

Methodological Concerns and the Progress of Ancient Near Eastern Studies

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It is in the nature of a developing field that, as its scope and documentary basis become wider, so too does the range of approaches to the subject matter become more differentiated. Progress can be measured, in other words, not only by the quantitative increase of the data available, but also by the quality of the reflection with which the researcher approaches the corpus. The rhythm of such progress naturally varies as the conditions for research vary. At times it may become so difficult to keep up with the expanding body of new evidence that little time is left for anything but the necessary task of providing an adequate publication of the material at hand. At other times, instead, whether or not the flow of new factual material continues with the same abundance, creative interest in attempting a deeper analysis of the evidence becomes imperative, and a more decisive effort is made at securing new vantage points.

The latter description is a rather fitting one for the current stage of research in the study of the ancient Near East. Next to the sense of discovery so characteristic of many branches of the field, where excavations and museum storages are serving as an inexhaustible source of new items for the scholars, there is a definite ferment in the way in which various attempts are made at new interpretations of the data. With Vico's apposite terminology, one may say that, besides securing the data as certain, the necessity is felt more and more to "inverare il certo", i.e. to bring out the inner "truth" of what has already been "ascertained" as factually correct.

Preparing a volume of studies to be offered to one of the major protagonists of the recent history of ancient Near Eastern studies, Ignace J. Gelb, seemed like a most appropriate opportunity to pause and take stock of the situation. Appropriate not only with respect to the field, where the rapid development in all directions calls indeed for some probing self-inquiry, but also with respect to the scholar we intend to honor; for his work has been exemplary both in the mastery of the traditional tools of the craft and, precisely, in the sustained and successful effort at opening new vistas.

It is no accident, then, that the title of the present volume echoes that of the presidential address delivered by Gelb at the American Oriental Society in 1966: "Approaches to the Study of Ancient Society". There he described, with the enthusiasm which accompanies a truly creative endeavor, the development of his research interests along new lines: a reconstruction of social structures and institutions which utilizes the insight of sociological and anthropological theory. Following this manifesto, the results of his socio-historical research

have begun to appear in the form of articles which are building up to a major work on the social and economic history of ancient Mesopotamia.

Such recent, seminal work in the field of history is all the more remarkable in that it dovetails with another type of research of quite a different orientation, namely his work on linguistic matters. Here too he has broken ground in a most innovative and authoritative way, by keeping abreast of general linguistic theory and utilizing it for a more insightful and accurate presentation of the data. The culmination of his work in this area is represented by the volume on *Sequential Reconstruction*, which appeared in 1969, three years after the Philadelphia AOS address. Interestingly, the application of new methods in the field of linguistic research has also been combined with the utilization of new techniques which are only gradually entering our field of studies, i.e. computer techniques: Gelb's Amorite project, though largely still unpublished, draws on the resources of electronic data retrieval, and promises to be a significant contribution in terms not only of specific results, but also of approach to the problems.

Social history and linguistic research are the two areas in which Gelb's concern for proper methodology has most prominently come to the fore in recent years; but much of his earlier work also bears the mark of a keen interest toward defining a proper research strategy, particularly when entering uncharted territory. It will be sufficient to mention, in this respect, his work on Hittite hieroglyphics and the book on writing (first edition in 1952) — the latter giving clear evidence, in the subtitle, of the author's concern for developing an overall theoretical systematization: "The Foundations of Grammatology".

It is not, however, my purpose to provide an assessment of Gelb's scholarly achievements — a task which would vastly exceed my capacity, as the wide range of interests reflected in his bibliography, given in the preceding pages, makes immediately clear. I would only like to indicate how an attempt at reflection over the best line of approach to the study of the ancient Near East is consonant with the interests apparent in Gelb's own research, and thus is a suitable topic for a volume offered in his honor. I would like to stress as well, admittedly with a strong element of personal indebtedness, how effective he has been in transmitting the concerns of his research through his teaching, from the very initial stages of the sequence of courses he offers, and from the closeness with which he follows his graduate students in their progress toward independent research, to the unfailing readiness with which he gives of himself to his younger colleagues as they go to him for advice and direction. The recognition which comes to him in return, and of which this volume is a small token, is instinctively felt not only in terms of an acknowledgement on the scholarly level, but, more deeply, as the result of a warm human relationship. A sure sign of this is that Gelb's school not only has its roots in the classroom and in his office, but it extends also to include his home, where his students have traditionally been welcome as friends. There, too, they learned to know and appreciate Hester Gelb, whose perspicacity and warmth have been unfailing on all occasions and have contributed in the highest degree to her husband's career.

