and therefore believed to be fluid, spontaneous, and lively, because original and vital.

Having overcome the theoretical and methodological restrictions that, in regard to the idea of oral traditions, had put the object at risk, we can now rejoin the far wider field of objects and approaches occupied by anthropology: “Either the idea of oral tradition is to be taken seriously, in which case anthropology’s aim is to research everything that is transmitted orally, that is to say, that which is passed from generation to generation and is not written – but by what right do we then separate from a community’s life what happened yesterday from what happened today, and what is oral from what is written?; and if we can’t, then no sharp contrast in the community’s symbolic life can be made between the old and the new, the oral and the written – but then how and why can we still speak of ‘oral tradition’ at all?” (J. Molino, 1985, p. 40).

This logical conception, which results in widening the concept of oral tradition, inevitably joins with a more comprehensive hypothesis (which itself arises in the course of exploration of those objects that research on orality summons forth) and is opposed to any more restrictive and reductive way of conceiving of these same objects. According to J. Molino, such a widening of conceptions, such an

extension of the contents of the concept of oral tradition to include all aspects of culture, inevitably leads to reflection on the status of the symbolic order: what is the symbolic order, and how can it be studied?

Contemplation of culture is contemplation of the symbolic realm, but it is not only thinking about or contemplating this realm; it is describing it. Here, before embarking on a new inquiry into the subject of orality, it might be wise to couple, in a single reflection on culture, the expanding of our understanding of the symbolic realm with the kind of inquiry on anthropological practice envisioned by P. Bourdieu (1972). It will be useful, in this regard, not only to contemplate the practices but to describe them as well. But is not this double questioning precisely what marks the demands of interdisciplinarity, that is to say, the presence of a coherent discourse – at the very heart of a discussion on the oral sphere, oral traditions, and orality – regarding the operational concepts that give us access to the objects in the first place?

3. ORALITY

Although admittedly more generic than the others, the concept of orality does not solely serve to gather together whatever is not specifically applicable either to the oral sphere or to oral traditions. However, when the concept of orality is conceived of apart from the others, there is a readily observable upsurge of the striving for purity.

The violence of this upsurge is perhaps not very different in character from the passion with which assertions pertaining to the oral sphere and oral traditions are made. But, in the case of orality, the debate cannot be reduced – so to speak – to a mere draining of the contents of the oral sphere, in order to measure its appropriate “thickness,” even if only as it concerns codes; nor is it limited to a gauging of orality’s exact nature, apart from its assimilation to speech acts. With orality, this debate cannot be restricted to questions of oral traditions alone, although we have seen that these traditions touch on nearly all matters of general culture and identity (that is, when we do not limit our understanding of oral tradition to a mere chronological inventory of oral products).

As we pursue the concept of orality, major stakes become immediately and irremediably apparent; perhaps the same ones, in essence, that are at work in the more restricted contexts of the oral sphere and oral traditions. Although these stakes are often enough

presented in the form of a sharp opposition or even confrontation between two types of civilization, on a more profound level they concern the formation of areas of knowledge. In all cases, we witness an opposition between the use of dichotomous expressions on the one hand, and more nuanced formulations on the other: formulations that defend the idea of a *continuum* or overlap of data of different orders.

It is in the name of the materiality of the verbal sphere itself that the opposition between societies with an oral tradition – in which the power of speech is supposedly at work – and societies with a written tradition – in which the power of the text is supposedly at work – is advanced (L. J. Calvet, 1984). A common extrapolation from this oppositional framework is that law and rigor are concomitant with peoples of the book, and imagination, anarchy, liberty and malleability are the province of orality.

J. Goody (1987) proposes an altogether different arrangement. He ascribes benefits to writing that supposedly lead to a logical and coherent approach to experience, to critical skepticism or, in brief, to freedom of thought; to oral civilizations he ascribes a lack of perspective and distance, and a lack of freedom due to too great an immersion in matter itself. By making use of heterogeneous criteria, and by proposing contradictory concepts of the same object, Calvet and Goody create opposing models: one scholar offers two forms of linguistic communication and two forms of society; the other, two kinds of being in the world.

However, beyond the divergence in contents, a similar type of argumentation runs through both approaches: Calvet's statement that "In a society with an oral tradition, the organization of the world is tied to material and ideological conditions that are very local and rigid" (L. J. Calvet, 1984, pp. 54–55) is echoed by Goody when he writes that writing and scholasticization lead to a "decontextualization of knowledge" (J. Goody, 1979, p. 52).

Apart from the premises that underlie the proposed dichotomies, the chief merit of the argument presented by the authors quoted above is that it necessitates a closer look – on the basis of the contexts of knowledge – at the processes of formation of this knowledge and at the conditions of its social and political implementation. Once these processes and conditions take center stage in anthropological research, it immediately becomes clear that a contextualization of knowledge takes place in all cases, and that the forms that this knowledge assumes can be elucidated only after a painstaking

description of the modalities of knowledge and a comparison of its forms.

Once we do this, the theoretical debates over the status of orality and writing are deprived of much of their substance. In any case, all clear-cut oppositions, formed on the basis of specific times and places, on impenetrable codes and modalities, on absences, and, finally, on the idea of basic impossibilities, are invalidated. *Instead, a rigorous analysis of the formation of different kinds of knowledge leads to a description of their modes of contextualization, here and now; and, as regards codes, an analysis of methods of elaboration, of processes of acquisition, treatment, transmission, and recognition: this can be done not only with linguistic but with other kinds of data (sociological, historical, etc.) that prove to be relevant in the framework of an analysis of particular objects.*

Taken up in this way, this general approach requires us to reconsider certain assumptions. In doing so, we find that the forms and functions of rhetoric are not absent from orality (C. Seydou, 1989); the characteristics that constitute a "performance" can also be found in written texts (R. Finnegan, 1977); the capacity to classify is not the exclusive property of writing civilizations (R. Finnegan, 1977; C. Seydou, 1989).

If neither type of civilization is fundamentally lacking, *a priori*, any specific categories of knowledge, and if anthropology cannot exclude anything from its purview, then there is, even more certainly, an overlap – specific to each situation – of data of different orders (R. Finnegan, 1977; L. Treitler, 1981; M. M. J. Fernandez, 1987). M. Detienne implies as much when he writes, in regards to the civilizations of both ancient Peru and ancient India, of "The Brahmins of today" and a "Koranic school in an African village" (1988, pp. 10–11): "On the one hand, great civilizations have been built without recourse to written techniques, without the development of intellectual technologies based on a system of the written notation of thought"; and "besides, many societies have known a mixed social organization, half oral, half written, without an initial preference for one form of communication over the other."

What should be retained from our brief journey into orality? First, we noted, in regard to the central concepts of the border disciplines, a powerful temptation for purity that was already quite perceptible in the way these disciplines defined and denominated themselves. This temptation for purity was based on various arguments, although all of them were marshalled when it came to numbering

orality's objects. But however and wherever the temptation was justified, the effects of this tendency were everywhere fundamentally the same: they seemed to contradict the aims that the disciplines assigned to themselves (unless we credit them with helping us better to define the demands and ultimate difficulties of purity). However, this temptation for purity, which was already evident in the definitions of orality itself (a concept that we chose to investigate because of its particularly revealing operating principle), was also apparent in the many other approaches that analyzed the concept of orality according to their own ideas.

Along with this tendency, which amounts to separating and cutting out multiple and slender bands of cloth, there is surely a place for a rigorous analysis of the bands themselves. When this is done, central questions arise – questions that have nourished classic anthropological debates and that the border disciplines can only hope to elucidate in a new light, since it is their job to embrace the complexity of the object as well as describe the modalities of the scientific continuum that this complexity summons forth.

These classic debates are no longer a mere pretext for flights of philosophical fancy; they can become the source of a necessary methodological activity that the needs of analysis make justifiably inevitable. But how are we to embrace this necessity?

III. Interdisciplinary Activity and the Analysis of Materials

Having made the establishment of relations the keystone of its intellectual activity, interdisciplinarity must face a series of choices regarding both its objects and its practices.

Where and when ought this relation be established? Does this activity correspond to a particular stage of the enterprise; and if so, to which? Or is it rather a watchword that, transcending the successive stages of the study, fundamentally remakes the overall way in which we work? What ought to be the content of this process, and who will establish it, and in the name of what? What are the logical corollaries of any particular analytic technique? Does the number of such techniques have any real importance?