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In more general terms, the decision to put together a volume unified by a central theme stemmed from the desire to present a tribute whose value would lie not only in the merit of each contribution taken in itself, but also in the

intrinsic interest of the volume as a whole. The collaboration in the volume could not naturally be conceived as the result of a rigidly planned team work, but rather as an ideal symposium cutting across distances of space and time. Some of the articles are less explicitly methodological, and are thus integrated more loosely within the scope of the volume. Other contributors, instead, have taken more squarely a stand in front of the central issue of method, and provide not only a *status quaestionis*, but also an evaluation of the attempts at methodological innovation and some practical indications of the results which may be obtained.

The nature of the coverage is uneven also with respect to the fields within the ancient Near East. As most of the contributions come from Gelb's colleagues and former students, it is natural that those areas are emphasized which are closer to Gelb's own field. Yet in some cases it has seemed advisable to branch out and ask for the collaboration of other scholars in an attempt to have some major fields represented where important innovations have been introduced. That this could have been done even more often hardly needs mentioning — there are admittedly major areas which are not included in the volume, to some of which explicit reference will be made in the rest of this introduction. All in all, the volume aims at offering, selectively, an assessment of some of the areas where an explicit reflection about methodology may contribute significantly toward an improvement in our understanding of the data.

When speaking of methods and approaches it is natural that attention should be given mostly to new departures, those which are still being tested and from which not all implications have been drawn. It must be stressed, however, that the goal of the volume is by no means to press innovation for innovation's sake. Far from it; the usefulness of mature reflection becomes all the more manifest in a critical appraisal which can underscore limitations and even drawbacks of novel developments especially if these are embraced with too quick and naive an enthusiasm. The thoughtful remarks by Gragg in his contribution to the present volume are particularly enlightening in this respect. For an application of methodological refinement to be fruitful, it is necessary to fully understand the validity and the scope of the new tools to which one sets one's hand—or else we may end up by being engaged in little more than a rephrasing of previous insights, only in more fanciful form, and with the disadvantage of a uselessly complicated jargon. If terminology is to serve a purpose, it has to stem from the inner working of a system, and not become simply a form of window dressing.

On the other hand, innovation should not be dismissed simply because it is a new departure, or because the terminology is not immediately transparent. It is incumbent upon the scholar to seriously evaluate alternative approaches to his method of research, rather than simply choosing to ignore them. This point is made forcefully by Callender (below p. 65). On the basis of an informative review of the historical traditions of language studies as applied to the ancient Near East, he comes to an interesting and perhaps unexpected conclusion: by ignoring modern linguistic theory and opting for the traditional approach, one would in fact be going *against the tradition* of our branch of Orientalism which has been, *by tradition*, ready to apply to the data new theories of grammar as they were developing. Ironically, then, one would be antitraditional in spirit even as one is trying to remain close to the letter of the traditional doctrine.

The reason which often underlies a noncommittal attitude in the face of new developments is the very complexity of such new developments: this is

particularly true not only where new technical tools require a whole new range of expertise, as in the case of new dating techniques (see especially the contribution by Berger and Protsch) or the use of computers (Segert and Hall), but also where wholly new conceptual models are introduced, in particular those with mathematical and statistical underpinnings (Fronzaroli; Kelly-Buccellati and Elster) or those with a complex symbolism, as in the case of contemporary linguistics (Callender; Gragg). There are well established disciplines behind each one of these approaches, and it is only natural that they should have developed their own frame of reference, which includes not only a special terminology, but also a special inventory of conceptual categories and research strategies. Complexities of this type are not immediately accessible without some particularized study; yet if no deliberate effort goes into just this kind of study, the gulf may open wider and wider between the competencies of what may be called the area specialist on the one hand and the theory specialist on the other.

To help bridge such a gap is precisely the intent behind this volume, where scholars who are primarily area specialists have concerned themselves with the clarification of points of method, and not so much from the viewpoint of theory as rather from that of area studies. It was interesting to note how in the correspondence with the editor some of the authors underscored the difficulty inherent in dealing with questions of method from a theoretical point of view. This observation (which, it must be stressed, has normally been a measure of the commitment with which the individual author has faced the central topic proposed for the volume) is indicative. There is a certain reluctance to deal with matters of theory because it is felt that the discussion may remain sterile; also, that there is an uncomfortable chasm between someone who confronts directly the subject matter of a discipline and someone who looks at a discipline from a distance and writes *about* it rather than within it. In point of fact, the output coming from ancient Near Eastern studies is rarely of a type where data are used to establish a theoretical system—unlike such other fields as linguistic description of English which has served as a channel for the development of transformational theory, or the elaboration of excavated data from American sites which has been at the basis of the so-called “New Archaeology”. Naturally there are exceptions—one which obviously comes to mind in this context being Gelb’s book on writing (1952), another being the work by Petrie on seriation and by Meyers on statistical archaeology mentioned below by Kelly-Buccellati and Elster; but they remain exceptions. Otherwise there is a restraint toward theory per se, a restraint which, typical of the discipline, is naturally reflected in this volume, where the main concern remains the practice, not the philosophy of Orientalism.

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Practically, then, how does a discussion about method serve to the progress of the discipline? Many contributors have tested the applicability of the theory by means of some concrete illustrations. The improvement in our understanding of the specifics is a sure gauge of the validity of the system used—in other words, does a textual passage or a set of artifacts become more meaningful as the result of a proposed new approach? It will be for the reader to judge the measure of success achieved in trying to bear this out in the present

volume. Here I would only like to call attention to some more generalized considerations which can be made in this respect.

For one thing, an active concern with methodology can have a useful heuristic effect in that it suggests new testing grounds which may widen the present horizons of research. Gragg has put it well: new methods do not claim to provide an answer to all old problems, rather they are meant to raise new meaningful questions (below, p. 86). Thus the study of language has stirred new impetus for the investigation of syntax (see especially the articles by Gragg and by Callender) and, perhaps unexpectedly, for a new understanding of the cuneiform writing system viewed in his function as a communication medium (Civil); application of statistical models has suggested new parameters for the evaluation of relationships among languages (Fronzaroli) and artifacts (Kelly-Buccellati and Elster); anthropological interest for social systems and institutions has proposed patterns against which even our fragmentary body of evidence can be usefully tested (Rowton; Renger) and has, at the same time, prompted a novel type of "Quellenkunde" whereby we take a critical distance from the documents in order to come in effect closer to a proper understanding (Liverani); and phenomenology of religion has provided a set of assumptions for interpreting isolated manifestations of the religious spirit as fitting in a meaningful whole (Jacobsen). Naturally, syntax, comparative Semitics, systematic ordering of artifacts and history of institutions or of culture are not new in themselves; on the other hand the search for criteria of analysis germane to the structure of the object of research (e.g. primary syntactic structures rather than derivatives of morphology), the emphasis on quantification as a criterion for more precise and reliable assessments, the interest in systems as organic wholes within which one may better understand the parts in their interrelationship of contrast, complementarity and symmetry—all this does, in fact, trigger mechanisms of research which lead to unexpected conclusions.