Next, where are the remaining parallels, and where the divisions, between, on the one hand, the decomposition of the object and the subsequent synthesis of its constituent elements and, on the other, the successive and complementary phases of a scientific activity that itself is composite in its stages and analytic tools?

Various answers have been given to these questions: sometimes in the form of postulates and propositions, and sometimes as comments offered in the midst of the treatment of data. What do they say about the interdisciplinary approach?

Postulates and Propositions

Among the existing propositions, let us begin by recalling what M. P. Ferry says in regard to ethnolinguistics (1977). This discipline, positioned at the border of linguistics and ethnography, is faced with a methodological choice. This choice comprises two separate stages, each with its own tools and a succession of specific practices in regard to a shared object that is probed by a comprehensive view.

J. H. K. Nketia too speaks of a comprehensive view in regard to ethnomusicology (1985). This view, which is asserted from the start although not concretely employed until a final stage, is realized with the help of integrative techniques that strive to assemble the work carried out, either in parallel or in succession, on the same object.

In short, a scientific continuum is asserted. It is actualized in a series of successive stages that are equal to the number of disciplines involved; the unity of the object, asserted from the beginning, becomes visible only thanks to the work of reciprocal and cumulative clarifications. In the other methodology, the scientific continuum, whose necessity is again assumed, develops by means of specific practices whose results are later collated on the basis of integrative techniques.

We have here, then, a demand for unity that necessarily calls for the use of specific analytic tools (in order better to clarify the various sides of the object); its terms of actualization may vary, although its unity is assumed from the start.

But is it enough that the demand for unity emerges cumulatively, as the result of a confrontation of partial and successive results? Moreover, can this unity be accepted when the theoretical foundations of this confrontation are suppressed?

Is it true that only techniques used *a posteriori* can permit us to account for the intrinsic links that unite the sides of an object, especially if we cannot know the nature of the philosophical ins and outs that define these techniques?

In addition, are we not thus compelled to concede that each particular discipline, as inevitable as its existence may be, is incapable

of submitting its own aims and analytic tools to separate examination, even if it be in the name of unity or, at the least, of coherence – but must act as if the specific methodologies that define it had always been conceived of as part of a scientific continuum in relation to a single, complex object?

This series of questions emerges not only in regard to the above-mentioned propositions but also from other postulates and opinions expressed in regard to various other disciplines.

According to the archaeologist M. de Bouard (1975, p. 15), “the digger should be an historian; otherwise, he or she may not be able to appreciate the significance of the materials that are found. Nor is it reasonable to suggest that the archaeologist can carry out the dig, even if he or she is willing submit the findings to the prudent judgment of the historian. This division of labor, which is advocated by some, is on no grounds admissible, because the work of interpretation does not begin with the end of the dig; it is begun and reborn, day after day, at the heart of the dig, and creates working hypotheses that influence the orientation of the work.”

The activity, therefore, is of a composite nature from the start, because work in the scientific continuum cannot be accurately conceived as a series of completely separate steps; rather, it is a constant and essential labor of articulating the relationship between collecting and interpreting.

The ethnomusicologist J. Blacking takes this approach a step further when he discusses the extremely important moment that occurs precisely between the acts of collection and interpretation. This interval, which can be interpreted in terms of succession, is, however, part of an inevitably interdependent totality in which collection and interpretation are but two particular terms of an intellectual aggregate: “the analytic tools [of ethnology and musicology] cannot be borrowed and used as shortcuts to the important truths of ethnomusicology, as electronic devices such as tape recorders can be used: rather, they must emerge from the very nature of the subject under study” (1972).

This assertion is something of a complete reversal of the proposals offered by J. H. K. Nketia: the tardy application of integrative techniques (and the reason for the use of which is not made explicit) would no longer completely invalidate parallel and preliminary work. Rather, submitted to the interdisciplinary approach, these labors would need to be reevaluated because their tools are inadequate to the object.

Is this not also what is implied by M. P. Ferry, when she distinguishes between the paths of the linguist and ethnologist? "The activity of the linguist seems to be the inverse of the ethnologist's: the former, on the basis of the answers he receives to his questions, tries to construct a system that accounts for the functioning of language; the ethnologist, however, as Frake has written, 'tries to find the questions that correspond to the answers that he himself has observed since his arrival.'"⁶

If the approaches are inverse, can the tools of linguistics and ethnography be borrowed and used without a preliminary evaluation? And can this evaluation be sufficient? Or, on the contrary, is not the job of interdisciplinarity to create a means of analysis by "causing the very nature of the subject under study to emerge"? (J. Blacking).