While asking the right question provides already half the answer, and while theory is a good source for the formulation of just such questions, yet another reason why interest in method can be fruitful is that it allows for a sharper focusing on, and a clearer definition of, the terms of a given problem. Naturally, every scholar operates within the framework of a methodology of sorts; what varies is the degree of awareness of one's own presuppositions, which may remain more, or less, articulate. Clarifications along these lines undoubtedly help to provide a sense of perspective with respect to limits and possibilities. In practice, this means that confusion can be lessened if sharper distinctions are introduced; especially, it means that mistakes can be avoided if wrong assumptions are exposed. A case in point is the contribution by Civil which in line with its programmatic intent, not only lays the groundwork for a reexamination of Sumerian writing in a thorough and systematic fashion, but also dispels commonly held opinions which are often based on uncritical assumptions. Similar results are reached by other articles, which go a long way toward consciously defining the goals of research in given areas—as with Biggs cautioning against the use of palaeography for dating purposes especially if regional variation is not sufficiently taken into account, or von Soden calling for greater differentiation among types of root, or Sollberger reflecting on the limits and possibilities of translation. Similarly, the notion of morphographemics and morphophonemics (as employed by Reiner) or that of contrast among the structural components of a system (Edzard; Kuryłowicz) allows for a much sharper and more precise

description of the data, which otherwise remain lumped together in a less differentiated and more opaque picture. In most of these cases satisfactory results can be obtained without the need for an explicit elaboration of the underlying methodology; the concepts are simply proposed and justified on the basis of their practical applicability to the material at hand. As von Soden has well pointed out (p. 144), it is necessary to tread the thin line between an empirical and intuitive procedure on the one hand and, on the other, a careful "Nachdenken" which is only possible if one takes a certain distance from the object of research. Every type of research is based on what may be considered a generic methodological consensus within the discipline, and on such foundations, empirically, each one of us can normally proceed. Yet at times the accepted platform may reveal itself too thin for given conclusions and that is the time when a deeper inquiry into the supporting methodological scaffolding becomes necessary.

A successful way to achieve the desired balance between an empirical and a reflective trend is to graft, as it were, on our discipline procedures tested already in other fields. Thus the effects of new methodology are filtered through the practical applications which have borne fruits elsewhere—and then, rather than focusing on method per se, we might speak of comparative approaches. Here the question posed at the beginning of this section—"How does a discussion about method contribute to the progress of the discipline?"—can be slightly rephrased and made more concrete: if a given method has borne good fruits in other disciplines, how can it best be applied to our data? In a way, this is generally the case whenever speaking of method, since methodological perspectives are not normally opened up within a rarefied theoretical atmosphere, but rather in connection with a given body of data. If we want, however, to draw a distinction between a comparative and a methodological approach, we may say that in the former there is more concern with the results and the practical way of arriving at them, while in the latter more stress is laid on the conceptual fabric itself. This brings us back to the point made earlier about theory and practice; and in the light of what was said there, particularly with respect to a certain distrust for theoretical elaborations, it would appear as though a comparative type of methodological applications would be likely to be preferred in ancient Near Eastern studies. In point of fact, this has normally been the case in the past—juridical studies of Mesopotamian legal texts being one outstanding example, about which Renger speaks at some length in the first part of his article. In this area, the presuppositions which are at the basis of the study of Roman law are accepted as a workable methodological framework for the study of Mesopotamian law, and applied to that body of data, with little or no specific concern for method in and of itself. Comparisons, then, can be fruitful even when they bear on segments of the system without a specific and systematic analysis of the ways in which details of method are derived from basic underlying principles.

It is also from a careful study of the results achieved in other areas of study that approaches as yet unattempted will probably come to be considered desirable. The present volume can only begin to describe some of the areas where a reflection on methodological presuppositions seems in place, especially those where current research shows notable and rapid advances. Of those which are left out, only one or two will be mentioned here by way of exemplification. The field of literature is one which would lend itself to important results, in part anticipated by the contributions of Gevirtz and of Liverani; a refined set