In the face of such questions, we can no longer limit ourselves to listening to various proposals but are ourselves obliged to observe analysis at work; for this is the stage when the comprehensive view and the demand for unity have command of the anthropological adventure: not only in terms of putting into practice specific techniques but in the name of an intellectual adventure of a different order.

But what is the aim of this intellectual adventure, if not to think through the constitutional multiplicity of the object and to attempt to account for it, beyond the limits of any – necessarily partial – separate view?

This amounts to saying that the philosophical demand for unity necessitates the emergence of a mode of thought devoted to defining both the limits of discourse and of particular analytic practices; which amounts to saying that the taking into account of the constitutional heterogeneity of the object is an inevitable and never-ending exercise, necessary to defining the limits of discourse and of the analytic processes; and it is only through this activity that we can envision the ultimate intellectual conditions required by the demand for unity. Once more, this amounts to saying, in this context, that a review of the practical solutions adopted heretofore can only be of partial help if this review does not simultaneously aim at apprehending and enumerating both the obstacles and means to the interdisciplinary act.

Let us then begin by placing what may at first appear to be a near-

6. M. P. Ferry, 1981. The quotation of C. O. Frake is taken from an article that Frake published in 1962 under the title "The Ethnographic Study of Cognitive Systems," in the volume *Anthropology and Human Behavior*, edited by J. Gladwin and W. C. Sturdevant.

ly insuperable obstacle to any comprehensive approach: specific analytic tools are terribly necessary, but their use, when unaccompanied by adjustments required by the analytic situation, can be terribly mutilating. We should therefore add to this observation that the specific tools of analysis cannot be left as they are. Because of the composite nature of the objects, and thanks to them, and also because of the demand for unity, these particular tools must be analyzed, reevaluated, and transformed.

The intrinsically complementary nature of the tools must therefore be conceived, apart from all intellectual naïveté; and the composite nature of these tools must be thought through. The necessary co-existence and articulation of both specificity and comprehensiveness ought eventually to lead to an evaluation of the contents and respective potentials of the tools – potentials simultaneously so different and yet so linked.

Let us now move on to a review of the solutions and positions taken by various authors and schools, in order to illustrate the much advertised recurrence of the demand for unity and the multiplicity of actual paths taken in order to express these facts, as well as a review of the varying quality of the results of each. By doing so, we will be able to observe the never-ending revision of measures specifically devoted to a comprehensive view and be able to describe the development of this comprehensive view itself. Finally, we will observe how this view is displayed when interdisciplinary activity reflects upon the conditions of its existence and efficacy.

Analyzing and Articulating

How can the different approaches be articulated when the practitioners of these approaches are themselves often trying to clarify the paths that they have used in order to carry out their analyses? Also, how, and on what material, are we to clarify the approaches? How, and on what material, are we to articulate our ideas?

An excellent, and relatively recent, example of a comprehensive approach is to be found in the study that H. C. Conklin and J. Maceda devoted to the music of the Hanunoo people (1971). In this study, each specialist analyzed the subject from the point of view of his or her specialty. The results that were obtained were then juxtaposed. Yet the object remained divided. An intrinsic unity, which was to overcome the permanent exteriority of the constitutive elements, was, however, indisputably sought. The presentation of cul-

tural data was then followed by the presentation of "vocal and instrumental forms of the sonorous production of the Hanunoo people" (p. 190). And this presentation was itself organized on the basis of "large cultural contexts" (p. 190), although the approach in fact appealed alternately to linguistic, literary, organological, and sociological factors, without failing to mention aspects that bore relation to what, in English, is called *performance*.

This first accumulation of data, composed of general cultural facts and the forms of their production, was followed by a second, devoted specifically to music. This analysis strove to be musicological; yet the starting point for the analysis of specific data turned out to be either functional, sociological, organological, or tied to musical genres.

The comprehensiveness of the view was achieved through a successive reading of data, analyses, and the results of the analyses. It was also derived from nuances detected by the analysts in each part and sub-part. However, the question of its intellectual components was not raised, although the material under question prompted at least two different kinds of questions: how, on the one hand, can ethnic music be analyzed when all the tools at our disposal were created in order to analyze musical products of a completely different type? And, on the other hand, how can we justify the never-ending and nonsystematized mixing of data of diverse orders in the name of an analysis of *one* of these orders?

In other words, if a discipline must resort to using materials of another order in order to analyze materials that concern it directly, how is it done and on what basis? By resorting to something other than one's own material, are we not summoning primary materials of one kind in order to answer questions that concern other primary materials; or does one discipline appeal to another in order to be able to say something that it cannot say by itself? And is this solicited aide necessitated by an intrinsic difficulty or rather by methodological laziness?

It is clear, in regard to ethnomusicology, that a vast enterprise of conceptualization is necessary, because of the recognized deficiencies of the analytic tools employed in the analysis of ethnic music (C. Seeger, 1958; A. Merriam, 1964; B. Nettl, 1964; S. Arom, 1985). Given that this conceptual labor has been late in coming, is it any wonder that researchers are presented with the daily temptation of turning to external materials and to methods not strictly musicological? Social categories, cultural norms, and the spontaneous ideas of the researchers themselves become the means by which musicology

approaches its sonorous material. But can these substitutes, which are themselves heterogeneous by nature, legitimately account for material of a totally different order? And does not the mixing of orders hinder the development of a clear conception of the identity of each of them, and of the conditions of their expression?

At bottom, it is these questions and, more generally, their implications that bear on two reflections relative to technology and oral literature (R. Cresswell, 1972; G. Calame-Griaule, 1970). Although quite separate, these two areas have in common the fact that they unite facets of an object that is diverse by nature; that is, each of whose facets is irreducible.

Technology begins by manipulating a collection of primary materials. These materials are worked on and transformed, as are its agents and the means of the work. Equally transformed are the uses of the material and its technological ends.

Oral literature is first defined as a collection of textual data, the nature of which is variable. These data can simultaneously be defined by their form or by the contents that they either convey or imply. These include sociological data, as regards the agents directly concerned and the condition of the circulation of the texts, and cultural data (extending from the uses and functions of oral literature to the impact and more profound aims of this literature). How, under such conditions, can we account for the data of each order? And how can we articulate them?

To this end, R. Cresswell makes use of three successive intellectual structures. The first was created by A. Leroi-Gourhan and allows Cresswell to study the tools, techniques and technical ensembles "on the basis of their inherent characteristics and not through a grid developed on the basis of extraneous considerations: economic, political, or cultural" (1972, p. 23). Having carried out this analysis, he finds that not only are "the technical balance sheets and the description of tools and complexes of production" now available, but also "a model of the technological totality of each society" (p. 24).

When dealing with questions of ideology and symbolic systems, Cresswell turns to the theories of Claude Lévi-Strauss. This activity leads to "placing the model of the technological totality side by side with the model of cultural manifestations" (p. 25). But why choose Lévi-Strauss? Answer: for theoretical and methodological reasons, and in spite of the limitations that Cresswell discerns in Lévi-Strauss's intellectual edifice: "the models he builds account not only for the deep structures that underlie the manifold and unique char-

acter of daily life, but also for the transformations and the processes of transformation inherent in these structures" (p. 25). And further on: "By simultaneously directing its attention to the abstract realities that subtend tangible realities and on the dynamic aspect of these realities, structuralist research allows us to acquire an understanding of the factors on which the structure of evolutionary forces hinge; and by so doing, structuralism offers a model that can be compared with the models of productive and technological forces" (p. 25).

It is worth mentioning that it is at this stage of his activity that Cresswell outlines the basic components of the object under study. As for the scientific process itself, this same author has recourse, in regard to method, to an approach that is simultaneously capable of accounting for both the intrinsic and specific nature of each component and for the establishment of relevant bridges between these two essential components whose structures are fundamentally different.

This recourse to a third intellectual construction – the theses of the Marxist school – is not intended to supplement the description of the constituent elements of the object. Rather, its aim is to help in constituting the fundamental *unity* of the total scientific approach. This is accomplished by overcoming the burden of what Cresswell defines as the intrinsic limits of any intellectual structure that is applicable merely to a limited sphere ("*utilisé localement*").