of critical tools could be brought to bear on the extremely rich material which scholars have endeavored so far to clarify from a philological standpoint. Such an approach, for which considerable experimentation can be said to have been introduced **only** in Old Testament studies, would give us a **more** satisfactory insight into the nature of the text. At the same time, a **definite** contribution could be made by our field to the general discipline of comparative literature, especially given the antiquity of the material with which we are concerned; the weight of conclusions based on it would be considerable in assessing the proper value and function of literary categories which in part at least can be traced back to the growth and development of writing, the **main** medium for giving permanency to a characteristic mode of expression of the **human** spirit.—Equally promising are the developments in archaeology which go under the name of "New Archaeology", touched here only in passing in the article by Kelly-Buccellati and Elster. With its emphasis on problem orientation, it stresses the importance of a planned approach to the fact of excavating, which should be inserted, more decisively and consciously, in an overarching theoretical model where the very first turn of the spade is conditioned by the same set of problems which are reflected in the **final** report.—Or one may think of areas which a certain amount of amateurish and unprofessional research done in the past would seem to have **wrongly** damned as unbecoming for a professional scholar. A case in point is the application of psychological and psychoanalytic theories to historical analysis, which in some quarters may at first smack of fanciful and subjective exaggerations, but has in fact begun to establish itself (outside our field) as a serious discipline generally known as psycho-history, with its own well tested and exacting methodology and with convincing and enlightening results.

In what has been said so far, method has been essentially understood as a given conceptual scheme for ordering the data. (For more explicit remarks on the notion of method per se see Gragg below, p. 84). We must now consider, briefly, those cases in which the new tools consist of technical facilities which allow the **formulation** of questions one would otherwise not even know how to ask. The one obvious innovative tool which comes to mind in this respect is the computer, which expands in practically a boundless way the reaches of the human mind in its effort at coordinating and correlating data. Even though the improvements made possible by electronic data processing are really only of size, not of substance, the **impact** on thought processes has been such that new theoretical approaches have in fact been born from computer applications. Because of its relatively young age and, at the same time, because of its enormous resources and capabilities, electronic data processing has given rise to its own folklore; sometimes this reduces the possibility of a rational utilization of the tool and makes the very term "computer" sound like a magic word which, like anything magic, both fascinates and repels. Now there is no doubt that the computer will rapidly become a matter of **fact** tool for any type of research where the correlation of data is important, and the corpus sufficiently vast. Correspondingly, there is no question that we have to **familiarize** ourselves with its potentialities and its limitations, or else run the risk, to use Calender's words (p. 73), to dead end in "an increasingly sterile methodological *cul de sac*". As with any other technical innovation, it is important to properly appreciate the performance range of the machine—with the danger, otherwise, of falling flat on our face. To draw on the analogy of another machine which

has played an important role in the development of scholarship: obviously no one would raise objections today to the use of the printing press, but no one would use it for tasks for which its powers are excessively superior—no one, would use it, let us say, to produce his weekly grocery list. Yet in terms of electronic performance range, some projects can be likened to putting a grocery list through the press—with the expected resulting waste not only of money, but also of intellectual resources. It is only natural that scepticism should result from such attempts; but, to be salutary, the scepticism should be addressed to the approach used, not to the potentials of the techniques. Otherwise we do injustice not so much to the field of computer science, as to our own field of research. Some indication of the richness which the computer holds in store is contained in the two articles by Segert and Hall for language, and by Kelly-Buccellati and Elster for archaeology. As the potential impediment of overlong calculations is reduced or, in fact, annulled, and as working hypotheses can receive an immediate answer by the unrestricted speed of electronic computation, the inclination to quantification of the data increases, specifically when the total amount of data becomes too staggering for human control.

Another instance of a technical tool which is playing an important role for the fixation of absolute points on our chronological scale is radio-carbon dating, which has undergone considerable changes since it was first introduced and still now cannot be considered to have reached a definitive stage. The article by Berger and Protsch is precisely an indication of some of the areas where further refinements of the techniques can be pursued, and also an indication of the types of results which we can still expect.

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Underlying the concrete methodological options which have been briefly described above there are basic attitudes which are more difficult to define. Broad terms come to mind in this respect, whether one thinks of an anthropological approach, or of structuralism, or the like. With this we are removing ourselves one step farther, as it were, from the data as the object of study for the scholar, in that we speak not just of specific criteria for manipulating the evidence, but of basic goals which give a sense of direction and finality to the entire research endeavour. Here, in a way, the term approach stands not so much for methodological as it does for philosophical presuppositions. Even so, the problem has a definite bearing on the concern which is central to this volume: how do we approach our discipline? Hence it seems fitting to give it some attention in the present context, however preliminary and generalized the level of discussion will have to remain (and even at the risk of oversimplification).

A common denominator to many of the more recent trends of research is to view the object of study as an organic whole, where the interrelationship of segments is as important as the segments themselves. The parts should not be isolated and divorced from the context in which they are naturally embedded: rather, their degree of association should be analyzed both in its own right and because it sheds light, in turn, on the individual elements in themselves. In other words, the system or the structure is as important as its components. This principle (effectively stressed by Jacobsen below, p. 275) applies to all dimensions of the discipline—whether we study the political and socio-economic order (history), the inner spiritual experience (religion), the data of material

culture (archaeology), the main communication medium (linguistics), or whatever else.

An immediate corollary of the fact that increased attention is paid to systems is the avoidance of value judgements relating certain aspects of the world we study to our own sense of appreciation. If we like a work of sculpture, or perhaps the entire tradition of figurative arts of a given culture, we tend to absorb it easily and make it part of our experience, we may even consider it paradigmatic and call it "classical". If instead we don't like it, we reject it as incompatible with our taste, and label it as primitive, barbaric, or the like. Such disparaging terms mean in effect that the products of a given culture are heterogeneous with respect to our sensitivity, and that our set of mental categories cannot properly assimilate what this other culture has to offer. It must be stressed that when aspects of a given culture are considered acceptable, we normally deal precisely with single aspects only — the revival of classical antiquity in the Renaissance, for instance, was in fact the revival not of classical antiquity, but simply of certain aspects of the artistic and literary tradition, at the exclusion of social, political and religious dimensions among others. From these considerations it appears already why, if emphasis is diverted from the single aspects to a study of the system as a whole, value judgements which would normally bear on single aspects naturally fall in the background. But, more importantly, the very notion of value judgement with reference to our own set of values is called into question: for the criterion of the validity of a system and its justification have to be derived from within, not from without. Specifically, one will look for the degree to which a system is integrated within itself as an objective means of assessing the inner working of the system ("objective" in the sense that no reference is consciously made to the "subjective" standpoint of the researcher).

As already indicated, this attitude reflects a far-reaching concern which is operative at all levels of research—with the social scientist, the anthropologist, the linguist, and so on. (For the sake of simplicity, in the following discussion I will subsume these categories under the single label of "anthropology"). The contrast which comes readily to mind is with a humanistic approach, but this contrast should not be taken superficially to mean that there is a natural progression from one to the other, in the sense that one approach supersedes the other. Rather than as evolution, the relationship between the two must be envisaged as one in which the two poles are irreducible and equally important for a truer understanding of the data. The humanist, in other words, is not the ancestor of the anthropologist: his approach survives not as a fossil, but with full justification and productivity next to the anthropologist's. Perhaps a concept which can help to clarify the issue is that of immediacy. In the reconstruction of a system we purposely prescind, as we have already seen, from relating the system to our experience; to take the concrete example of a social system, we may reconstruct it in its inner functions and operations, but we do not attempt to reenact and relive its concrete realization which is forever beyond our reach. There is, in other words, no immediacy in our contact with the system: we attain the system through the medium of our own reconstruction which provides a conceptual scheme observable in abstract, and not a concrete situation in which the system acquires flesh and blood. By contrast, if we take a product of the artistic tradition, for instance, such as a sculpture, the object is in fact immediately and concretely present to us as it was to the individuals