There is therefore a triple purpose to the propositions offered by Cresswell: to identify correctly the components of a complex object; to seek out and find the theoretical point of view and methodological tools most adequate for describing these components and how they are put into practice; and to turn to a complementary group of theoretical and methodological operations capable of transcending the recognized limits of all particular approaches (limited because adapted to each particular component). In all, Cresswell applies the multidisciplinary approach to both the identification and the enumeration of the components of the object; to the study of the intellectual paths best able to account for and articulate these components; and to the necessity of thinking about the limits of these intellectual paths by identifying them and proposing a new intellectual ensemble whose aim is unity.

Although constructed in a totally different way, the reflections of G. Calame-Griaule on oral literature clearly demonstrate how the three obligations enumerated above can help us think about the

question of the demand for unity. Like Cresswell, Calame-Griaule assembles the basic constituent elements of a complex object. Just as the specialist in technology does, the specialist in oral literature explores the question of the most adequate tools for analyzing the different components of the object. However, contrary to Cresswell, Calame-Griaule does not strive systematically to create a basic methodological approach to each of the object's components. At the same time, she leaves unanswered the question of what means would be most likely to transcend the limits that define each of these means taken separately; instead, she describes the various constituent parts of the complex object. To do this, she makes good use of the most effective practices employed in the study of oral literature and is able to discern – in a totally different way than Cresswell, but with equal efficacy – the stumbling blocks that lurk in any interdisciplinary approach guided by a demand for unity.

More importantly, however, a comparison of the two approaches allows us to extend and broaden our inquiry into the conditions that will make unity possible in interdisciplinary discourse. Both the above mentioned approaches are in fact programmatic. One of them, however, asserts that its propositions have succeeded in achieving the aim of unity. The other, lacking a clear solution, does more than offer a mere inventory of approaches. Indeed, this second allows us to glimpse interdisciplinary activity as it is carried out on a daily basis; beyond the theoretical formulations, we see the swarm of dangers and, more importantly, the array of concrete stages, all of which convinces us that the aim of unity cannot be achieved without paying attention to this unavoidable everydayness.

In other words, if the interdisciplinary act is simultaneously a way of thinking that integrates the object and its facets, a way of putting necessarily particular intellectual approaches into action, and a reflection on the limits and exercise of coherency, then how can such an enterprise – so terribly ambitious and so fundamentally necessary, philosophically speaking – be concretely carried out?

In the face of such questions, our only recourse is to seek the help of some evidence that is based, in an absolutely explicit manner, on interdisciplinarity and carried out on an extremely broad scale. The research should also have proved to be fertile in its results and based on materials of different orders that were summoned, analyzed, and evaluated by a large number of specialists whose aims exactly corresponded to the demand for unity. Such evidence is not

totally lacking. We can cite, in particular, the work – limiting ourselves to French examples – carried out in the Aubrac (1970–1975) and Brittany regions (A. Burguière, 1975), and also the vast enterprise of creating linguistic and ethnographic atlases of all France.

These three scientific endeavors, rare examples of an approach that can be defined as both multiple and integrated, are best known for the results they produced. However, they also constitute a fertile field on which to base theoretical, historical, methodological, and pedagogical evaluations, when such an evaluation appears essential to the health of the anthropological sciences (as it does now, in this *fin de siècle*).

In order better to understand the urgency and merit of such a demand, we think it useful to review two other stages in the development of the anthropological sciences. These stages – along with other developments that have produced numerous studies carried out over a long period of time – have produced, or are still producing (in one of the cases), a copious theoretical, methodological and didactic literature marked by a pronounced penchant for analyzing interdisciplinarity in action. The first stage covers the period 1910 to 1940 and coincides with the activities of the Bucharest School of Sociology. The other, more recent, is represented by the totality of work carried out in central Africa by J. M. C. Thomas and extends over a period of more than twenty years.