of the society for which it was first intended. This is to say, naturally, not that the reaction, but simply that the stimulus is one and the same for us as for them. For all the difference in time between the original date of the object and that of our own observation of it, there is a moment of immediate contact when we face the object as is, and as it has been since its inception. True, this relationship to the object prescind from the overall social and artistic context in which the work was originally inserted, which was indeed part of the work itself—and which can be in part restored through the medium of historical reconstruction, i.e. precisely without any degree of immediacy. But, to the extent in which a response to the object is conditioned by its concrete and physical characteristics, to that extent our contact is immediate and direct. It may be noted, at the same time, that the abstract reconstruction of the web of relationships within which a concrete object was enmeshed from its first conception will also help toward a better appreciation of its concrete message to us today: the tools of an anthropologist will, in other words, help refine and train the sensitivity of the humanist. In the case, for instance, of figurative style, if proper attention is paid to the formal idiosyncrasies and preferences in the tradition from which a given object stems, our sensitivity can become more properly attuned and the message which is in store for us richer in value.

From this point of view one understands why the humanist is tendentially concerned with objects of study in which the documentary vehicle does not so much give evidence of something beyond the document itself, but rather is in itself the primary world to be explored. (Similar consideration, though aiming in a different direction, are insightfully put forth in the article by Liverani). Thus the humanist will be especially attracted to literature or the arts, the anthropologist instead to social and political systems or the like. Tendentially, one will try to approach a field with a bias in favor of one's own vantage point: the humanist will tend to view history as literature, the social scientist as the reconstruction of interlocking systems. Occasionally a contrast may ensue where one approach appears to be followed too rigidly at the exclusion of the other—as with the *topos*, cherished by the language scholar with a humanistic background, of the linguist who cannot speak a language he studies; in point of fact, a linguist may be so concerned with the conceptual scheme through which he reconstructs the structure of the language as to lose interest in appropriating for himself that particular language as a means of expression: but conversely the traditional scholar may be so interested in the cultural aspects disclosed to him by the medium of language that he neglects to consider the medium as a worthy object of study in itself. It appears then that certain fields lend themselves particularly well to both types of approach: besides language, we may mention especially religion, which can be viewed at the same time as a system of beliefs and operations retrievable only in the abstract without attempting a concrete reenactment, and as the exploration of the way in which man categorizes his spiritual world, thus attaining values which may be considered universal and of direct import to us.

The contrast between an anthropological and a humanistic approach (a contrast, incidentally, which is ironically belied by the etymology of the two words, both of which are derived from a similar notion of "man") is then not one of good vs. bad, but rather one of polarity between two equally legitimate, and equally necessary, orientations. They represent overriding concerns in the field at the moment, and their reflection can be seen in most types of research,

but it must be understood at the same time that they do not cover the entire spectrum of possible approaches and that there are in fact topics which are rather neutral with respect to the distinction on which we are here insisting. For instance there is ample necessity, it must be obvious, for studies which simply make available the data and provide a *prima facie* commentary without overtones of either an anthropological or a humanistic vein. Some authors would use the term "philology" to refer to this type of research, and it might perhaps be useful to retain the term in this specialized sense, referring, that is, to an approach whereby the primacy of the text stands out most (and for "text" we might broadly understand any document, whether written or of the material culture). The importance of "philology", in this sense, is unquestionable, and we must all be good philologists before we are anything else. The only valid contention is that we should not stop there. Otherwise, if reconstruction of culture is based on a narrow philologism, the resulting picture is too fragmented and unsatisfactory. To refer in a negative sense to such a narrow visual angle the term "antiquarianism" is sometimes used (thus for instance in the article by Renger below), and may conveniently be retained in that meaning.

Philological, humanistic and anthropological approaches (to mention only those which I have been discussing here) are irreducible, in the sense that they cannot be derived one from the other. They serve different purposes, and hence they must all be pursued with equal energy in order to achieve a more complete picture and a deeper understanding of the cultures which form the object of our study. Clearly, it is not incumbent upon the individual scholar to encompass in his research all the various aspects of the field, to be in practice both a humanist and an anthropologist besides being a philologist. But while working in one direction, the scholar should keep the doors open to other lines of inquiry; one approach should not become exclusive of the others. This brings us back to the opening considerations: the progress of our field is largely coterminous with the increased degree of differentiation with which we come to analyze the data. Naturally, the scholar tends to probe deeper and deeper in the terrain with which he is most familiar, and from which he knows how to draw best results. Since every other scholar proceeds analogously with this exploration in depth, each in his own domain, differentiation could become a barrier. And instead, it should be made into a bridge leading more directly and securely toward a common goal: a better grasp of the whole of human experience in past cultures. What differentiation can teach us is that the refinement of our methods of study, for all the difficulties attending the enormous increase of technical know-how, is indeed effective in opening new perspectives and endowing the data, as it were, with added documentary value. The proper application of method can serve as a microscope which by multiplying the power of perception also multiplies, as it were, the power of the object to serve as evidence.