Professor D. Gusti, who went to Rumania at the invitation of local researchers, carried out his work during a pivotal period: one that saw the triumph of evolutionist or diffusionist theories, and atomistic approaches to analysis; but it was also a period that saw the advent of what might be called “the structuralist age” (J. Molino, 1981, p. 239). By contrast, the studies carried out in central Africa took place when structuralist theories were already in decline. These studies took place in exotic lands and were carried out by European researchers who themselves, however, had contributed in no small measure to the development of certain cognitive theories, although this label, as applied to their approach, is based on different criteria than those that are generally accepted (S. Arom, 1985; J. P. Caprile, 1971; J. M. C. Thomas 1977).

The central assumptions of the two enterprises, as regard interdisciplinarity, were fundamentally different. In the case of the school of D. Gusti, a single discipline (sociology) constituted the cornerstone of an intellectual edifice which, in matters of theory and method, was both fully developed and amply elaborated and expli-

cated. Yet, to this day, this research remains poorly known and its results are difficult to gain access to (F. Alvarez-Pereyre, 1990). In the research carried out in central Africa, language played the central role. Indeed the study of language was the foundation of an intellectual structure whose theoretical and methodological characteristics have been fully studied (J. M. C. Thomas, 1967, 1985, 1987; L. Bouquiaux and J. M. C. Thomas, 1975).

Apart from these differences, which a more thorough analysis could clarify in full, several similarities characterized the development of both enterprises. One was their fundamental commitment to interdisciplinary unity. Most of the other similarities revolve around the idea of the scientific continuum, whose conditions of conceptualization were discussed and illustrated in the research.

Indeed, it should be noted that the Rumanian researchers, along with their French colleagues, systematically and correlatively studied both the theoretical and the methodological side of all relevant questions. They investigated how the data itself was collected; how the results were produced and analyzed; how pedagogical and research activity were carried out; what the meaning of research was in itself and what its social use was. These four ensembles, copious in themselves, were not, however, conceptualized in isolation; each was enriched by the progress and reflections stimulated or developed at the heart of an adjacent inquiry.

All in all, as the researchers investigated both the theoretical and the methodological aspects of the question, they seemed confronted with what appeared to be inevitable choices, although the question of these choices had been, until then, neglected by the majority of researchers. It thus became clear, to both the Rumanian ruralists and the French ethnolinguists, that any method or practice was closely linked to a theoretical edifice; and, consequently, that it was necessary not only to make this edifice explicit but to develop it in light of the daily practice of research on subjects that are always particular.

In any case, they realized that no collection of data could be carried out without a certain predetermined conception of analysis, and that all analysis depended, in some way, on the way the collection was carried out. The production of results depended simultaneously on the way the data was collected and analyzed, but also on elements that transcended these factors (the aims of the research, the public for whom the research was intended, the social setting of the research, and so on), and that this must also be taken into account.

Along with these assumptions, the researchers expressed the conviction that active research into pedagogy was especially necessary, because such research could both reveal more about the data and clarify the meaning of the act of research; also, that any concrete act of research that was fundamentally indifferent to the pedagogical dimension would become, in the long run, narcissistic and therefore stilted.

Finally, according to these two scientific enterprises, any research activity that suppresses the question of its aims and social setting – or refuses even to admit that it could be influenced by its setting – is also inevitably blind to its own existence and development. Inversely, an interest limited strictly to the sociological setting of the research – which would constitute an excuse for turning research away from its intrinsic duties – would lead to a damaging intellectual schizophrenia.

Let us now take a closer look at the implications and concrete meanings of these different choices. In the first place, as a result of voluntary and daily interaction with other disciplines devoted to the same object, both the Rumanian and French researchers began to perceive both the necessity and the problematic nature of the specific activity inherent in each discipline.

This interaction itself was expressed in many modes, some of them used concurrently: group travel to the same research site; frequent meetings and comparisons of the gathered data; synthesis of analysis during initial, intermediate, and final stages.

Secondly, this double movement, toward specificity and interaction, was clearly one of the essential engines of the synthetic view that developed, and of which the least that can be said is that it was not a result of simple coexistence or a succession of stages. Rather, this synthetic view was formed on the basis of specific imperatives tied to procedures of evaluation. We must include, among these imperatives, the act and the concern for pedagogy, which was manifest from the beginning and took various forms. It led to a period of self-evaluation in which each researcher was forced to project outside him- or herself the nature and techniques of his or her activity.

This led to a third phase, in which pedagogical analysis was joined to the immense labor of the elaboration of methodology, and this was a phase to which both scientific movements devoted much time and effort. The results of this elaboration were numerous and varied, although methodology never assumed the role of an aim in itself but rather remained the most subtle and systematized phase

of a continuum in which theory and practice, together, played an essential role.

Another kind of evaluation was used to supplement the analysis of pedagogic activity and methodological construction: this activity was a result of the constant and explicit comparison of the respective conditions under which data was gathered, how this gathered data was portrayed, and the use of the data to the aims of science, parascience, and altogether non-scientific activity.

Finally, this synthetic approach was enriched by an explicit inquiry into the means of access to the object in its totality – an inquiry that was taken up again and again in the course of scientific activity.

It is now clear that the fundamental and essential originality of these two “schools” was to have made an explicit duty of revealing all the ingredients, all the stages and components, that – whether we like it or not – constitute the activity of research; the research also, and perhaps especially, demonstrated that each researcher, face to face with the object, is left to choose between two fundamentally different types of approach.

Either the researcher can treat the object in a partial manner, ignoring or suppressing the ties that inevitably link this partial view to the overall view of the subject that the researcher cannot help but have, or he can make the inquiry into the overall view of the object a fundamental part of his own scientific activity: not simply as elements interesting to have a look at in themselves, but as the driving force behind a dynamic system whose inevitable impulse to unity can be the springboard to what becomes the duty to achieve unity. However, this unity can only be attained if the researcher remains ever cognizant of the numerous and powerful temptations to shirk this duty.

As a result of having chosen to approach the object comprehensively, these researchers exemplify quite well the four sides of a solid structure on whose foundation the requirements of unity become, if not operational, at least discernible. The four sides of this structure, let us recall, can be formulated in the following terms:

- a way of thinking that integrates the object with its facets;
- the ability to activate and direct the simultaneous use of several specific intellectual tools and approaches;
- the ability to conceive the limits that are a part of any specific intellectual tool or approach;
- achieving coherency of discourse.

Beyond the numerous appeals to interdisciplinarity and the many concrete examples of interdisciplinarity in action, there remains the need for more work, that is, for more interdisciplinary efforts. However, as D. Gusti, J. M. C. Thomas, and their colleagues have shown, this is no easy task; the work of interdisciplinarity is long and complex, and if it hopes to succeed it must rely on an overall framework that suppresses none of the stages – large or small – of the scientific act, since it is an act of man acting upon himself.

In this context, interdisciplinarity cannot be allowed to degenerate into a dangerously inexact mixture of approaches, nor should it approximate a simple process of fusion (which in fact is neutralization). Rather, it should consist of the systematic actualization of the four stages enumerated above, and each of the stages must itself consist of multiple phases and processes.

Without such links we would be unable to explain, for example, the fruitful cross-fertilization of the theories of Gusti (1941) – themselves unreceptive to the historical dimension – with the materialistic assumptions upon which the research of his colleague and direct collaborator H. H. Stahl was based (1971). This cross-fertilization, which was the result of a preliminary and explicit acknowledgement of the need for interdisciplinary unity (without either of them deluding himself about the complexity of the effort), in fact led both of them to surpass and reformulate their respective theories.

Without these same links we would be equally unable to account, for example, for the similarities and differences (as well as the causes and effects) between the work of Thomas (1988) and C. Hagège (1978) in regard to description and linguistic theories, or between the research and theses of S. Arom (1985), S. Bahuchet (1985), G. Drettas (1980) or M. M. J. Fernandez in regard to intrinsic descriptions and relating data of diverse orders.

The time has come for a method of pedagogy devoted to the analysis of the scientific act, and this pedagogy implies at least one preliminary activity: the description and evaluation of effective interdisciplinary practices, whether carried out recently or in the more distant past. This pedagogy, devoted to the analysis of the scientific act, will also cause us to rethink the pedagogy of the scientific act itself, whether or not it is explicitly multidisciplinary.

All that remains to be said of this intellectual activity is that it will reward anyone capable of coupling the essential specificity and intrinsically comprehensive nature of the object.

If interdisciplinarity is "a mode of intercourse among specialized scientific practices," it is too often carried out at the cost of obscuring the necessity for unity; and although an awareness of this necessity is frequently present in scientific inquiry, interdisciplinarity is rarely mastered because so often blind to itself.

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