But the very notion of differentiation implies unity. The object of study retains its ultimate unity, and what is differentiated is our analysis. The levels of analysis must be kept rigorously distinct, and must retain their own autonomy, with the risk otherwise of confusing our results. Hence a linguist should address himself to the theoretical analysis of language whether or not he cares to, or is simply capable of, functioning at the same time as a polyglot; a social scientist, qua social scientist, must operate with his own procedures, whether or not he chooses personally to also relate to his material as a humanist; the

reconstruction of historical process should not be confused with mere philological chronicling, even though sound philological method must first be used to establish a solid documentary basis. The important rule is that one level of analysis not be mixed with the other. If such a distinction is clearly safeguarded, then, and only then, can a mutual comparison and integration of results be truly useful. In other words, two apparently opposite courses of action must be followed at the same time: we must differentiate our approaches, and yet we must combine them all-together. That most scholars will in fact incorporate the various aspects in their research is only natural. It is also reflected in the division by chapters in the present volume, where the distinctions we have been making are not carried through systematically. In the first three parts (divided according to the subject matter rather than according to method) there is generally greater emphasis on the anthropological approach, while in the last there are contributions of a prevailing philological nature. The humanistic interest surfaces at various points, though with a lesser degree of visibility.

But regardless of the labels we can pin on the various chapters, it is a fair conclusion that the present volume does in fact provide the forum for an attempt along the lines here indicated. The various authors have pursued their own specialization, but at the same time they have tried to explain the nature of their approach in terms accessible to those outside the specialization. That the volume should draw its unity not only from the stated purpose but also from its dedication to an individual scholar is meaningful. For in struggling, and how successfully, both toward a more specialized analysis and toward the command of an ever widening range of fields, Ignace J. Gelb has provided an inspiring example of a scholar and a teacher who has unified in his personal research a diversified spectrum of interests and approaches. And as such he has set a standard for us all to follow.

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J. L. HAYES, Bibliography of the Works of I. J. Gelb	1-8
G. BUCCELLATI, Methodological Concerns and the Progress of Ancient Near Eastern Studies	9-20

Part One: Writing and Palaeography

M. CIVIL, The Sumerian Writing System: Some Problems	21-34
E. REINER, New Cases of Morphophonemic Spellings	35-38
R. D. BIGGS, On Regional Cuneiform Handwritings in Third Millen- nium Mesopotamia	39-46

Part Two: Linguistics and Literature

J. B. CALLENDER, Grammatical Models in Egyptology	47-77
G. GRAGG, Linguistics, Method, and Extinct Languages: The Case of Sumerian	78-96
P. FRONZAROLI, Statistical Methods in the Study of Ancient Near Eastern Languages	97-113
J. KURYŁOWICZ, Verbal Aspect in Semitic	114-120
D. O. EDZARD, Die Modi beim älteren akkadischen Verbum	121-141
W. VON SODEN, Ein semitisches Wurzelwörterbuch: Probleme und Möglichkeiten	142-148
S. SEGERT and J. R. HALL, A Computer Program for Analysis of Words According to Their Meaning	149-157
E. SOLLBERGER, Problems of Translation	158-161
S. GEVIRTZ, On Canaanite Rhetoric: The Evidence of the Amarna Letters from Tyre	162-177
M. LIVERANI, Memorandum on the Approach to Historiographic Texts	178-194

(Continuatur in pagina III involucris)